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IN THIS ISSUE

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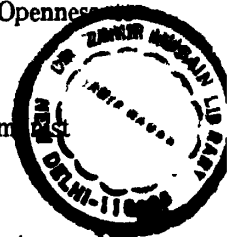
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IN THIS ISSUE

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TO OUR CONTRIBUTORS:

The *American Journal of Sociology* is not exempt from the cost-price squeeze that plagues scholarly publishing. Costs of editorial work and production steadily mount, while we wish to forestall for as long as possible further increases in subscription rates.

Regretfully, we must ask contributors of articles and research notes to bear a portion of the cost of manuscript processing. Effective March 1, 1977, each article or research note submitted to the *AJS* must be accompanied by a check or money order for $10.00, payable to The University of Chicago Press. Papers will not be reviewed until this fee has been paid.

The submission fee will be waived for student authors. Submissions from students should be accompanied by a faculty member's certification of their student status.

Errata

In "Communism, Conformity, Cohorts, and Categories" by James A. Davis (*AJS*, November 1975, pp. 491–513), the coefficient from NEW to TOLERANCE in figure 6 should be +.243, not +.116. Consequently: (*a*) the fourth line of numbers in table 12 (p. 508) should read +.243 1.6 2 >.50 47.3 3 <.001; (*b*) the fifth line of numbers in table 13 (p. 509) should read + *.419•.243* = *.1018* and the bracketed sum into which it leads is now .0976; (*c*) the total entry for table 13 should be .28115; and (*d*) the text directly below table 13 should read, ". . . (*a*) direct effects of changes in cohort composition, summing to +.10778. . . . These changes sum to +.28115, which differs from the. . . ." The error does not lead the author to modify any of his substantive conclusions.

For the following two books, Humanities Press (Atlantic Highlands, N.J.) should be credited as the American distributor: *The Transition from Feudalism to Capitalism*, edited by Rodney Hilton (reviewed in *AJS*, July 1977, pp. 213–15); *Power: A Radical View*, by Steven Lukes (reviewed in *AJS*, March 1977, pp. 1165–68).

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IN THIS ISSUE

ANNE FONER is associate professor of sociology at Rutgers University. She is coauthor of *Aging and Society*, vol. 1: *An Inventory of Research Findings* and vol. 3: *A Sociology of Age Stratification*, and editor of *Age in Society*. She has published articles on age and politics, the family, and age and class stratification. Her current fields of interest are social stratification, the sociology of age, and the sociology of sex roles.

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BARBARA F. RESKIN is assistant professor of sociology at Indiana University. She is currently completing a study of the ascriptive effects of academic sponsorship of scientists' careers. She is also studying sex differences in the relationship between labor force participation, occupation, and symptoms of psychological distress.

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Errata

In "Prestige among Peasants: A Multidimensional Analysis of Preference Data" by Mitchell A. Seligson (*AJS*, November 1977, pp. 632–52), the following errors appeared: (1) In footnote 1 (p. 632), "Coobs" should read "Coombs," and "Schwartz" should read "Schwarz"; (2) in footnote 2 (p. 633), line 6 should read "laborers *of* various," not "laborers *or* various"; (3) in the 7th line of the section called "The Data" (p. 635), the period after "(Seligson 1973)" should be deleted; and (4) in the 6th line from the bottom of footnote 3 (p. 636), the correct spelling is "*huasipunguero.*"

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CHARLES DERBER

IN THIS ISSUE

R. STEPHEN WARNER is assistant professor of sociology at the University of Illinois at Chicago Circle. The article in this issue stems from his studies of the theoretical functions of concepts of consensus in sociology. He is currently completing a field study of an evangelical movement in a mainline Protestant church.

TALCOTT PARSONS, professor emeritus at Harvard, has been working on volumes of his collected essays for publication by Free Press. The first one, *Social Systems and the Evolution of Action Theory,* will be followed in 1978 by *Action Theory and the Human Condition.*

WHITNEY POPE is currently gathering and analyzing comparative historical data with Nick Danigelis in order to test Durkheim's theory of suicide. With Charles Ragin and Christie Pope, he is investigating variation in lynching rates in the American South.

JERE COHEN, assistant professor of sociology at the University of Maryland, Baltimore County, received his Ph.D. in 1971 from the University of Chicago. His research interests span the areas of theory, adolescence, small groups, sociology of education, and modernity.

ERIK OLIN WRIGHT is assistant professor of sociology at the University of Wisconsin—Madison, and a member of the Center for Research on Politics and Society and the Institute for Research on Poverty. He is the author of *Class, Crisis and the State* (London: New Left Books) and is currently planning a large-scale research project on the class structures of advanced capitalist societies.

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MARK GRANOVETTER is associate professor of sociology at the State University of New York at Stony Brook. His current projects include a monograph on threshold models of collective behavior and a theory of income differences.

NOAH E. FRIEDKIN is a research associate of the Educational Finance and Productivity Center at the University of Chicago. He is engaged in a longitudinal study of interactions between local community social structure and school district decision making. The author's research on university social structure and social networks among scientists is proceeding with a survey of approximately 800 biological, physical, and social scientists at two universities.

GARY G. HAMILTON is assistant professor of sociology at the University of California, Davis. His current research includes a study of commercial organization in prerevolutionary China and a comparative historical investigation of the use of personal staffs as an instrument of rulership.

DAVID E. LOPEZ is assistant professor of sociology at UCLA. He is currently studying racial appearance and bilingualism as they relate to social mobility.

GEORGES SABAGH is professor of sociology at UCLA. His primary interest is the social demography of differential fertility and migration in the United States and the Middle East.

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Structural Differentiation, Efficiency, and Power[1]

Dietrich Rueschemeyer
Brown University

The theory of structural differentiation, long a major conceptual tool of the study of social change, is critically reviewed. The link between differentiation and efficiency advantages involves a number of unsolved problems. In addition, past differentiation theory paid too little attention to actual processes of differentiation as well as de-differentiation. Both sets of problems can be tackled better if power constellations and power interests are systematically introduced into the analysis of differentiation and de-differentiation.

Division of labor, specialization, the structural differentiation of functions—these concepts have served as conceptual tools for the study of social change since the very beginnings of modern social theory. In spite of this long history, our understanding of structural differentiation is rather poor, so poor in fact, that a *theory* of structural differentiation is still more a goal than an accomplished reality. What is presented as the theory of differentiation consists largely of either concept formulations or descriptive generalizations about tendencies toward greater social complexity in long-term historical developments.

In particular, past and present theories say little about the causes of structural differentiation. More attention has been paid to its consequences, especially after Marx joined the debate, although his concern with the relation between the division of labor and changes in the class structure has since receded to the background. Instead, Durkheimian interests focusing on problems of moral integration and anomie assumed a central role in recent theoretical work on structural differentiation (see Eisenstadt 1964; Smelser 1963; Parsons 1961, 1966). Significantly, the most important causal arguments are of the functional type, that is, an analysis of the consequences of differentiation serves as the basis for an indirect causal explanation. This model plainly requires a careful study of "feed-back mechanisms" or "causal loops," which link the effects of differentiation to its causal conditions, a requirement that often has been either neglected altogether or fulfilled by global assumptions only. In this essay I shall identify some of the critical problems of the prevailing functionalist approaches, which are built on asserted links between differentiation and efficiency, and argue that a causal analysis of actual

[1] I wish to thank Mark Elchardus, Josef Gugler, George C. Homans, and Martin U. Martel for critical comments and suggestions.

processes of differentiation can be advanced significantly by focusing on the role of power.[2]

I

"Structural differentiation is a process whereby one social role or organization . . . differentiates into two or more roles or organizations. . . . The new social units are structurally distinct from each other, but taken together are functionally equivalent to the original unit." Smelser's definition (1959, p. 2; 1963, p. 34), shared with certain variations by most recent theoretical analyses, establishes a concept significantly more inclusive than the older "division of labor": It does not limit itself to the economic sphere, and it deals with more complex structural elements than roles—with organizations and on occasion with even broader and less tangible phenomena such as institutions, values, and cultural complexes. While there is considerable merit in this broader concept, which derives from Spencer, it requires further theoretical distinctions. For instance, it is necessary to recognize that differentiation at the level of roles can go hand in hand with an agglomeration of functions at the level of organizations or broader institutional complexes, and vice versa.

For Smelser, and to an even greater extent for Parsons (1961, 1966, 1971) the "structural differentiation of functions" is part of an elaborate and complex conceptual framework which relates different structural forms and the functions they serve to a catalog of structural components and functional problems inherent in a postulated model of society conceived as a system with different levels and kinds of subsystems. This framework helps to solve important analytic problems. For instance, it aids in identifying and distinguishing different functions and in assessing which seemingly diverse efforts are in fact serving the same function—are "functionally equivalent." In this framework any particular function is analytically specified by its relation to an inclusive system or to one of several subsystems, each with its special set of functional problems.

A major shortcoming I see in this very ambitious attempt at theoretical conceptualization is that the transition from abstract analytic definitions to concrete structures and processes, often empirically identified in terms of prevailing social rather than theoretic sociological definitions, remains a difficult and uncertain undertaking, in which common sense and even

[2] There is no space here to trace the theoretical antecedents of relating the division of labor and organization to power structures and power interests. Important leads, though of a quite different nature, are found in the works of Karl Marx, Ludwig Gumplowicz, and Max Weber. My own thought is indebted to Lenski's comparative analysis of power and stratification (Lenski 1966) and, more generally, to the work of Talcott Parsons.

arbitrary decision too often have to complete the "translation." For this and other reasons, I prefer in this essay to work with a much simpler and less demanding theoretical scaffolding. Rather than ground every assessment of functions in theoretically postulated functional needs of "society" and its subsystems, I shall speak of *problems* identified and defined by the needs, interests, and expectations of different constituencies, which may or may not overlap in their composition and agree in their determination of the problems. These problems are dealt with by roles, organizations, and organizational complexes in more or less differentiated or aggregated fashion. Although this strategy leaves unsolved important problems of comparability of different concrete structures and "functional" problems, it does allow us to tackle a number of crucial theoretical issues without the encumbrances of a comprehensive analytic model of society. When faced with certain questions of determining and classifying problems and structures, we may still borrow from the more comprehensive analytic frameworks developed by Parsons and others.

In an earlier publication (Rueschemeyer 1974), I reviewed critically the current state of the theoretical analysis of structural differentiation. Here I shall discuss only one major tenet of past and present theories, the alleged link between differentiation and efficiency advantages; reviewing the shortcomings of what might be called the efficiency theory of differentiation will serve as an introduction to the arguments about the role of power in processes of differentiation and de-differentiation.

II

Since Adam Smith, it is above all the increase in efficiency attributed to specialization which has served as an indirect causal explanation of structural differentiation of social roles and organizations. The connection between structural differentiation and efficiency is not a simple matter. Its major aspects can be stated in several propositions: (1) Separating organizationally instrumental tasks and interests from other human concerns makes it possible to arrange things more single-mindedly for a rational pursuit of chosen ends. This is of fundamental importance because roles, relationships, and organizations best suited for "getting things done" are by no means optimal for other universal and, at least in the long run, equally urgent human interests—for enjoying the fruits of one's labor for instance or for getting along with one another, for bringing up children, and perhaps for coming to terms with fundamental problems of meaning and motivation. (2) Specialized executive roles are an important means for reaping the benefits of further specialization of instrumental roles to be indicated below. Such roles integrate the differentiated

parts and are the major locus of the social control of performance and of organizational planning. (3) Delimiting tasks narrowly and clearly facilitates the evaluation of performance. This in turn allows rewards and punishments to be tied more closely to the work done and provides a better basis for prediction of performance, thus aiding in more rational hiring and allocation of personnel. (4) Separating simple from complex tasks permits economies in training and recruitment. The efficient use of the untrained and the less gifted may be as important as or even more important than the opening of greater opportunities for the able.[3] (5) Dividing instrumental tasks further and further makes concentration on a few activities possible—a form of differentiation with obvious, but easily overrated, efficiency advantages. (6) Once the principle of a division of labor is fully established, it becomes possible to conceive of roles and organizations as building blocks to be used in rational organizational architecture. This makes organizations more adaptable to a changing environment and to technological innovations.

While these propositions are plausible, they should not, as is often done, be regarded either as established or as a complete analysis of the problem. First, these propositions must be tested systematically with the goal of identifying specific conditions particularly favorable *or unfavorable* to the different efficiency advantages. Second, a more complete analysis would take into account competing hypotheses about certain "costs" of differentiation which may arise out of the need for integrating more differentiated units, for instance, or from the problems of morale and motivation induced by high levels of specialization. The balance of these costs and gains in regard to efficiency may be positive or negative, depending on different environmental, technological, and sociocultural conditions. A crucial third set of questions would ask how different outcomes in terms of efficiency in turn affect the conditions furthering or inhibiting differentiation. Rational planning on the basis of past experience, differential survival of various competing social arrangements, and diffusion, which may or may not be based on rational understanding, are the major links between the outcomes and the conditions of differentiation implied in recent theoretical formulations; but the circumstances under which these "feed-back

[3] W. J. Goode (1967, p. 17) has argued that "the modern system is more productive because its social structures *utilize the inept more efficiently*, rather than because it gives greater opportunity to the more able." The interrelations between different aspects of differentiation and its consequences are more complex than this assertion of Goode indicates. The level of competence and incompetence in various strata is in part a function of previous developments of differentiation. The thrust toward a division of tasks indicated in the two previous and in the following points is likely to debase and to limit the competence of many individuals. The "production of the inept" is as important a topic as their subsequent, and often quite limited, protection and efficient use. I wish to thank Mark Elchardus for his insistence on this point.

mechanisms" work and the ways in which they influence each other are not at all well understood.

There is, however, a more fundamental problem that plagues all arguments about increased efficiency or "adaptive capacity" in the operations of collectivities. Any judgment about efficiency hinges on a given set of ranked goals and on a given evaluation of alternative means to reach these goals. What is efficient in terms of one preference structure may be wasteful by other criteria. Not only different cultures, but lord and serf, entrepreneur and worker, executive and employee, as well as many other social categories, differ fundamentally in their evaluations of the price paid and the advantage gained with a new arrangement of their social relations; and since preference structures and cost-benefit calculi vary, what efficiency means is always and inevitably determined by varied interests and value commitments.

Once this formal character of the concept is recognized, efficiency cannot serve the central role in a functional explanation of structural differentiation it has been assigned in the past. The indeterminacy of an abstract cost-benefit calculus is much too complex an issue to allow here even a brief review of the various attempts to come to a theoretical solution. I will content myself with the simple assertion that neither the discussions of welfare economics (see Arrow 1951; Mishan 1968) nor the strategy in social theory, argued by Talcott Parsons since the *Structure of Social Action* (Parsons 1937), of looking for shared goals and standards of evaluation in systems involving a plurality of actors, has succeeded in resolving the basic problem. Neither approach has led to solutions suitable to dispose of the indicated critique of an explanation of structural differentiation in terms of increases in the efficiency or "adaptive capacity" of whole economies and societies.[4]

These difficulties can be reduced considerably, though not eliminated in principle, if it is possible to identify in a given social context structural positions with disproportionate power, for power is by definition a chance

[4] It may be noted in passing that the implications of the critical argument advanced go far beyond the theoretical issues under discussion. Another conclusion would be, for instance, that the use of data on the gross national product (GNP), standardized in relation to population size or other economic resources, is a highly problematic means for comparing the efficiency of different economies, developed or underdeveloped, capitalist or socialist. The prices used in these aggregate calculations are the result (1) of complex need patterns backed up by differential income, (2) of diverse market conditions which are based on different institutional arrangements and on the differential power of various economic groups, and (3) of political and administrative decisions which play a different role in different economies. This is not even to mention that many aspects of human welfare do not enter the economic price calculus at all and that the borderline between conditions that are and those that are not expressed as priced costs and as incomes varies considerably from economy to economy.

greater than others' to realize one's goals even against resistance,[5] and a large share of power concentrated in the hands of individuals and solidary groups with similar preference structures therefore implies that a certain type of cost-benefit calculus gains a disproportionate influence on what happens in the situation. The interests and reactions of the most powerful are thus a point of great leverage for any analysis of this kind. They are in fact treated in such a manner in many studies, but the strategy typically remains implicit and does not receive adequate theoretical recognition. For instance, in his analysis of the industrial revolution in the British cotton industry, Smelser (1959) makes "dissatisfaction" the first of several phases in a development of structural differentiation without specifying theoretically whose dissatisfaction is relevant. In the empirical application of the theoretical framework this turns out to be the dissatisfaction of the early entrepreneurs and shop owners. Eisenstadt comes closest to the position advocated here. In his comparative analysis of premodern bureaucratization (1963) he focuses on the interests of the ruler, of the aristocracy, and of urban groups, which gain importance and power with the advance of market exchange and bureaucratic rule.

The structure of theoretical reasoning may very well—though it need not—remain functionalist in character, taking off from efficiency gains and other consequences of differentiation and then seeking to identify links between these consequences and conditions favorable or unfavorable for processes of differentiation. The points of reference of the analysis, however, are strategically altered by focusing on the powerful. The preference structures relative to which the consequences of differentiation can be analyzed become thereby more amenable to analysis. The unmanageable complexity of an endless variety of preference structures is reduced to a few patterns of interest and valuation. Furthermore, the behavior and even the attitudes and sentiments of the more powerful are better documented in the historical record than the behavior and attitudes of common people, an obvious advantage for any investigation of long-term social change, which almost by necessity must make use of historical sources.

Of greater theoretic interest is the fact that it appears possible to predict some of the interests pursued by the powerful; this endeavor would increase the explanatory power of the analysis considerably. It seems reasonable to assume, though this need not remain an untested assumption, that those in positions of power will seek to maintain their advantage and under certain conditions even attempt to increase it. It has been argued against this proposition that the assumed tendency depends on the total

[5] For the present purpose, it seems reasonable to use this general definition of power, which goes back to Max Weber ([1921] 1968, p. 53). Later his more specific concept of "domination" or "authority," defined by the probability that a command will be obeyed, will be used primarily.

balance of benefits and costs experienced by incumbents of power positions as compared to their life chances when out of power and that this balance varies with different circumstances in ways not easily predictable.[6] No doubt the proposition can and must be further refined and specified as to varying conditions. Yet as a rough generalization it seems established that differential power tends to be associated with differential advantages of other kinds. In addition, those in power will often think and act in terms of positional—in contrast to, and in addition to, personal—interests, strengthening a concern with maintaining their power resources. Finally, for all analyses involving large numbers of individuals and groups in positions of power, one can advance the statistical argument that the proportion of those who seek to maintain and extend their advantage will be increased by virtue of the fact that they have a better chance of remaining in power positions than those who make no such efforts. This argument merely presupposes that power can vanish if not taken care of and that attempts to husband and extend one's power resources have some effects in the intended direction. Clearly the powerful will pursue other interests as well, and a concern with maintaining their power resources is inevitably only one of several interests. However, to identify even one substantive interest that is relatively stable and relevant for policy and large-scale change is an important theoretical gain.

The study of the "causal loops" linking the consequences to the conditions of differentiation is also simplified by the approach suggested. Not only are the intentional reactions to perceived consequences stronger if one focuses on the powerful; one can also exclude a number of effects from the analysis because in many instances the powerful will not suffer—and thus count as "costs"—the consequences of their policies. Increased monotony of work or heightened job insecurity, for instance, have been of little concern to entrepreneurs unless worker morale or the politics of labor relations seemed affected. Finally, one can at least speculate that there is a correlation between holding positions of power and tending toward rational action, that is, action based on a review of goals and means in the light of one's basic preferences and the best information available about the consequences of alternative courses of action. Rational action in this sense represents an important causal loop which links possible and actual consequences of differentiation to the condition of its development.

Focusing on phenomena of power in the study of structural differentiation, then, leads out of the impasse that results for the prevailing functional approach from the formal character of the concept of efficiency, it simplifies the empirical investigations required, and it increases the explanatory

[6] George C. Homans raised this objection when an earlier version of this paper was presented to a colloquium at Harvard University in 1974. I am grateful for having been pressed to clarify my underlying arguments.

power of the theoretical analysis. This strategy also holds great promise for a better understanding of differentiation on other grounds. The distribution of power resources and the substance of power interests are, I submit, a major causal determinant of the actual social changes involved in differentiation. In the past, the role of differentiation in broad patterns of general evolution often received the greatest attention.[7] This has muted questions concerning the immediate causal circumstances and the actual processes leading to various types of differentiation. Furthermore, concentrating on the assumed "fact that the division of labor advances regularly in history," theorists have often failed to study the obstacles which must be overcome in processes of differentiation. A comprehensive analysis of both favorable conditions and obstacles plainly is required for a better understanding of how differentiation comes about. In this, considerations of power should have a central place. A related problem area neglected in past research is the analysis of instances of "de-differentiation," of processes in which previously separate roles or organizations are fused to deal with a broader set of problems. Investigation of this area, too, has been hindered because theoretical inquiry has concentrated on the broad pattern of advancing differentiation in western history. Studying both differentiation *and* de-differentiation within the same theoretical framework seems a promising strategy for understanding better the immediate causes and processes involved in either development, and it is my thesis that power interests and power resources play an important role in both.

Changes in the social division of labor have often been regarded as major determinants of other changes in the social structure, including the structure of power. In fact, the division of labor and, more broadly, the "relations of production" have been viewed as *the* aspect of social structure through which environmental factors, technological developments, and population change result in changes in other features of the social organization of human life. The analytic strategy proposed here does not intend to reverse this model and assert that social power is the master variable controlling all social change or all aspects of structural differentiation. That would mistakenly equate social power with social causation, and it would negate the interconnections between structures of power and cultural, social-economic, and environmental factors. What I do assert,

[7] To cite the words of Durkheim ([1893] 1964, p. 233), as one example among many, "What causes have brought about the progress of the division of labor? To be sure, this cannot be a question of finding a unique formula which takes into account all the possible modalities of the division of labor. Such a formula does not exist. Each particular circumstance depends upon particular causes that can only be determined by special examination. If one takes away the various forms the division of labor assumes according to conditions of time and place, there remains the fact that it advances regularly in history. This fact certainly depends upon equally constant causes which we are going to seek."

however, is that the distribution of power resources and the interests and actions of the powerful, though themselves dependent in the long run on other circumstances, are among the major factors determining how a given pattern of social organization responds to changes beyond its immediate institutional control. Power variables may have long-term effects of their own, too, provided that there is sufficient continuity in the structures of power and the associated policy propensities. I have suggested elsewhere (Rueschemeyer 1974) that the interests of the powerful in maintaining their position seem to be sufficiently similar in different societies and periods to result in a stable core of interests despite elite turnover and structural change. In analogy to the notion of a *raison d'état* one might speak of a *raison d'élites* to designate this similarity in all but the least complex societies.

In the remainder of this essay I shall seek to demonstrate the utility of considering power variables in the analysis of differentiation and de-differentiation. First I shall discuss how power interests affect processes of differentiation within a unified sphere of organized domination. The next section will consider interactions of relatively independent centers of power and their impact on the division of labor and organization, while a third section is concerned with de-differentiation as a function of power structures and interests. I shall concentrate throughout on selected instances and themes that appear particularly suited to elaborate and specify the general argument.

III

Structural differentiation pursued by power holders in their own sphere of authority represents the simplest form of an interaction between structures of power and processes of differentiation. It comes closest to the model based on efficiency because the interests of the ruler suffer little interference and thus provide a relatively unambiguous and stable point of reference relative to which effects of different policies and developments can be assessed. In fact, the counterpart of the political ruler in the economic sphere, the capitalist entrepreneur, provided the model for what was generalized in economic and social theory as the efficiency perspective on differentiation.

There is good reason for the hypothesis that some form of organized domination is, in evolutionary terms, a necessary condition for developments that go beyond the most elementary division of labor built on differences of age and sex and on the organization of family and kinship.[8] Aside

[8] It is worth noting that two theorists as different as Gerhard Lenski and Talcott Parsons assign a similar importance to the emergence of organized domination for the evolution of a more complex division of labor (see Lenski 1966 and Parsons 1964).

from the efficiency advantages of specialized executive roles noted earlier, the crystallization of positions of social authority separate from kin and family roles establishes certain preference structures as stable standards for judgment and action, and it creates an instrument for the accumulation of capital as well as the aggregation of demand—two important requisites for all roles that serve specialized goals and do not immediately secure their incumbents' livelihood.

One incentive for rulers[9] to move toward a division of labor and organization in their own household and staff derives from the gains in efficiency this may bring about. Another set of considerations involves the advantages and disadvantages differentiation has for maintaining and expanding the power of those in charge.[10] These latter effects of differentiation are specially relevant for political rulers, but they are of some importance for incumbents of any position of hierarchical leadership and control. Both considerations may lead to the same conclusion as, for instance, in the case of closer evaluation of performance and stricter work discipline as a result of specialization. Of more theoretical importance are those cases in which the conclusions do not coincide. Such a divergence has two primary sources. First, division of labor and organization often leads to problems of supervision and control of the more specialized staff and, second, its direct and indirect effects may erode the bases of authority of the ruler in the wider society. Each problem will be discussed in turn.

Beyond some point of differentiation, increases in the number of specialized roles (as well as the often-correlated increases in the number of people and the scope of operations involved) require a division and delegation of authority because one person, family, or clan is no longer able to cope with the problems of supervision, discipline, and direction. Delegation of authority, however, creates problems for continued central control. An increased number of specialized roles often entails the subdivision of organizations; this in turn is likely to aggravate control problems and jeopardize the power of the ruler. Moreover, higher levels of organizational and technological complexity require specialized skills and

[9] The following discussion will deal primarily with the political realm. On occasion it will turn to the economic realm in order to pursue important similarities and analogies. Thus, the term "ruler" is used primarily in the narrow political sense, but is on occasion broadened to mean any incumbent of a position of organized domination.

[10] One can never assume that complex objective cause-effect relations are fully understood and foreseen by the actors. However, important as the exact role of knowledge and ignorance may be as a subject for research, here we will neglect the resulting variability and postulate that a combination of factors will make for a rough correspondence between the interests of rulers and prevailing policies. Among these factors are: (1) some foresight about alternative courses of action, intuitive as well as rational; (2) a knowledge and partial imitation of the policies of successful others; and (3) the elimination or reduction in number and importance of those least successful in the choice of their policies, provided the analysis covers large numbers and a sufficiently long period of time.

knowledge in certain positions, and this development, which is more important as more knowledge is used in actual operations, has similar consequences. Those in positions of power become dependent on specialists who are much harder to control than those whose work is open to commonsense evaluation.

Broadly, one can distinguish three outcomes of this dilemma. First, the rulers may shy away from a further division of labor and organization because of the problems indicated, or they may fail in their attempts to cope with them and lose much of their power. Since their power was the aggregating factor on the basis of which further differentiation became possible, the latter alternative would most likely also arrest the process of differentiation. The repeated centrifugal tendencies of European feudalism may serve as a well-known example of such an arrest of structural differentiation. Any systematic investigation of the history of business firms, especially in the early phases of modern economies, would probably turn up many illustrations of an unwillingness to engage in a division of authority.[11]

Second, the ruler may be able to cope with the problems of control by a variety of complex additional arrangements of supervision and discipline. If we take the government of 18th-century Prussia as a case in point, these measures include the establishment of collegial authority, which diffused responsibility but allowed for multiple lines of information to the center and for mutual control, the development of a bureaucracy paralleling others with a primary task of spying, inspection, and control, and the recruitment of certain officers with an eye as much to loyalty as to ability and expertise. As quite costly arrangements, these measures can weigh heavily on the negative side of an efficiency analysis, but

[11] One component of the famous argument that French family firms contributed to a retardation of the French economy (see Landes 1951; for a skeptical view, see Kindelberger 1963) revolves around the desire for family control. The controversy surrounding this thesis concerns the long-term stability of behavior patterns of family firms and their effects on economic retardation; less in question is the fact that for a long period problems of control restrained growth and division of authority. Less well known are similar findings about West African entrepreneurs (Garlick 1971, p. 139; Kilby 1965, pp. 85 f., 101, 111, can be interpreted along the same lines). To add one more example to the list, an unwillingness to risk changes in traditional relations and in the structure of power, which outweighs fairly obvious advantages for productivity, seems the key to an explanation of the economic inefficiencies of Latin American latifundia run by absentee landlords and administrators of very limited competence (Feder 1971, p. 87 f.; see also Barraclough 1973). A variant that leads into the next category of outcomes of the control dilemma is provided by the cases in which subordinates prove able to cope with such control problems and by doing so gain a position of power of such character that, in the extreme, they replace the original ruler. Of greater consequence than such simple displacement are developments in which a corps of top subordinates gains power in a similar way at the expense of the ruler without displacing him. For an excellent case study, see Rosenberg (1958). Such a development can in several ways overlap the next two outcomes discussed.

they did answer problems of central direction and control created by increasing division of labor and organization (see Smith 1972).

Third, the corps of relevant officers may develop attitudes and value commitments consonant with the interests and responsive to the wishes of those in top positions. Normally this requires cultural developments which over a long period of time remain consistent with the interests of rule by divided authority and which forge tenets of legitimation acceptable to both rulers and officials. These cultural developments have to be complemented by generous rewards of income, status, or both. According to Max Weber's well-known analysis, access to these rewards must remain under the control of the center lest the subordinates create their own independent power centers.

The first two outcomes of the control dilemma created by the division of authority and the increased use of experts indicate important obstacles in the process of differentiation as well as some ways in which these may be overcome by rulers who command sufficient resources. The third possibility identifies a crucial *breakthrough* in the emergence of a modern social order. The early developments of this kind involved secular changes; both in time and in social scope, they reached far beyond the horizon of even the most powerful and far-sighted actors. Subsequent developments, taking place within a transformed sociocultural environment, can rely on already existing patterns of attitude and value commitment which may ease, and in the extreme even eliminate, the dilemmas engendered by a division of authority.

To understand more fully the conditions which bring about such fundamental transformations, much more research and analysis is required—research of the historical-sociological variety exemplified by the work of Weber and Eisenstadt—rather than general formulations about evolutionary stages which gloss over the actual processes of change. There is, incidentally, reason to think that transformations of the ethos of administrative officials tend to be much more limited in their effects than some formulations about the modernization of public and private administration suggest. The formal readiness to serve loyally in a system of divided authority seems in most instances bound up with some substantive ideals, aversions, and taboos, which are deeply grounded in the outlook of officials and which limit the range of policies given unreserved support in a specific service subculture. It is for this reason that we find complex control arrangements, similar to those of the 18th-century Prussian state, in such modern societies as Germany under the National Socialist regime (Burin 1952) and the socialist states of Central and Eastern Europe after the Second World War (Fainsod 1963; Beck 1963). The conflict between the habits, outlook, and value commitments of the established corps of

civil servants and the goals of the new political elites gave rise to the underlying problems of control and direction.

Obstacles to the advance of specialization and differentiation within the administration of rules do not derive only from contradictory interests of the power holders. Resistance against differentiation is likely to come from officials and organizations who have some share of power and whose jurisdiction would be subdivided. Their resistance will intensify as further differentiation infringes on their economic privileges and on their status in society. Their resistance will be reinforced by commitments to certain policies and values for which they claim a special responsibility. Similarly, a division of roles and organizations will be favored by those who thereby gain in privilege, status, or power, which they may or may not exercise with claims of a wider responsibility. The ultimate result of such contradictions between vested interests will depend largely on the power resources of the opposing factions and on the interests and the power resources of higher-level decision makers. This suggests that specialization in an organization, once those in authority decide it is to their advantage, is likely to proceed more smoothly the lower the rank of those affected, because opposition is less likely to be powerful.

Another conclusion of some importance is that where opposition to differentiation from vested interests in established organizations is considerable, more complex organizational forms may develop more easily in new organizations than through the transformation of existing ones. Later the old organizational forms may be gradually eliminated, be it by decree or as a result of competitive pressure. However, they also may succeed in surviving under certain and not necessarily rare conditions; this results in the not unfamiliar picture of a coexistence of several different organizational forms with the character of each testifying to the conditions that shaped its development (see, for instance, Stinchcombe 1965). Either outcome is incompatible with naive conceptions of differentiation according to which more complex social patterns evolve directly and smoothly out of simpler ones.

We now turn to the second problem area identified above, which concerns the adverse effects that differentiation potentially has on the authority of rulers outside their immediate organizations of domination.

S. N. Eisenstadt, who—following Max Weber—has done much to advance the analysis of the problems discussed so far, has also provided a succinct formulation of the central issues involved in the potential erosion of the ruler's authority in the wider society due to advances of structural differentiation (Eisenstadt 1963). Since differentiation requires a freeing of resources and people from uses and role assignments fixated by tradition, and since many specialized positions gain in the process of dif-

ferentiation some autonomy from direct control, the specialization of urban groups and the related expansion of market exchange tend to undermine ascriptive, "traditional" authority. At the same time, such developments are necessary to advance the more efficient forms of bureaucratic rule. A ruler whose power in the wider society rests to an important extent on ascriptive fixations and traditional consent thus finds himself in a dilemma if he seeks to expand his power through bureaucratic centralism. How this dilemma is resolved depends to a large extent on coalitions and compromises with old and new subdominant groups. There are in particular the old aristocracy and urban strata of new importance; their interests fall primarily on opposite sides of the dilemma and their support can be used, respectively, to compensate for erosions of old foundations of the ruler's power or to develop new bases of power and legitimation more consonant with bureaucratic rule.[12]

Eisenstadt's analysis deals with early forms of bureaucratization. While certain features of these developments have universal significance, it is important to keep in mind that the problems encountered here are transitory ones in a longer term perspective. Not only are the principles of authority and legitimation less heterogeneous and contradictory in more modernized societies, but also the modern institutional structure offers—with general legal norms, enacted law and formalized adjudication, and with such legal forms as contract, property, and corporation—a firmly established and widely accepted framework in which individuals and collectivities can gain and lose power, or set up organizational forms and recompose them, without having to secure anew in each case the necessary foundation of power and justification in the wider society. Here, as in the complementary case of the development of a modern service ethos among officials, what are needed are historical-sociological process analyses which clarify how a sociocultural environment that is favorable to such building and restructuring of organizations has developed under varying conditions. The present state of knowledge suggests that the con-

[12] This line of argument focuses on problems of legitimation. Another problem derives from the interdependence engendered by increased differentiation. If such interdependencies extend beyond a sphere of power, important resources may come under partial control of actual or potential opponents. Whether under these conditions those in charge will opt for self-sufficiency, and thus a limitation on certain advances of differentiation, depends—aside from problems of foresight and other factors—on their assessment of the likelihood of conflict and on the amount of control a likely enemy or coalition of enemies is seen as gaining. A closely related problem concerns the cultural and ideological exchanges associated with structural differentiation. The interdependencies created by differentiation often become also channels for the flow of ideas. To the extent that power holders see their power endangered by such diffusion or that they value the maintenance of certain cultural orientations as an end in itself, they may seek to limit differentiation in order to limit the reception of foreign cultural elements. The radical isolationist policies of Tokugawa Japan and the more complex but basically similar policies of socialist societies in Eastern Europe may serve as examples.

centration of power that was a condition and a further consequence of the bureaucratization of political rule was of paramount importance in initiating and establishing these more pervasive changes (Eisenstadt 1963; Parsons 1964).

IV

Karl Marx distinguished between the "division of labor in manufacture," or more generally differentiation within organizational complexes under a single authority, and "division of labor in society," or specialization of autonomous social units relative to each other.[13] This distinction has important implications concerning the type of change involved. Differentiation within the jurisdiction of a single authority more often takes the form of directed rather than unplanned, "crescive" change and is probably more amenable to a conclusive and determinate analysis than differentiation that grows out of the interaction between autonomous units.

One might be tempted to extend this insight and argue that, while power variables may play a significant role in the "division of labor in manufacture" and its noneconomic equivalents, the "division of labor in society" is subject to different forces, forces that transcend the resources and interests of the particular authorities and privileged strata of a society. I shall argue that this view is mistaken and derives from conceptions of economic competition as a natural state relative to which political regulation and economic policy are artificial, unjust, and ultimately ineffective. Aside from this fundamental criticism it is worthwhile to note here that the "division of labor in society" and, more generally, the specialization of autonomous social units relative to each other cover a much wider range of phenomena than competition among a large number of equals. There may be many units involved, or as few as two, and the distribution of power—among the units potentially involved in differentiation as well as among social positions and organizations in general—can vary from a very strong concentration to a near-equal dispersion. Clearly the direct role of power variables in processes of differentiation is likely to be greater if only a few units are involved and if the concentration of power reaches a high level.

[13] Marx's argument insisting on this distinction is worth recalling: "The same bourgeois mind which praises division of labor in the workshop, lifelong annexation of the laborer to a partial operation, and his complete subjection to capital, as being an organization of labor that increases its productiveness—that same bourgeois mind denounces with equal vigor every conscious attempt to socially control and regulate the process of production, as an inroad upon such sacred things as the rights of property, freedom and unrestricted play for the bent of the individual capitalist. It is very characteristic that the enthusiastic apologists of the factory system have nothing more damning to urge against a general organization of the labor of society, than that it would turn all society into one immense factory" (Marx [1867] 1959, p. 356).

Perhaps the most dramatic case of differentiation due to power struggles between relatively autonomous interests is the "separation of powers" in modern government. At least in its democratic variant, this did not come about because of a drive toward greater efficiency of administration, adjudication, and legislation, but rather because of attempts to limit the power of the crown. The balance of power between crown and other groups was also at stake in instances where a "separation of powers" was imposed from above. Thus, when the Prussian kings took administrative tasks away from the *Regierungen*, organizational strongholds of the Junkers, and narrowed their function to judicial tasks, they moved in the direction of a structural differentiation of governmental functions and at the same time gained important power advantages over their conservative opponents (see Rosenberg 1958, esp. p. 55; Wagner 1936).

One may wonder why similar developments should come about as the result of very different power interests. Is it possible that the *lines* of differentiation—delimiting which functional problems are structurally separated from others—follow more universal exigencies, while power interests determine only whether or not a given process of differentiation takes place? I doubt that such a distinction is very useful. For the developments under discussion one might argue that they constitute a differentiation of decision-making processes with the rationale of separating concerns the vigorous pursuit of which may interfere with each other—an application of the general principle of which the legal treatment of "conflict of interest" is also a special case. The argument is reasonable enough; in fact, it formulates a point that perhaps should be included in the list of potential efficiency advantages developed earlier. Yet, as with all efficiency advantages, one must ask here too, "efficient" in terms of whose interests? The absolute king, his top administrators, civil servants in the offices affected, and citizens in various relations to the royal adminstration had most likely quite different ideas about the incompatibility of various tasks of decision making, and these differences were rooted in divergent interests. It would be foolish to rule out the possibility that different interests may on occasion lead to similar, partially identical results, but the controlling proposition remains that this is not to be taken for granted. To return to the Prussian example, one may want to remember that in another aspect of the separation of powers, the separation of legislative powers from administration, the crown was extremely reluctant to make concessions, and a full separation came about only in 1919 after the demise of the monarchy.

The "separation of powers" in government does not stand alone; it is an instance of a more general phenomenon. The delineation of the "jurisdiction" of large institutional complexes inevitably raises questions of power, especially if control over crucial resources is at stake and if the organizations and institutions in question command general influence and

moral authority. In modernizing societies, struggles about the role of church, working-class organizations, or universities provide cases in point. In these matters of organizational and institutional differentiation there does not seem to exist any simple trend toward narrower specialization. In fact the later discussion of de-differentiation will concentrate on similar and related problems.

Political struggles about the jurisdiction and tasks of such institutions as church and universities involve centers of power and influence, which are different from each other in functional character. This suggests the complementary question, which indicates a concern that was central to the classic discussions of the division of labor: What are the effects on differentiation of conflicts between functionally similar units, for instance, between two or more political sovereignties or between business firms? One outcome is that a party loses its autonomy and becomes a part of the winner's organization, possibly with a more specialized task. Changes in the role of the aristocracy and its institutional strongholds at times when strong rulers succeeded in imposing bureaucratic centralism provide a variety of examples for such imposed specialization; it is also a common result of competition among business firms, especially under oligopolistic and quasi-monopolistic market conditions. Another outcome, of special importance for the economy, is organizational specialization in order to avoid direct confrontation and possible defeat; product specialization reduces the intensity of competition. Both outcomes played an important role in Durkheim's analysis of differentiation (Durkheim [1893] 1964).

While such fights are under way, and especially if they are waged between organizations of comparable strength and therefore extend over long periods of time, competitive pressures increase tendencies to look for the most efficient forms of internal division of labor, most efficient in terms of gaining a competitive advantage or at least holding one's own in the market. These effects on the internal division of labor of organizations would presumably be stronger as competition approached more closely that construct of the economic imagination, "perfect competition."

Competition that approaches the pure type of perfect competition differs from the forms of political and economic struggle discussed so far, because the intent to reduce the power of competitors or to displace them altogether is minimized. The number of competitors is too large for such attempts to be worthwhile. The model of perfect competition is of special importance for the central problem of this essay, the interaction between structures of power and processes of differentiation, because it was long advanced with the claim that here was a combination of order and progress that could function without help from the powerful.[14] An ideo-

[14] It may be noted in passing that the claims made for the efficiency effects of competition pose a challenge for the conventional efficiency theory of differentiation, since by

logical descendant of the tradition of natural law, this classic conception of competition as the "natural" state of affairs, relative to which intervention and planning by the state and by monopolistic power holders are "artificial," still has some currency.

Yet on reflection it is abundantly clear that various elements of power play an important role even in situations approaching perfect competition. There is first the power of entrepreneurs and executives over their subordinates in a firm. There are differential opportunities for entry into a given market, variously based on factual, customary, and legal requirements regarding capital, education, social affiliation, or age and sex. There is the power of the state undergirding the legal framework which regulates relations among competitors, as well as between suppliers and customers, entrepreneurs and workers; this set of binding norms and optional contractual forms is never completely neutral with respect to the various interests involved, even if such neutrality is intended. Finally, there are often policies, instigated and opposed by different interests and pursued by governmental power, that aim directly to affect the pattern of competition—to maintain it in a certain form, to limit its intensity, or to keep imperfections of competition from having undesired results; modern antitrust legislation and medieval guild regulations, which limited the range of production and the number of employees allowed in a single shop, are but two well-known examples of such policies.

It is not, then, the absence of power or of power constellations favoring special interests that distinguishes situations in which many participants of roughly similar strength compete with each other. What is characteristic, however, is a peculiar pattern of resultant change, in the division of labor as in other respects—crescive rather than abrupt and *discontinuous* change, change less subject to autonomous planned direction by any of the participants and more determined by "mechanisms" that are rooted ultimately in the conditions of the man-made and natural environment no single participant can affect much. It was these mechanisms that gave rise to the classic image of the "invisible hand." Yet as modern economic policymakers in capitalist countries have learned, however imperfectly, such processes of crescive, aggregate change can be influenced, and even steered, if one controls certain points of leverage.

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definition competing units are not, relative to each other, specialized in complementary ways, but deal, "segmentally" separated, with similar goals. The problem raised complements Marx's polemic quoted in the preceding note. If one were to pursue the efficiency theory of differentiation, one would have to compare the efficiency results of competition, based on such factors as the mobilizing effects of the pursuit of self-interest or the peculiar combination of information and authority characteristic of decentralization, with the efficiency effects of further structural differentiation under varied circumstances in order to arrive at an explanation of differentiation based on efficiency consequences.

Occupational specialization, the central theme of the early discussions of the division of labor, has often been analyzed as a variant of decentralized, competitive change. Here, too, the prevailing conceptions have neglected the part played by organized interests, power, regulation, and planning and emphasized impersonal socioeconomic mechanisms which respond to changes in the scope of markets, in technology, and in the environment. The foregoing arguments regarding the role of power even in markets approaching perfect competition apply here too; in addition, labor markets are rarely models of competition on both sides of demand and supply.

If workers are employed and not well organized, the level and types of specialization will be determined by the economic and organizational interests of their employers. Unions and occupational associations exert an influence that varies with their power resources, but that can by no means be dismissed as a mere modification or retardation of ineluctable trends. The higher the class and status position of the members of an occupation, the stronger this influence will be—other things, especially the degree and efficiency of their organizations, being equal. For this reason, specialization in a profession reflects more closely conditions and interests within the occupational group than it does in lower ranking occupations. There is no space here to explore which interests in an occupation are likely to favor, which to oppose, further specialization. Generally, a reliable guide to prediction can be derived from analyzing the perceived impact of pending specialization on the competitive advantage, on income, status, and influence of different subgroups in the occupation. Government regulation often serves as an instrument to protect and advance the interests of certain occupational groups in a pattern of specialization. In many, if not in most, instances of a breakdown of complex craft skills into simpler unskilled or semiskilled components, changes in the legal order were an important contributing cause rather than merely a formal acknowledgment of more fundamental developments.[15]

V

Earlier I suggested that a consideration of developments of de-differentiation might be helpful in overcoming the diffuse and largely descriptive character of many analyses of structural differentiation and that it may aid in approaching a process analysis of these changes. De-differentiation in the most literal sense is the reversal of a process of differentiation and

[15] The role of power in shaping the place and functions of the professions in society, including their internal differentiation and their relations to other occupations, has recently received more attention (see Bucher and Strauss 1961; Rueschemeyer 1964; Freidson 1970*a*, 1970*b*; Johnson 1972; Rueschemeyer 1973).

a return to the *status quo ante* or, since history rarely can be turned back exactly, a situation similar in its basic outline. In the preceding discussions various obstacles and sources of resistance to developments of differentiation were identified; they constitute the main causes of de-differentiation in the literal sense if a new division of labor and organization does not become firmly institutionalized and the balance of power tilts toward the opponents of the development in question.

Of greater theoretic interest is another form of de-differentiation, which perhaps might better be called a "structural fusion of functions." Here we do not deal with a return to the original pattern, and thus the original constellation of interests favoring or opposing the development in question becomes irrelevant.[16]

If students and professors try to commit their university to a certain political position, we have—in the institutional context of the United States or Western Europe—a clear, though minor, example of an attempt at organizational de-differentiation. The underlying pattern is fairly general. Various specialized institutions are strongholds for certain opinions and concerns of a wider significance. If critical developments intensify these concerns the attempt may be made to mobilize the resources of the specialized institution for broader, here political purposes. Recent developments of UNESCO policy, political and moral pronouncements of scholarly societies beyond their immediate sphere of technical competence, the inclusion of political aims in the program of unions that previously had a narrow "business" orientation, and "political" strikes can all be subsumed under this type. Often such moves toward de-differentiation will be temporary phenomena only or minimally successful in the first place, because powerful interests are served by the specific delineation of the "proper" functions of most important institutions in stable societies. Furthermore, other interests with no direct stake in the dispute in question are often, through various mutual accommodations, committed to the existing pattern, too. This is particularly likely if an entry of previously nonpolitical organizations into the arena of broad political decision making is at issue. "Cobbler, stick to your last" is a demobilizing injunction. It takes a considerable exercise of power sustained over a long period of time to bring about such de-differentiations and make them stick. Still, a cer-

[16] Before I proceed further, it seems useful to touch briefly on certain conceptual difficulties. It may be objected that a fusion of functions, especially at the level of control and coordination, should not be considered as a process of de-differentiation, but should rather be analyzed as a form of integration. However, the seeming contradiction is resolved once it is realized that differentiation and integration are logically heterogeneous concepts, the former being a concept of structure, the latter indicating a functional problem. Thus, integrative problems may be solved, under different circumstances, by newly specialized roles and organizations as well as by a structural fusion of functions in one role or organization.

tain amount of tug and pull over the purposes of many institutions should be expected in all societies.

Another type of fusion of functions in certain organizations is also rooted in attempts to marshal additional power resources for broad purposes. Here a political, ideological, or religious organization seeks to secure the allegiance of its adherents and to broaden its following by offering intrinsically nonpolitical, nonideological, and nonreligious services and opportunities for association of various kinds. Missionary schools and hospitals, religious or political fraternal associations, as well as hiking, gardening, or singing clubs under denominational or party sponsorship represent an assortment of examples.[17] That developments of this kind often proceed with much greater ease, provided the potential clientele is willing and interested, highlights by comparison what was said about the obstacles encountered by attempts of previously nonpolitical organizations to move into the political arena of power. In the present case, the organizations are already established in that arena, and their expansion of activities is often not easily interdicted. On the other hand, great resources of power and coercion may be needed if the potential clientele is unwilling or if their interests are already served under the sponsorship of competing institutions. One might look from this perspective at the introduction of compulsory education under the sponsorship of the modern state in continental Europe, or at the imposition of ideological party auspices on most organized activities in 20th-century totalitarian states.

Certain forms of de-differentiation at the level of control and direction may be of strategic importance for large-scale and thorough transformations of a social order. Totalitarian control and penetration of all spheres of life is only one of these forms, one that is of particular importance in relatively developed societies where many people are potential political actors and where advanced structural differentiation makes centralized elite control difficult without pervasive indoctrination and control of commitment. In less developed societies the fusion of different functional elites and the concentration of their efforts on certain goals of social change may be sufficient to create a similar potential for rapid and thorough institutional change.

In either case, one particular fusion of functions, or at least a very close integration, seems of special importance—the linkage of "managerial" policy concerns with new value orientations and ideological guidance; the insistence in Mao's China on not letting the concepts of "red" and "expert"

[17] The latter types of associations were characteristic of the "political culture" of Germany before and after the First World War; see for instance Roth (1963). Similar patterns were found in the Netherlands after the Second World War (see Lijphart 1968). For an analysis of the general rationale of this type of fusion of functions see Olson (1965).

drift apart is a powerful example. Social change that involves such a reduction of differentiation and complex mediation between concerns of ultimate orientation and the more "realistic" preoccupations with running the machinery of society might be called charismatic change, borrowing from Weber's conception of charismatic authority (Weber [1921] 1968; see also Shils 1965 and Eisenstadt 1968). In analogy to Weber's theorems about the routinization of charisma, one should then expect that such processes of fusion of functions are followed later by new developments of structural differentiation, though their results are likely to be of a nature different from the social structure preceding the change.

VI

I have argued for an inclusion of power in the conceptual frameworks used to deal with structural differentiation of functions. First, the prevailing functional arguments that efficiency advantages underlie long-term advances of differentiation remain indeterminate without such an inclusion, because efficiency is always bound up with specific preference structures and the power resources of the actors variously affected by differentiation are thus a crucial element in the causal loops that give the functional argument explanatory power. Second, whether or not efficiency advantages have a central place in the explanatory model, the differential power resources and the power interests of the various relevant individuals and groups are likely to be of strategic importance for the immediate causal constellations underlying actual processes of differentiation and de-differentiation.

Taking off from these considerations, I have first reviewed the problems and dilemmas arising for power holders if they engage in policies encouraging structural differentiation within their immediate and wider spheres of authority; some of the conditions under which these contradictions between power interests and differentiation advantages are reduced were also discussed. A second set of arguments dealt with the consequences for differentiation that result from conflict, rivalry, and competition between more or less autonomous organizational units; the special case of competition which approaches the pure type of perfect competition was related to underlying patterns of power and to the resulting pressures for differentiation. Finally, various types of de-differentiation, or structural fusion of functions, were analyzed in relation to power interests; a tentative inference is that certain forms of such fusion are of strategic importance for far-reaching transformations of a social order.

The material discussed was varied indeed—a reflection of the abstract nature of the two central concepts, structural differentiation and power.

While I have referred primarily to the more narrowly defined phenomenon of organized domination rather than to power in general and have indicated at several points how the concept of differentiation should be subjected to differentiation itself, it is clear that further conceptual distinctions will become increasingly important in future work in this area. Yet it is well to remember that conceptual differentiations should not run much ahead of the substantive development of theory; and if theoretical conceptions cut through the compartmentalization of phenomena in their concrete social definitions and thus transcend the parochialism of common sense, one should treasure that as a gain of, and for, theoretical analysis—provided the concepts identify phenomena which are indeed similar in regard to causal conditions and consequences.

The aim of this essay was to demonstrate the theoretical utility of looking at power constellations and power interests as one important set of proximate causes involved in processes of differentiation and de-differentiation or fusion of functions. This must not be misunderstood as an assertion that power interests constitute the primary cause of differentiation and de-differentiation in all their variety. Power and domination rest on a very broad range of conditions and are used to pursue many different goals. If these conditions and goals are subjected to causal study, it seems plausible in any long-term analysis to expect the greatest impact from the same causal complexes that have been viewed in the past as major determinants of long-run structural change—population change, developments of technology and economic structure, changes in the basic value orientations and cognitive premises of sociocultural life. The exercise of power and domination and the interests it engenders, while constituting a dimension that may very well have some causal autonomy even in the long run, in large part merely reflect changes in other aspects of society and culture. I consider the separation of the dimension of power from the structure of productive relations a strategic mistake of Dahrendorf's (1959) attempt to reorient the Marxian analysis of class and class conflict; to view power in isolation from other major causal complexes would be a similarly consequential error in the theory of differentiation. What I do argue is that power constellations and power interests are of eminent importance as proximate causes in actual processes of differentiation and de-differentiation and that process analyses of both types of structural change should be given high priority in theoretical work in this area.

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A Macrosociological Theory of Social Structure[1]

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Social structure is conceptualized as the distributions of a population among social positions in a multidimensional space of positions. This quantitative conception of social structure is the basis for a deductive theory of the macrostructure of social associations in society. The likelihood that people engage in intergroup associations under specifiable structural conditions can be deduced from analytic propositions about structural properties without any assumption about sociopsychological dispositions to establish intergroup associations, indeed, on the assumption that people prefer ingroup relations. Group size governs the probability of intergroup relations, a fact that has paradoxical implications for discrimination by a majority against a minority. Inequality impedes and heterogeneity promotes intergroup relations. The major structural condition that governs intergroup relations is the degree of connection of parameters. Intersecting parameters exert structural constraints to participate in intergroup relations; consolidated parameters impede them. The more differentiation of any kind penetrates into the substructures of society, the greater is the probability that extensive social relations integrate various segments in society.

This paper presents a deductive theory of the structure of social associations, which rests on a quantitative conception of social structure. It is inspired by Simmel (1908), the father of quantitative sociology. To be sure, Simmel did not employ quantitative methods in his work. But by quantitative sociology I mean a subject matter, not the procedures used in investigating it. Quantitative sociology is the conceptual and theoretical analysis of the quantitative dimension of social life—of the implications of the numbers and distributions of people for their social relations—and in this Simmel was a pioneer. The conception of social structure adopted is also akin to that of the British structuralists in anthropology, notably Radcliffe-Brown (1940), Evans-Pritchard (1940), and Nadel (1957), except that their concepts pertaining to the structure of small tribes are adapted to make them applicable to the macrosociological study of large societies as well.

The analysis to be presented may be described as a primitive theory of social structure, in two senses of the term. First, it is a deductive theory,

[1] I am grateful to Ralph Bulle and Hilary Silver for bibliographical assistance and helpful comments.

in which theorems are derived from axioms or primitive propositions that logically imply them. These axioms are either analytic propositions, which are true by definition and refer to the way social structure is defined, or synthetic propositions, which are assumed to be true and rest on simple, plausible, and testable assumptions, for example, that social associations depend on opportunities for contact. Second, the theory is rooted in a primitive, rudimentary conception of social structure. The concept of social structure is confined to the distributions of people among different social positions. This is a very narrow view of social structure, which leaves out of consideration numerous broader implications and connotations of the term, such as value consensus, normative orientations, institutional systems, and functional interdependence. Not everything about social life can be explained in structural terms so narrowly conceived, but the endeavor here is to see how much can be.

CONCEPTUAL FRAMEWORK

There are a variety of approaches to the study of social structure, and implicit in them are different ways of conceptualizing social structure. The focus may be on the class structure or value orientations, on networks of social relations or institutional integration, on the division of labor or the construction of social reality, on status sets and role sets or the ecosystem. Yet certain elemental properties of social structure are recognized by most social scientists, notwithstanding differences in approach and focus. Whatever else may be encompassed by social structure, it nearly always includes the concepts that there are differences in social positions, that there are social relations among these positions, and that people's positions and corresponding roles influence their social relations. Typically, however, theories of social structure extend the concept beyond these elemental properties. Thus, Marx explains the class structure and the conflicting relations between classes on the basis of the dialectical interplay of productive forces with productive relations. Although Parsons explicitly distinguishes structures of roles and social interaction, which constitute social systems, from patterns of values and meanings, which constitute cultural systems (Parsons and Shils 1951, pp. 20–26; Kroeber and Parsons 1958), his theoretical explanation of social relations and interaction is in terms of value orientations, that is, in cultural rather than structural terms, and he acknowledges that this makes him a "cultural determinist" (Parsons 1966, p. 113). Homans's (1961) deductive theory explains social interaction and role relations on the basis of psychological principles that govern human motivation.

In short, most social theories seek to explain the patterns of relations among people, which are constituent elements of social structure, in terms

that refer to realms outside the social structure, narrowly conceived, be they technological, economic, cultural, or psychological factors. The opposite approach is adopted here. Of course, there can be no doubt that technological and economic conditions, cultural values, and psychological motives influence human behavior and hence social relations. This is not at issue. Granted the existence of these influences, the question raised is what independent influences the structure of social positions in a society or community exerts on social relations.

Macrostructure

Social structure is conceptualized narrowly as referring to the distributions of a population among different social positions that reflect and affect people's relations with one another. To speak of social structure is to speak of differentiation among people. For social structures, as conceptualized, are rooted in the social distinctions people make in their role relations and social associations. These social distinctions find expression in differences in roles and positions, which in turn influence subsequent social associations. But when the structure of an entire society or community is under consideration, persons naturally occupy several social positions simultaneously, not just one; they have occupations, belong to religious groups, live in communities, work in establishments, are more or less educated, and occupy socioeconomic statuses. A population distribution exists for each type of position.

Accordingly, the macrostructure of societies can be defined as a multidimensional space of social positions among which people are distributed and which affect their social relations. This abstract conception makes society's macrostructure homologous to the microstructures of role relations of individuals. In both cases, formal properties of social positions and relations are abstracted from their substantive contents, notably from cultural and psychological orientations. A fundamental difference, however, is the way in which social positions and relations are defined. Two important problems the macrosociological analysis of social structure must solve are how to deal with the huge number of personal relations in a society or community and how to take into account the multiple positions persons occupy, as Laumann (1973, pp. 2–7) notes.

Microstructures are the networks of interpersonal relations anchored in individuals, as illustrated by a sociogram of links between persons in a small group. Originating in the tradition of sociometry (Moreno 1934), microstructural inquiry has given rise to three main approaches: graph theory, network analysis, and block models. Graph theory (Harary, Norman, and Cartwright 1965; Davis 1967) uses psychological assumptions and mathematical principles to derive propositions about the configurations of links

that are most likely to develop. Network analysis (Barnes 1954, 1972; Mitchell 1969) dissects in detail the networks of person-to-person links in which specific individuals are involved, and the study of these microstructures anchored in individuals is explicitly juxtaposed to the macrosociological study of positions and groups in society represented by British structuralists in anthropology (Mitchell 1969, pp. 1–10). The block models constructed by White and his collaborators (White, Boorman, and Breiger 1976) divide a group into blocks of individuals with equivalent structural positions, defined on the basis of the similarity of the links of the persons in one block with those in others, which means that a block is not necessarily a subgroup of individuals who have direct links to one another. Whether microstructural studies center attention on direct links, as graph theory does, or not, like block models, they define the location of individuals in the social structure on the basis of an examination of all, potential as well as existing, person-to-person links. Such an analysis is not possible without modification for large collectivities. It is usually applied to groups of fewer than 100 persons, though procedures to apply it to groups of up to 1,000 persons have been developed (Coleman and MacRae 1960).

Macrostructural inquiry is concerned with the patterns of social relations among different social positions occupied by many persons, not with the networks of all relations between individuals. This requires redefinitions of two central concepts, position and relation, as Laumann and Pappi (1976, pp. 18–20) point out. Whereas microstructural studies define the positions of individuals in terms of the social relations in which they are involved, the definition of social position in macrostructural analysis is initially in terms of common or similar social attributes of people, such as their religion or socioeconomic status. The social relations between positions with many incumbents are not dichotomous links that either exist or do not exist but are defined as the variable likelihood or rate of association of incumbents of one position with those of another, for example, the rate of intermarriage or the frequency of intergroup contacts. Note that this macrostructural definition of social relations rests on actual associations between persons, as the microstructural definition does—except that a variable rate is substituted for a dichotomous link—which distinguishes the structural approach from theories that focus on the relations of functions, institutions, and values.

A crucial remaining problem is how to take account of the multiple positions people occupy. One strategy for dealing with this problem is to select one dimension of social positions, which is considered on a priori grounds to be of special importance, and to infer from the empirical analysis of observed social relations among these positions other dimensions that affect social relations. This is the strategy Laumann (1973, pp. 1–82)

adopts in one of the few systematic empirical studies of macrostructure. He selects religion, ethnic affiliation, and occupation as three types of important positions. For each, the data on friendships between positions are subjected to smallest-space analysis, the results of which indicate new dimensions of social differences that influence these friendship choices. For instance, the rates of friendships among occupational groups depend on differences in the socioeconomic status of occupations and, to a lesser extent, on differences between primarily bureaucratic and primarily entrepreneurial occupations.[2] A different strategy for dealing with multiple positions is to analyze how the extent to which various types of positions are correlated influences social relations, after having first examined how differences in positions of a single type influence them. This is the strategy adopted here.

Structural Parameters

The structures of societies and communities are delineated by parameters (Blau 1974). Structural parameters are the axes in the multidimensional space of social positions among which the population is distributed. They are attributes of people that underlie the distinctions they themselves generally make in their social relations, such as age, race, education, and socioeconomic status. People can be classified on the basis of innumerable attributes, any of which may be a parameter. But if a classification made by an investigator does not influence social relations at all, or exerts only idiosyncratic influence on the personal relations of some individuals, it is not meaningful to consider it indicative of social positions. Hence, the double criterion of a parameter circumscribing social positions is that it is an attribute by which a population is classified and that the social relations among persons similarly classified differ on the average from the relations between persons in widely different categories. A typical difference, according to an assumption to be introduced, is that the social associations among incumbents of the same position or proximate ones are more prevalent than those between incumbents of different or distant positions. In short, a parameter is a variable that characterizes individuals and differentiates their role relations and social positions. At the same time, the distribution of the population among these positions yields a new variable that characterizes the degree of differentiation of the society in terms of the parameter. The focus of structural analysis is on the derived variables indicative of the degrees of differentiation of societies in various respects and on their implications for social life.

The two basic types of parameters are nominal and graduated param-

[2] Laumann (1973, pp. 111–30) also devises an ingenious procedure for the empirical study of the microstructures of interpersonal relations of individuals in a large city.

eters. A nominal parameter divides the population into subgroups with distinct boundaries and without an inherent rank order. Sex, religion, race, and place of residence are nominal parameters. A graduated parameter differentiates people in terms of a status rank order, which is in principle continuous, so that the parameter does not draw boundaries between strata. Income, wealth, education, and power are graduated parameters. The social positions delineated by nominal parameters are designated as group memberships, those delineated by graduated parameters as status. Hence, group and status are defined very broadly; for instance, females are a group and age is a status. Groups are not confined to collectivities all of whose members associate with one another, and status is not confined to differences in deference and compliance.

The two generic forms of differentiation, under which its specific forms can be subsumed, are heterogeneity and inequality. Heterogeneity refers to the population distribution in terms of a nominal parameter. The criterion of degree of heterogeneity is the probability that two randomly selected persons do not belong to the same group. For any nominal parameter, the larger the number of groups and the more evenly the population is divided among them, the greater is the heterogeneity. Thus, a community's ethnic heterogeneity is greater if there are many than if there are few ethnic groups; but it is not so great if most people belong to one ethnic group as it is if the population is more evenly divided among several. The criterion takes both components of heterogeneity into account.[3]

Inequality pertains to the population distribution in terms of a graduated parameter. The criterion of degree of inequality is the average difference in status between any two pairs relative to average status. For example, the more the average difference in years of schooling exceeds the average number of years of schooling in a society, the greater is the inequality in formal education. Another way of looking at inequality is that it refers to the extent to which a status resource is concentrated. For instance, the more the national wealth is concentrated in the hands of the richest persons, the greater the inequality in wealth is. It turns out that these two ways of conceptualizing inequality are actually equivalent, and they are indicated by the most widely used empirical measure of inequality, the Gini coefficient.[4] The former conception of inequality is meaning-

[3] The empirical measure of heterogeneity is the index proposed by Gibbs and Martin (1964): $1 - [\Sigma x_i^2/(\Sigma x_i)^2]$, where x_i is the number of persons in a group and the sum is taken over all groups.

[4] A formula for the Gini index, which is equivalent to the one usually used for computing it, is: $2\Sigma s_k p_i (p_j - p_k)/2\Sigma s_i p_i$, where s_i is the mean status in a category, p_i the fraction of the population in that category, and p_j and p_k the fractions of the population whose status is below (p_j) and above (p_k) that category, respectively, with the sum taken over all categories. The numerator is the (estimated) mean status difference between all pairs, and the denominator is twice the mean status of the population. I am

ful for any status criterion, even when the latter is not (which discloses that the Gini index is substantively appropriate for any status criterion). It is not very meaningful to speak of the degree of concentration of years of schooling, mathematical aptitudes, or intelligence, but it is meaningful to speak of the average difference in these characteristics.

Parameters are not orthogonal dimensions of social structure. On the contrary, their correlations are fundamental characteristics of macrostructures of primary interest in their analysis. The correlations of a nominal parameter with graduated ones indicate status differences among groups, for example, the differences in education and income among ethnic groups. Then one can examine how the status distance between groups affects the associations between their members.[5] If a nominal parameter with many categories is substantially correlated with graduated parameters, a new graduated parameter can be derived from the ranking of the categories. Thus, occupation is a nominal parameter, but classifying occupations by education and income yields an index of occupational status (Duncan 1961). Generally, the degree to which parameters intersect, or alternatively consolidate differences in social positions through their strong correlations, reflects the most important structural conditions in a society, which have crucial consequences for conflict (Coleman 1957) and for social integration.

Societies vary in the extent to which their structures are differentiated along various lines, and they also vary in the extent to which the different segments are integrated. Differentiation and integration are complementary concepts, and the definition of integration takes this into account. Both inequality and heterogeneity, the two forms of differentiation, are barriers to social intercourse, on the assumption that proximate status and common group membership promote social associations. If there were no connections among different social positions, however, these positions would not constitute elements of a single social structure. These connections that integrate the various segments of society are produced by the social associations between persons who occupy positions in different segments, in different groups or hierarchical strata. Society's integration is conceptual-

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grateful to Professor J. P. van de Geer, University of Leyden, for providing this formula and indicating its equivalence with the one usually used to compute the Gini index from the Lorenz curve.

[5] McFarland and Brown (1973) note that the concept of social distance has been used in two distinct ways: some, like Sorokin, use it to refer to differences in the attributes of persons, such as their occupation or income; others, like Bogardus, use it to refer to differences in social relations, such as rare marriages or disinclination to engage in social intercourse. Graduated parameters are indicative of status distance in the first sense, which is distinguished from rates of social association between incumbents of positions (social distance in the second sense). Laumann's (1973) dimensions indicate social distance in the second sense.

ized as resting on the actual social associations between persons in different segments, not on functional interdependence or common values, though these may contribute to integration by promoting social interaction between persons in different groups and strata. Although the assumption is that social associations are less extensive between persons in different social positions than among those in the same ones, high rates of associations among different groups and hierarchical strata are the criterion of macrosocial integration.[6] Dense networks of ingroup relations integrate individuals in their groups, but they threaten to fragment society, the integration of which depends on extensive intergroup relations.[7]

In accordance with this conception of social structure, the theory centers attention on its quantitative properties: the number of persons in different positions and the size of groups; frequency distributions among positions indicative of inequality or heterogeneity; whether various parameters are nearly orthogonal or highly correlated; the degree to which differentiation occurs within or among society's substructures; and how these structural conditions affect the rates of social association among groups and strata. Concern is with the implications for social life, not of the attributes of individuals, but of the distributions of their socially relevant attributes and the correlations of these distributions in society, which are considered the fundamental properties of social structure. Starting with propositions that employ simple concepts, like group size, the theory progresses to more complex terms derived from the simpler ones, such as heterogeneity, inequality, intersecting parameters, consolidation, penetrating differentiation. It is a theory of the influences of structural conditions, conceived narrowly and in quantitative terms, on social interaction, a theory of the structure of social association.[8]

INTERGROUP RELATIONS

Given the significance attached to intergroup relations for society's integration, it is important to ask which groups have higher rates of intergroup associations than others and why they do. Is the rate of religious intermarriage higher for American Catholics or Protestants? Are inter-

[6] Society's social integration is a theoretical term not directly measured but reflected in the measured prevalence of social associations among different positions in terms of various specific parameters. Similarly, the structural complexity of society is an unmeasured theoretical term reflected in the degree of differentiation in terms of various specific parameters and the degree of intersection of these parameters.

[7] For ease of expression, the term "intergroup relations" is sometimes used, as here, to include relations among different strata as well as groups.

[8] It hardly needs saying that not all important aspects of social life can be considered in a brief paper. Thus, this paper does not deal with ingroup relations, with change, with conflict, but the fuller exposition of the theory does (Blau 1977).

racial friendships more prevalent among blacks or among whites? Do Jews or Christians spend more time, on the average, associating with members of the other religious group? To deal with a specific form of association: Are Catholics or Protestants more likely to engage in premarital sexual intercourse with members of the other group? Blacks or whites? Jews or Christians?

Minority Groups

Empirical research could answer such questions, though it would be difficult to obtain reliable information for answering some of them, like the amount of time spent and the frequency of premarital intercourse. In any case, a hypothesis is needed to guide the research, or at least to interpret its findings concerning what properties of groups account for differences in extent of intergroup relations. A relevant hypothesis is derivable from Durkheim's (1951) theory that explains the lower suicide rates of Catholics and Jews than of Protestants on the basis of their stronger ingroup integration, which in turn is attributable to the deemphasis on individualism in Catholicism and Judaism and, in part, to position as a minority group. On the plausible assumption that strong ingroup bonds inhibit intergroup relations, one might infer that intergroup relations of various kinds are comparatively infrequent among Catholics and among Jews, and probably also among American blacks, owing to their being a minority. Other considerations lead to the opposite conclusion, however. Research indicates that the religious affiliations of Jews are less strong and have declined more than those of Protestants or Catholics (Lazerwitz 1961; Lenski 1961, pp. 44–53), which suggests that Jews are more likely than Christians to enter into interreligious marriages, and possibly premarital sex as well.[9] One might also conjecture, on the basis of admittedly quite limited empirical evidence, that premarital sex relations are more prevalent among blacks than whites (Sorenson 1973, pp. 172, 255) and more prevalent among Catholics than puritanical Protestants (Kinsey, Pomeroy, and Martin 1948, pp. 469, 472) and hence that such relations with the outgroup are correspondingly more prevalent among blacks and among Catholics.

Jews do have higher rates of premarital sex relations with the outgroup than Christians in the United States and in most western countries; blacks have higher rates than whites; Catholics have higher rates than Protestants. But the reason is not that Jews are less religious, nor is it that blacks

[9] But the same study reports that more Jews (92%) than Catholics (81%) or Protestants (75%) state that it is wiser to marry within one's own religious group (Lenski 1961, p. 54), which suggests the opposite, that Jews are less likely than Christians to intermarry.

or Catholics have fewer puritanical inhibitions about premarital sexual intercourse. It is simply that there are fewer of them than of their counterparts. Every act of intergroup sexual intercourse involves one person from each group, so that the difference between two groups in the rate of such acts is an inverse function of their size. The same is true of all dyadic symmetric association:[10] the rate of intergroup associations of the smaller of two groups must exceed that of the larger. The inference derived from Durkheim's theory is wrong. Minority groups may have stronger ingroup bonds, but their rates of social association with the majority exceed the majority's rates with them.

Hence, the arithmetic properties of groups imply the theorem that *in the relation between any two groups, the rate of intergroup associations of the smaller group exceeds that of the larger.* This first theorem (T-1) applies to three forms of associations and all their specific manifestations: (1) the proportion intermarried (or having another exclusive association, as mutual best friends) in the smaller group exceeds that in the larger (T-1.1); (2) the mean number of intergroup associates in the smaller group exceeds that in the larger (T-1.2); (3) the mean amount of time spent in intergroup associations is greater for the smaller than for the larger group (T-1.3).[11] Although the same principle does not apply to the proportion of members who have any intergroup associates, which can be greater in the small than in the large group, this is only possible if some members of the small group have particularly many intergroup associates and spend much time with them. The smaller group is more involved than the larger in the intergroup relations between the two, either because a greater proportion of the smaller group's members have intergroup associates with whom they spend time, or because those who do have intergroup associates have more of them and spend more time with them.

Implications

These theorems are tautological, entailed by the definitions of group size and intergroup relations. But they have implications that are not tautological, and even those that are are not self-evident. When differences in group size are very great, most members of the majority have no social contact with the minority. This implies that most WASPs have no social contacts with blacks, Jews, and other small minorities, notwithstanding

[10] Concern is with the frequencies of actual dyadic associations, which are necessarily symmetric. The theorems do not apply to sentiments, deference, or sociometric choices, which are often not symmetric, nor do they apply to relations between a speaker and his audience or a leader and her followers, which are not dyadic.

[11] Every main theorem is in italics, though corollaries are not, and it (or the set including corollaries) is explicitly derived in statements immediately preceding or following it.

extensive associations of minorities with the majority. The social experience of associating with persons with different backgrounds undoubtedly affects attitudes and conduct. It may well broaden people's horizons, promote tolerance, and stimulate intellectual endeavors (Simmel 1908, pp. 685–87; Laumann 1973, pp. 98–105, 126–28). The structurally generated differences in intergroup experience between small minorities and a large majority would lead one to expect these characteristics to be more prevalent among the minorities (Veblen 1919; Seeman 1956).[12] For example, high intellectual achievements would be expected of disproportionate members of minorities, which is apparently the case for some minorities, such as Japanese and Jews. The low intellectual achievements of blacks conflict with this expectation. But a recent study shows that the educational achievements of blacks, when their initial handicaps are controlled, exceed those of whites (Portes and Wilson 1976), which conforms to the expectation that the more extensive intergroup experience of minorities furnishes intellectual stimulation.

For the groups delineated by a given nominal parameter, the probability of extensive intergroup relations increases with decreasing size (T-2.1). Specifically, group size is inversely correlated with the proportion intermarried (T-2.1), the mean number of intergroup associates (T-2.2), and the mean amount of time spent with intergroup associates (T-2.3). These probability theorems, which are not tautological, logically follow from the earlier deterministic ones. If in every dichotomy of groups the rate of intergroup associations of the smaller exceeds that of the larger group (T-1), the average rate of all small with larger groups must exceed that of all large with smaller groups, wherever the array of groups by size is divided into smaller and larger ones. Although some small groups may have lower rates of intergroup associations than some large ones, this necessitates that other small groups have exceptionally high rates to compensate for it. The theorems are restricted to comparisons of groups in terms of a given parameter, because differences in the salience of parameters independently influence the extent of intergroup relations. One can hardly expect the proportion of blacks married to whites to exceed the proportion of blue-eyed persons married to brown-eyed ones in the United States, though there are undoubtedly fewer blacks than blue-eyed persons, since skin-color, so-called, has much and eye-color little salience in American social life.

Research generally corroborates these theorems. Studies of religious and ethnic intermarriage find that group size is inversely related to rates of outmarriage (Thomas 1951; Locke, Sabagh, and Thomas 1957; Barnett

[12] Although the theory is macrostructural, it has microstructural implications, as illustrated by these conjectures about possible further implications of the theorems, which themselves have been deductively derived.

1962; Bealer, Willits, and Bender 1963; Burma 1963), sometimes after having started with a different hypothesis. Thus, Bealer and colleagues tested the hypothesis that closeness of religious dogma governs rates of intermarriage between Christian denominations, but their data required rejection of this hypothesis and disclosed that the size of a denomination largely governs its marriage rates with various specific others as well as its total out-marriage rate.[18] Interethnic sociable associations also have been observed to be more probable in small minorities (Williams 1964, p. 162). An apparent exception is revealed by comparison of what are often referred to as the three major religious denominations in the United States—Protestants, Catholics, and Jews—which shows that Jews have the lowest out-marriage rate, although they are the smallest group. But these three groups are not a meaningful classification based on a single parameter. Religion divides people into Christians and Jews, as well as such other religious categories as Muhammedans and Buddhists, which can be further divided into Catholics and Protestants and still further into specific denominations. The salience of the major subdivision is greater than that of the finer ones, as indicated by the higher marriage rates between Protestants and Catholics than between either and Jews. Nevertheless, the low intermarriage rates of Jews must be considered a negative case for T-2.1. As a probability theorem, T-2.1 is not falsified by one negative case. What would falsify it would be the failure of the data on numerous groups (such as those on 18 denominations mentioned in n. 13) to reveal a negative correlation between their size and intermarriage rate.

Discrimination

The tautological theorem initially advanced (T-1) has unexpected implications for discrimination against minorities. The rate of intergroup relations depends on group size; so does a change in this rate, because the numerators are again identical while the denominators (group size) vary. A given number of associations yields a higher rate for a smaller group, and a given change in this number yields a greater change in the rate for the smaller group, owing to the smaller denominator. This has paradoxical implications for discrimination by a majority group against a minority, "discrimination" meaning simply the reluctance of majority members to associate with minority members, not any other bias in making decisions.

The more a majority discriminates in social intercourse against a minority, the smaller is the difference between the majority's lower and the minority's higher rate of intergroup associations (T-3). This surprising

[18] The rank correlation between the size of the 18 denominations and the rate of total intermarriage is —.79 (computed from table 3, Bealer et al. 1963).

theorem follows in strict logic from the premises embodied in the definitions of terms, and it applies to proportion intermarried, average number of intergroup associates, and average amount of time spent with them. If the purpose of majority discrimination against minorities is to control the majority's own members' tendencies to associate with members of the minority, be it by enacting laws or through informal pressures, it serves its purpose well. But if the objective is to prevent minorities from having access to the majority without infringing on the freedom of choice of the majority's own members, majority discrimination defeats its purpose. For it restricts its own members' opportunities for intergroup associations *much more* than that of the minority's members, the more so the more severe the discrimination and the greater the difference in size. This applies to intimate as well as casual associations, and it applies if concern is restricted to completely mutual associations wanted equally by both parties, excluding such well-known cases of exploitation as sweatshops and sexual exploitation by majority men of minority women.

A corollary of T-3 is: As a majority's discrimination in social intercourse against a minority declines, the difference between the majority's lower and the minority's higher rate of intergroup associations becomes greater (T-*3.1*). To be sure, reduced discrimination by the majority increases its rate of intergroup associations as well as the minority's. But it simultaneously enlarges the difference between the majority's own lower and the minority's higher rate of intergroup involvement, inevitably so, strange as this may seem. The proportion of group members insulated from any intergroup associations probably changes in complementary fashion with changes in discrimination, but not necessarily: If some minority members have many intergroup associates, the proportion of the minority who have none could be the same as (or greater than) the proportion of the majority who have none. The larger the difference in size between majority and minority, however, the greater is the probability that the proportion insulated, and changes in that proportion, are higher in the majority than in the minority, because a person can have only a limited number of associates, particularly of close associates. For example, for the proportion of American blacks and whites who are insulated from any intergroup friendships to be the same, the average black would have to have 11 times as many intergroup friends as the average white (with most blacks having still more and some having none). Consequently, a probability theorem can be deduced from T-3.1: *As a majority's discrimination in social intercourse against a minority declines, the probable difference between the majority's higher and the minority's lower proportion of members who are insulated from close intergroup associations increases* (T-4).

A fictitious illustration can explicate this. If there are 1 million pairs of mutual best friends between a majority of 100 million and a minority

of 10 million persons, 1% of the majority and 10% of the minority have such a friend, and 99% and 90% do not, a difference of 9% (table 1).[14] We imagine that a decline in discrimination by the majority increases these intergroup friendships from 1 to 3 million pairs, which raises the proportion with, and diminishes the proportion without, a best intergroup friend 2% in the majority and 20% in the minority. Hence the reduction in discrimination has increased the difference in both the proportion with and the proportion insulated from such intergroup friendships from 9% to 27%. It has also increased the ratio of the proportion insulated in the majority and in the minority, from 11:10 to 14:10.

The paradox is that when discrimination by a majority against a minority subsides, the inequality in intergroup involvement between the two becomes more pronounced. Discrimination against minorities in education, employment, and social intercourse has in all likelihood declined in the United States in recent decades, notwithstanding some backlash. Although this has expanded intergroup friendships and diminished insulation from intergroup contacts in the majority as well as the minorities, it has simultaneously increased the difference between minorities and the majority in the extent to which they know each other, socialize with each other, have close enough relations to trust each other. A decline in discrimination, while helping to integrate minorities in society, moves the social experience provided by intergroup life of most minority members and most majority members further apart.[15]

TABLE 1

BEST FRIENDS BETWEEN A MAJORITY AND A MINORITY

	Majority (%)	Minority (%)	Difference (%)	Ratio
1 Million Intergroup Pairs:				
1. Number of persons (millions)	100	10	..	...
2. Proportion with intergroup friend	1	10	...	...
3. Proportion without intergroup friend	99	90	9	11:10
Reduced Discrimination:				
3 Million Intergroup Pairs				
4. Proportion with intergroup friend	3	30	...	...
5. Proportion without intergroup friend	97	70	27	14:10

[14] Mutual *best* friends are used in this fictitious illustration to simplify it. Were the criterion close associate or "mutual friend," whether or not both are best friends, the values for "with intergroup friend" (rows 2 and 4) would be the same, but the values for "without intergroup friend" (rows 3 and 5) would be somewhat altered, though the pattern of differences would be essentially the same.

[15] The opposite extreme with virtually no social associations between minority and majority implies that there is no difference between the two groups in the social experiences entailed by intergroup relations, but it also implies that the minority is hardly integrated in the society, inasmuch as the integration of groups in society is defined by their intergroup relations.

If the trend of subsiding discrimination continues, all members of a small minority have eventually close associates in the majority, while most members of the majority still have no close minority associate. Ultimately, minority members may be as likely to marry and to be intimate with members of the majority as with members of their own group, which would mean that the minority has become fully assimilated and has ceased to exist as a group governing social distinctions. The process of assimilation enlarges some differences in the social experiences of a minority and the majority—those resting on intergroup involvement—before finally obliterating most or all of them.

What structural conditions foster intergroup relations and diminish discrimination? Two aspects of differentiation have these consequences: heterogeneity and intersecting parameters.

DIFFERENTIATION

The size of collectivities and the size distributions within and among them have a dominant impact on social life. This profound insight of Simmel's has long been neglected in social theory and research, particularly in macrosociological inquiries, which have either ignored size completely or tried to control it in order to search for other social influences presumed to be more interesting.[16] Recently, however, social scientists have begun to take size seriously as a concept of fundamental theoretical importance for understanding social relations and social structures. Thus, Mayhew constructs theoretical models that explain on the basis of size alone such diverse phenomena as the formation of elites in societies (1973), differentiation in organizations (Mayhew et al. 1972), and crime rates in cities (Mayhew and Levinger 1976). Research on organizations finds that their size governs many features of their administrative structure (Pugh et al. 1969; Blau and Schoenherr 1971). This approach, which characterizes the theory here outlined, centers attention on the implications of the number of people and their distribution for social relations and for the social structure, including the implications that are mathematically inevitable, from which often further conclusions can be deduced that are not mathematically inevitable.

If differences in group size influence rates of social association between group members, as we have seen, it is of interest to examine how social relations are affected by the distributions of society's entire population among groups and in terms of status, that is, by the two forms of differentiation, heterogeneity and inequality. For this purpose, substantive assumptions about two conditions that influence people's associations are

[16] Attempts to control size in research on social structure often actually fail to do so, as noted by McFarland and Brown (1973, pp. 238–40) and others.

introduced. The preceding theorems do not require these assumptions, or any others, because they are deducible from the analytic propositions defining groups (in terms of size) and intergroup relations (in terms of rates). It should be noted that all theorems, those depending on assumptions as well as those that do not, make assertions about rates of social association, not about the social associations of particular individuals, just as Durkheim's (1951) theory makes statements about rates of suicide. Not all Protestants commit suicide, of course. Similarly, many American whites have extensive friendships with blacks, and many blacks have no white friends, but this does not conflict with the deterministic prediction, implied by T-1 and the racial composition of the United States, that the rate of interracial friendships of blacks exceeds that of nonblacks. Even deterministic propositions about rates of social events imply only probability statements about individual events.

Assumptions

The first assumption is that *social associations are more prevalent among persons in proximate than those in distant social positions* (A-1). Social proximity is defined in terms of parameters, not in terms of rates of social association. Hence, it is a dichotomy for a nominal parameter but varies by degree for a graduated parameter. The assumption has accordingly two corollaries: for nominal parameters, ingroup associations are more prevalent than outgroup associations (A-1.1); for graduate parameters, the prevalence of social associations declines with status distance (A-1.2). The prevalence of social associations refers not to their absolute rates but to the excess of the observed rates over those theoretically expected on the basis of the population distribution.[17] (The degree to which these observed values exceed the expected ones indicates the salience of the parameter.)

There is much empirical evidence that supports this assumption. For example, disproportionate numbers of marriages involve spouses of the same religion (Kennedy 1944; Hollingshead 1950; Coombs 1962; Willits, Bealer, and Bender 1963); ethnic origin (Murdock 1949; Coombs 1962); residential area (Schapera 1946; Murdock 1949); education (Blau and Duncan 1967); socioeconomic status (Centers 1949; Murdock 1949; Hollingshead 1950; Goode 1962; Blau and Duncan 1967). Friendships, too, are disproportionately often formed with persons who share one's re-

[17] The actual rate of ingroup associations of very small groups is most unlikely to exceed in absolute value their rate of intergroup associations, which is the reason why the operational criterion of the assumption is the excess of actual rates over those expected on the basis of the population frequency distribution. But the operational criterion of intergroup relations is actual rates, without controlling frequencies, since the influence of size or frequencies is the substantive concern of the theory, which would be concealed were frequencies statistically controlled.

ligion and socioeconomic background (Hollingshead 1949; Laumann 1973), as well as with those of the same sex and race (Lazarsfeld and Merton 1954). To be sure, the assumption is undoubtedly not met for all possible attributes of people. Those attributes for which it is not met may be considered not to refer to social positions, which transforms the principle that prevalence of associations depends on proximity from a substantive assumption into an analytic proposition that defines social positions and parameters.

A second assumption is that *social associations depend on opportunities for social contacts* (A-2). This assumption is hardly open to question, because persons cannot associate without having opportunities for contact, and a direct implication of it is corroborated by empirical evidence. Inasmuch as the physical propinquity of people increases their opportunities for contacts, the assumption implies that *physical propinquity increases the probability of social association* (T-5). Research shows that propinquity, including very small differences in physical distance, increases the likelihood of friendships (Festinger, Schachter, and Back 1950) and of marriage (Christensen 1958).

A final assumption is that *the influences of various parameters on social associations are partly additive, not entirely contingent on one another* (A-3). The assumption is not that parameters have no interaction or joint effects on social relations, merely that the effects of any one are not entirely dependent on and cannot be completely suppressed by variations in other parameters. In other words, it is assumed that persons who share several social positions are more likely to associate with each other than are those who share fewer and that sharing a given social position makes associations more likely between both those who do and those who do not share other positions. For example, people who have the same religion or education or any other salient social attribute are expected to be more likely to associate with each other than are others, regardless of whether they also share other social attributes. Actually, A-3 simply makes explicit what is already implicit in A-1—that any common or similar attribute that is considered a parameter makes social associations more probable.

Differentiation in a Single Dimension

Before turning to the implications of these assumptions for differentiation, implications of the initial theorems for status differences are examined. Status distributions are nearly always positively skewed, with few persons in high status and many in lower status. Some status structures are pyramids, with the largest frequencies at the bottom and declining frequencies as one moves up. Authority in organizations and wealth in society are typically distributed in this manner. Other status structures are trun-

cated diamonds, with frequencies first increasing as one goes up from the bottom and only then decreasing. This tends to be the case for income distribution in western societies. A new theorem is deducible from T-1, that the rate of social association of a smaller with a larger group exceeds that of the larger with the smaller group, given the size differences in a pyramidal status structure or in the pyramidal upper part of another status structure:

In a pyramidal status structure, the rate of social association of any higher with any lower stratum exceeds that of this lower with this higher stratum (T-6).[18] If such a difference necessarily characterizes the rates of association between any two strata in a pyramid, it is probable that all strata except the lowest have higher rates of association with lower than with equivalent higher strata. The reason is that all strata in a pyramid except the lowest are smaller than others below and larger than others above them, and the theorem that a group's probable rate of association is an inverse function of its comparative size (T-2) implies that the association rate of a group with another compared to which it is small probably exceeds its association rate with another group compared to which it is large. Consequently, an inference from T-2 is: *People in middle as well as in high strata in a pyramid probably associate more with others below them than with others above them in status* (T-7).

Reductions in inequality diminish the impact of status on social associations (T-8). This theorem follows from the assumption that status distance discourages social associations (A-1.2) and the definition of inequality in terms of average status distance. Note that there is no reason to assume that a decline in educational inequality, for instance, lessens the salience of education, at least in the short run; the implication is that a given difference in education will continue to have the same adverse effect on social intercourse. However, after educational inequality has declined, fewer people than before differ widely in education; therefore educational differences have less impact on social intercourse in the society. Whether reductions in inequality affect in the long run the salience of education (or another status) and consequently have further repercussions for social associations is a moot question.

Heterogeneity and inequality, the two forms of differentiation, have opposite implications for social associations among persons whose social positions differ. To be sure, heterogeneity, like inequality, creates barriers to social intercourse, in accordance with the assumption that intergroup associations are less prevalent than ingroup associations (A-1.1). But much or increasing heterogeneity weakens these barriers and promotes intergroup relations. This paradoxical conclusion follows from the assump-

[18] Strata are defined by any equal status intervals, but not by population percentiles; for example, by intervals of $1,000 in income, or by intervals of two years of schooling.

tion that social associations depend on opportunities for social contacts (A-2). Since the degree of heterogeneity is defined by the probability that two randomly selected persons belong to different groups, increasing heterogeneity makes it more likely that fortuitous contact will involve members of different groups, thereby increasing the opportunities for and hence the probability of intergroup associations. In relatively homogeneous communities, there are few group barriers, but those that do exist tend to inhibit social associations more than the more prevalent group barriers in heterogeneous communities. Prevalent group barriers are not so great barriers to social intercourse. The theorem logically deducible from A-2 and the definition of heterogeneity is: *Increasing heterogeneity increases the probability of intergroup relations* (T-9).[19]

Multiple Differentiation

Only the influences of a single parameter on social life have been considered so far, not yet those of combinations of parameters. People in all societies have several group affiliations, and those in complex societies have many, which are partly intersecting. Differences in sex, race, national background, religion, and occupation do not coincide, though some are correlated. The numerous, partly intersecting nominal parameters of complex social structures engender multiform heterogeneity, which exerts much greater structural constraints on intergroup relations than simple heterogeneity in one dimension.

Pronounced multiform heterogeneity compels people to establish intergroup relations, because it implies that ingroup relations are simultaneously intergroup relations in terms of different parameters (Blau 1974). It is impossible not to associate with outsiders when one's ingroup associates in one dimension are, in several others, members of other groups than one's own (Merton 1972, pp. 22–25). For individuals to satisfy their most salient ingroup preferences, they must set aside other ingroup preferences and enter into intergroup relations along other lines. The very fact that prejudiced persons discriminate against associating with out groups of various kinds restricts their other choices and constrains them

[19] In the analysis of differentiation in a single dimension, nominal parameters are treated as unordered categories and interest is confined to the degree of heterogeneity they indicate. Actually, nominal groups differ in various ways on the basis of which they can be ordered. Occupations are an obvious example: They are nominal groups, but they differ in status. The analysis of multiple parameters takes into account such graded differences among nominal groups in terms of which they can be ordered, inasmuch as it deals with the correlations of a nominal with various graduated parameters. However, the primary substantive focus here is not on trying to discover the major dimensions in terms of which groups can be ordered, as it is for Laumann (1973), but on the extent to which parameters are correlated or orthogonal and its distinctive significance for social relations.

to be more tolerant about associating with outsiders in terms of these other parameters. For example, many academics seem to exhibit strong ingroup biases in their conduct in favor of associates with similar advanced education, who are also academics, and who often are even in the same discipline. One might conjecture that these ingroup biases, by restricting freedom of choice along other lines, are in part responsible for the tolerance of academics about associating with colleagues regardless of religion, class origin, and ethnic background.

What generates these structural constraints on intergroup relations is that parameters are intersecting—that differences of one kind among people are not related or are only slightly related to differences of other kinds among them. *Intersecting parameters promote intergroup relations* (T-10). This theorem applies to the intersection among nominal, among graduated, and of graduated with nominal parameters, and it follows from the assumptions that proximity promotes social associations (A-1) and that the influences of parameters are partly additive (A-3). If differences in income are little related to differences in religion, education, and other social attributes, ingroup preferences in regard to these attributes prompt persons to associate with others whose income differs from theirs. The opposite is the case, however, if income differences are strongly related to differences in other social attributes. Under this condition, the ingroup preferences in regard to the other attributes lead—inadvertently, as it were—to disproportionate associations among persons whose income is similar too, reinforcing the independent effect of income on social associations. Accordingly, a theorem complementary to T-10 is implied by the same two assumptions (A-1 and A-3).

Strongly correlated parameters consolidate status and group differences and thereby impede intergroup relations (T-11). When the social differences delineated by various parameters largely coincide, their inhibiting effects on social intercourse reinforce one another. Individuals who differ on many salient social attributes do not have sufficient common interests to sustain extensive social associations, and differences in numerous social positions also reduce the likelihood that instrumental activities bring persons together. If parameters are consolidated, even individuals who do not have ingroup preferences in terms of a given parameter will mostly associate with the ingroup, owing to this parameter's correlations with other parameters in terms of which they do have ingroup preferences. This can be illustrated with the case of the academics mentioned. Tendencies to associate with persons in the same discipline lead to intergroup relations in terms of social background only if academic discipline is little related to background characteristics and colleagues in a discipline greatly vary in religion, class origin, and ethnic background. If academic discipline and background characteristics are strongly correlated, most colleagues in a

discipline have the same social background. In this situation, the tendency to associate with colleagues in one's discipline entails associating mostly with persons who also share one's religion, class origin, and ethnic background, even for individuals who have absolutely no ingroup preferences in terms of these background characteristics.

When parameters are intersecting, ingroup bias leads to intergroup relations; when they are consolidated, lack of ingroup bias leads to ingroup relations. Variations in structural conditions—the interrelations of parameters—determine what consequences given sociopsychological tendencies have for the social processes that integrate the various segments of society. The consolidation and intersection of parameters are polar opposites on the same continuum, indicated by the correlations of parameters. The stronger their positive correlation, the more consolidated parameters are. The weaker their positive correlation, the more parameters intersect.[20]

Intersecting parameters reflect a highly differentiated social structure, whereas consolidated parameters are indicative of a less complex structure with fewer independent lines of differentiation. At the same time, intersecting parameters further the integration of the different segments of society by promoting extensive associations among groups and strata. No assumption about value consensus or functional interdependence has been made to arrive at this conclusion. Functional interdependence typically entails much one-sided dependence and power inequality, which are more likely to impede than to foster social integration. The only substantive assumptions needed to derive the conclusion are that people associate more with others in proximate than with those in distant social positions (A-1) and that this is the case for every parameter (A-3). Although the assumptions stipulate merely proclivities for ingroup associations, nothing about intergroup associations, they suffice to deduce from them, jointly with analytic propositions defining structural properties, tendencies to engage in intergroup associations under specified structural conditions. Variations in structural constraints explain the influence that given (invariant) sociopsychological dispositions exert on the social processes that integrate the segments of society.

SUBSTRUCTURES

The components of complex social structures are themselves social structures. Societies are composed of communities, which have their own social

[20] Correlations of nominal parameters have no sign and thus cannot be negative. But correlations of graduated parameters can be negative, though it is empirically rare for various aspects of status to be negatively related. Negatively correlated graduated parameters are conceptualized as being still more intersecting than uncorrelated ones, because the structural constraints on social life of opposite status differences in two dimensions are similar to but even greater than those of unrelated status differences.

structure. The propositions advanced apply to the structure of communities as well as that of societies. But society is more than the sum of its communities. Society's structure comprises differences and connections among as well as within its substructures. Any form of differentiation in society is the result of the differentiation within its communities and the differentiation among them. For example, the income inequality in society is the product of the inequalities in income within communities and the income differences among communities. Society's ethnic heterogeneity is the result of some such heterogeneity within and some ethnic differences among communities. The consolidation (correlation) of education and income in society is produced partly by their correlations within communities and partly by the ecological correlation between mean education and mean income for communities.

Decomposition

The influence exerted on social associations by a given form of differentiation in society can be decomposed into the influence of the differentiation within communities and that of the differentiation among them. The question raised is not what influences various forms of differentiation within communities exert on social relations. This question has been partly answered in the preceding discussion, which applies to the structure of communities, as noted. The problem posed now is what relative significance differentiation within and differentiation among communities, respectively, have for society's intergroup relations and integration. Does the existing degree of heterogeneity or of inequality in society at large influence intergroup relations more if it occurs primarily within the various communities or if it is largely the result of differences among communities?

A corollary of the theorem previously advanced that propinquity increases the probability of social associations (T-5) is that most social associations take place in people's own communities (T-5.1). One would therefore surmise that intergroup relations are more affected by heterogeneity and inequality within communities, where most social associations occur, than by average group and status differences among communities. Since heterogeneity promotes intergroup relations (T-9), one would expect heterogeneity within communities to promote them more than society's heterogeneity that is largely the result of group differences among communities. Correspondingly, since inequality impedes interstratum associations (T-8), one would expect much inequality within communities to impede them more than society's inequality that is largely the result of great status differences among communities, with lesser inequalities within

them. Actually, only the first of these two expectations is correct; the second is false.

The more society's heterogeneity results from heterogeneity within rather than among communities, the more probable are both intergroup associations and associations among different communities (T-12). This theorem is deducible from T-5 (or T-9) and A-1. The propinquity of different groups (T-5) living in the same heterogeneous communities increases opportunities for and probabilities of intergroup relations. Besides, mostly within-community heterogeneity implies that the members of a group are dispersed among different communities and few of them are in the same community, which makes it more difficult to satisfy ingroup preferences (A-1) within one's community and encourages associations across communities to satisfy them. These tendencies will be most pronounced for members of small groups living in small communities, for instance, black professionals living in small towns. On the other hand, heterogeneity that exists mostly among communities entails greater homogeneity within communities, with different groups largely living in different communities. In this situation, physical (T-5) and social distances (A-1) tend to coincide and discourage both intergroup and intercommunity associations. In short, the segregation of groups in different communities counteracts the positive effect of heterogeneity on intergroup relations.

The more society's inequality results from inequality within rather than among communities, the more probable are social associations both among different strata and among different communities (T-13). This conclusion is quite unexpected, but it follows from the same premises as the preceding theorem. To be sure, great inequality makes social associations among different strata less likely, not more likely. The comparison in the theorem is not among communities that differ in inequality, however, but among societies that vary in regard to the extent to which their existing inequality is mostly owing to inequality within or mostly to that among communities. Still, one might have thought that inequality within communities would have greater adverse effects on social associations than would average status differences among communities. Yet this is wrong, as an illustration clearly shows. When poor and rich live in different communities, the inhibiting effects of status distance (A-1) and physical distance (T-5) on social associations reinforce each other. Mostly within-community inequality implies that different strata live in the same communities and that the same strata are dispersed among different communities. Under these conditions, persons from different strata are more likely to associate than when they live in different communities, owing to physical propinquity (T-5), and the common interests of persons in the same stratum who live in different communities promote some association among them, owing to status proximity (A-1.2). Although inequalities within commu-

nities inhibit social associations, they do not inhibit them as much as do inequalities that are reinforced by residential segregation among strata.

Surprisingly, society's inequality as well as its heterogeneity is more compatible with integrative social associations among its different segments when it exists primarily within communities than when it results mostly from the great differences among communities. Much differentiation of any kind within communities and little among them furthers social relations among the various segments of society. Indeed, a still broader generalization can be derived.

Penetrating Differentiation

Communities are not the only substructures into which a society can be divided. Place of work instead of place of residence can be the criterion of substructure, and one can partition the division of labor, for instance, into the occupational differences within and those among work organizations. As a matter of fact, any nominal parameter can be the criterion of substructure. Moreover, successive levels of subdivision can be taken into account: nation, province, town, neighborhood; or, organization, department, section, subunit; or, major occupational group, detailed occupation, specialty. The following proposition applies to any form of differentiation (consolidated as well as simple inequalities and heterogeneities), any type of substructure, on any level.

The penetration of differentiation into substructure promotes intergroup relations of all kinds, that is, it increases the probabilities of social associations among the differentiated groups and strata and among the substructures (T-14). This theorem is deduced from T-10, that intersecting parameters promote associations among groups and strata, since greater penetration into substructures entails more intersection of the parameters delineating differentiation with the parameter defining the substructures.[21] For example, if there is much income inequality within communities but little within the neighborhoods of the various communities, inequality penetrates less deeply and income intersects less with location than if there is also much income inequality within neighborhoods. Similarly, if great ethnic differences exist among major occupational groups and also among detailed occupations but not within detailed occupations, ethnic heterogeneity prenetrates less into the occupational structure and ethnic differences intersect less and are more correlated with occupational differences than if there is much ethnic variation within the detailed occupations. The

[21] The two preceding theorems, T-12 and T-13, are also alternatively deducible from T-10, inasmuch as differentiation within rather than among communities entails more intersection of the parameter delineating the differentiation with community location, which may be considered a nominal parameter.

greater internal inequality or heterogeneity fosters social associations among the different strata or groups, owing to their common position in the same substructures, and also among substructures, owing to the common status or group membership of many members of different substructures (according to A-1 and A-3, from which T-10 is derived, and which are the only assumptions required to deduce T-14). In short, the penetration of differentiation into substructures weakens the correlation of parameters, which promotes intergroup relations (T-10).

Penetrating differentiation exerts a centrifugal force on social relations that directs them outward and integrates the diverse segments in society. The social integration of a large population in a complex society cannot rest solely on some common values and some interdependence; it requires that the diverse groups and hierarchical strata are not isolated from one another but connected through social associations among their members. This is the reason that society's integration is here defined in terms of the extent of social associations among different groups and strata. The plausible sociopsychological assumption that people prefer ingroup (and proximate-status) to outgroup (and distant-status) associates seems to imply that large societies are necessarily fragmented into segments with few social relations among them. This inference is wrong, however, because it ignores the impact of structural conditions on social relations. When the multiple parameters characterizing social structures are taken into account, it becomes apparent that this very assumption has implications for intergroup relations. The extent of social relations among society's different segments depends on structural conditions. Specifically, the more structural parameters intersect, the more extensive are intergroup relations. Although the homogeneity of narrow social circles discourages intergroup relations, intersecting parameters disturb this homogeneity. Numerous strongly intersecting parameters imply that differentiation in various forms penetrates into the interstices of society, exerting structural constraints to engage in intergroup associations and thereby integrating the diverse segments of society.

CONCLUSIONS

This paper has outlined a theory of the structure of social associations which is elaborated elsewhere (Blau 1977). The foundation of the theory is a quantitative conception of social structure as the distributions of a population among social positions in a multidimensional space of positions. The axes in this space are termed "parameters," which distinguish either nominal positions, like sex and race, or status gradations, like education and income. The two generic forms of differentiation are heterogeneity, defined

by the distribution of people among nominal positions, and inequality, defined by their distribution in terms of a status criterion.

The substantive focus of the theory is on the influences exerted by structural conditions, particularly the relationships of parameters, on the rates of social associations among different groups and strata, inasmuch as social integration is conceived to rest on extensive social relations among the different segments of society. The theory constructed is a deductive one: 14 theorems and a number of corollaries have been derived—in strict logic, I believe—from the analytical propositions defining properties of social structure and three assumptions, which are synthetic propositions. The assumptions are: (1) social proximity promotes social associations; (2) social associations depend on opportunities for contacts; (3) the influences of parameters are partly additive. The role of these assumptions in the theory is as givens, equivalent to the role of assumption of maximizing utility in economic theory. The substantive focus is not on the significance of these assumptions but, given them, on the significance of variation in structural conditions for processes of social association and social integration.
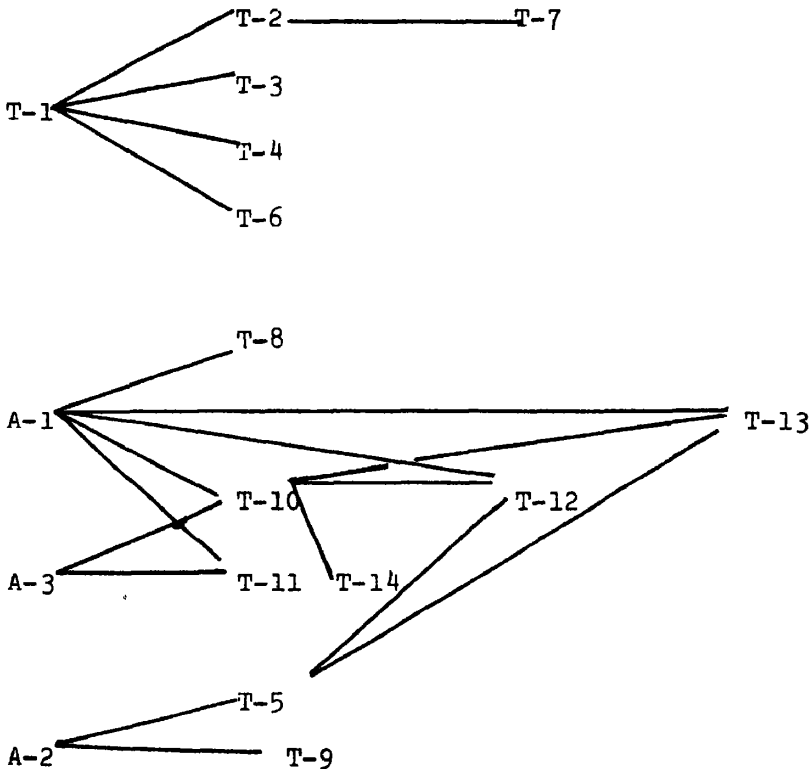
FIG. 1.—Chains of implications

The main chains of implications (not including definitions) are sketched in figure 1. To summarize the 14 theorems derived: (1) In the relation between two groups, the intergroup involvement of the smaller exceeds that of the larger group. (2) The probability of extensive intergroup relations decreases with increasing group size. (3) The more a majority discriminates against a minority in social intercourse, the smaller is the difference between the majority's and the minority's intergroup involvement. (4) As a majority's discrimination against a minority in social intercourse declines, the probable difference between the majority's higher and the minority's lower proportion of members who are insulated from intergroup associations increases. (5) Physical propinquity promotes social associations. (6) In a pyramidal structure, the rate of social associations of a higher with a lower stratum exceeds the rate of the lower with the higher stratum. (7) People in middle as well as in high strata in a pyramid are more likely to associate with others below them than with others above them in status. (8) Reductions in inequality diminish the impact of status on social associations. (9) Increasing heterogeneity promotes intergroup relations. (10) Intersecting parameters promote intergroup relations. (11) Consolidated parameters impede intergroup relations. (12) If society's heterogeneity results mostly from heterogeneity within rather than differences among communities, it promotes intergroup and intercommunity relations. (13) If society's inequality results mostly from inequality within rather than differences among communities, it promotes social associations among different strata and different communities. (14) The penetration of differentiation into substructures promotes social relations among various segments of society that integrate them.

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The Effects of Education as an Institution[1]

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Education is usually seen as affecting society by socializing individuals. Recently this view has been attacked with the argument that education is a system of allocation, conferring success on some and failure on others. The polemic has obscured some of the interesting implications of allocation theory for socialization theory and for research on the effects of education. But allocation theory, too, focuses on educational effects on individuals being processed. It turns out to be a special case of a more general macrosociological theory of the effects of education as a system of legitimation. Education restructures whole populations, creating and expanding elites and redefining the rights and obligations of members. The institutional effects of education as a legitimation system are explored. Comparative and experimental studies are suggested.

How does education affect society? The dominant view has it that the schools process individuals. They are organized networks of socializing experiences which prepare individuals to act in society. More direct macrosociological effects have been given little attention. Yet in modern societies education is a highly developed institution. It has a network of rules creating public classifications of persons and knowledge. It defines which individuals belong to these categories and possess the appropriate knowledge. And it defines which persons have access to valued positions in society. Education is a central element in the public biography of individuals, greatly affecting their life chances. It is also a central element in the table of organization of society, constructing competencies and helping create professions and professionals. Such an institution clearly has an impact on society over and above the immediate socializing experiences it offers the young.

Recently, the traditional socialization view has been attacked with an argument which incorporates a more institutional conception of education, though in a very limited way. Education is seen as an allocating institution—operating under societal rules which allow the schools to directly

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confer success and failure in society quite apart from any socializing effects (e.g., Collins 1971; Bowles and Gintis 1976). Allocation theory leaves open the possibility that expanded educational systems have few net effects on society. The polemic controversy has obscured the fact that allocation theory (and institutional theory in general) has many unexplored implications for socialization theory and research; those implications are considered here. For instance, allocation theory suggests effects of expanded educational institutions both on those who attend and those who do not attend schools. It also can explain why completing a given level of schooling often matters much more in determining educational outcomes than do the features of the particular school attended.

But conventional allocation theory, while considering the institutional properties of educational systems, focuses mainly on the outcomes for individuals being processed. It tends to be assumed that education has no effect on the distribution of political, economic, and social positions in society. Allocation theory is thus a limited special case of a more general institutional theory—legitimation theory—which treats education as both constructing or altering roles in society and authoritatively allocating personnel to these roles. Modern educational systems involve large-scale public classification systems, defining new roles and statuses for both elites and members. These classifications are new constructions in that the newly defined persons are expected (and entitled) to behave, and to be treated by others, in new ways. Not only new types of persons but also new competencies are authoritatively created. Such legitimating effects of education transcend the effects education may have on individuals being processed by the schools. The former effects transform the behavior of people in society quite independent of their own educational experience.

In this paper, I develop the ideas of legitimation theory and propose comparative and experimental studies which could examine the effects of education on social structure, not simply on the individuals it processes. I move away from the contemporary view of educational organization as a production system constructing elaborated individuals. Modern education is seen instead as a system of institutionalized rites transforming social roles through powerful initiation ceremonies and as an agent transforming society by creating new classes of personnel with new types of authoritative knowledge.

THE TRADITIONAL SOCIALIZATION MODEL

Prevailing research on school effects is organized around a simple image of socialization in society: Schools provide experiences which instill knowledge, skills, attitudes, and values in their students. These students then have a revised and expanded set of personal qualities enabling them to

demand more from, and achieve more in, the role structure of modern society. As the competence and orientation of the personnel of society are expanded and modernized, so society as a larger system is modernized and expanded.

Three general propositions are at issue here and make up a simple model, which is diagramed in figure 1:

> Proposition 1 (*Socialization*). Schooled persons are socialized to expanded levels of knowledge and competence and expanded levels of modern values or orientations.
>
> Proposition 2 (*Socialization and Adult Competence*). Early socialization to higher levels of knowledge, competence, and modern values or orientations creates higher levels of adult status and competence.
>
> Proposition 3 (*Individual Competence and Social Progress*). The expansion of the number of skilled adults expands the complexity and wealth of society and social institutions.

Research on proposition 1 is rather clear-cut. Children and youth in schools learn a good deal more, and acquire more expanded social capacities than those not in school, even when background factors are controlled (see, e.g., Holsinger 1974; Plant 1965). The main problem in the research on this subject is the finding that the particular school students attend often seems to make little difference (see Jencks et al. [1972]; or the studies reviewed in Feldman and Newcomb [1969]). I return to this issue below; the point here is that something about participation in schools creates notable effects on all sorts of socialization—from knowledge to social values to status expectations.

Little direct empirical research has been done on proposition 3—the idea that changed people produce a changed social structure—though this kind of "demographic" explanation (Stinchcombe 1968) has been a main theme of sociological theories of social change. In recent decades some doubts have arisen, with a conservative fear that "overeducated" people create more social instability and breakdown than they do social development. There is no evidence of this, but the issue remains.
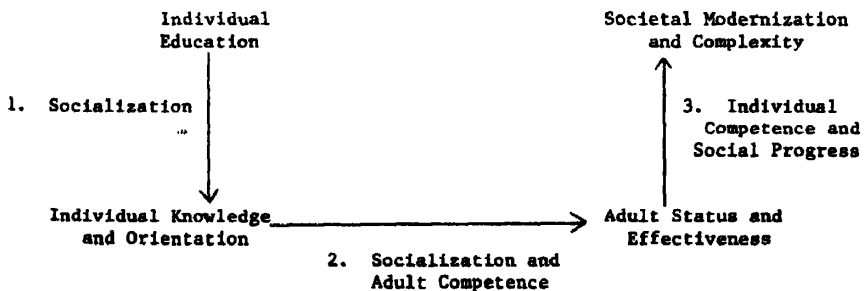
Fig. 1.—Traditional socialization theory

Proposition *2* has been one source of doubt about the whole model. Traditional socialization theory in sociology (and child development research) becomes an adequate account of social structure only if (*a*) socialized qualities remain with the person with some stability over long periods of time, and (*b*) such qualities predict adult effectiveness in roles. But current research on personal qualities often suggests low autocorrelations over time (see the review by Mischel [1971]). Many empirical studies suggest that the personal qualities schooling creates do not effectively determine occupational success, once occupational entry has been obtained (see the polemic review by Berg [1971]). Even if socialized qualities have fair stability and offer fair predictive power, it is unlikely that the product of these effects (which amounts to a very low overall effect) explains the high correlation of education with adult status.

Thus, socialization theory, as an account of educational effects on society, has one area of success and two of failure. On the positive side, schooling does predict, with other variables held constant, many of the outcomes of socialization. On the negative side, many of the measurable socialization outcomes of schooling have little long-run staying power or predictive power.[2] Also on the negative side, variations among schools in their socialization programs show small effects on outcomes—if schools socialize through the immediate experiences they provide, schools providing different experiences should produce very different effects. The research literature provides little encouragement on this subject.[3]

INSTITUTIONAL THEORIES: ALLOCATION THEORY AS A LIMITED CASE

Traditional socialization theory defines education as an organized set of socializing experiences. It treats as peripheral the fact that modern educational systems are society-wide and state-controlled institutions. In discussions of socialization theory this property of educational settings barely appears (e.g., Wheeler 1966).

Partly in reaction to this limitation, but more in reaction to the empirical weakness of socialization theory and in polemic reaction to the earlier optimism about the socially progressive effects of education, allocation theories have been developed. It is argued that people in modern

[2] Socialization researchers, of course, *continue to* pursue the grail, looking for new properties of individual socialization that are stable and that do effectively predict long-run success. The search has been going on for a long time.

[3] A number of ideas have been suggested in defense of traditional theory: (1) we have not yet found or measured the relevant aspects of school structure; (2) schools tend to be random collections of teachers and thus to appear alike even though teaching is of great importance; (3) on the relevant properties—normative commitment and organization, or simply the time devoted to various topics—most schools in a country are very similar and thus have similar effects. I pursue a related, but more general, line below.

societies are allocated to adult roles on the basis of years and types of education, apart from anything they have learned in schools. Education is thus more a selector, sorter, and allocator than it is a socializer.

Education, in allocation theories, is a set of institutional rules which legitimately classify and authoritatively allocate individuals to positions in society. Allocation theories are limited in that they define only a few consequences of this system and consider effects mainly on the individuals being allocated, but they open up a broader range of institutional theories which are discussed below.

The power of the allocation idea arises from its obvious empirical validity. We all know that status positions in modern societies are assigned on the basis of education. Sometimes, as with civil service and professional positions (e.g., medicine, law, teaching), this is a matter of law. To teach in a high school one must have an educational credential. Whether one knows anything or not is less relevant. Often, rules about credentials are simply part of established organizational practice, as in the assignment of college and business-school graduates to managerial positions and of others to working-class jobs. Sometimes the whole process is informal, as in the inclination of juries and informal friendship groups to attend to the advice of their more educated members.

In any event, the relationship between education and social position—over and above socialization or learning—is quite direct. The line of research pursued by Blau and Duncan (1967) and Duncan, Featherman, and Duncan (1972) shows large direct effects of education on status attainment, sometimes with ability measures held constant. Education plays a direct causal role in occupational transition even late in the individual's career (Blau and Duncan 1967, chap. 5)—decades after any direct socialization effects must have decayed or become outmoded.

The basic idea is clear:

> Proposition 4 (*Educational Allocation*). In modern societies, adult success is assigned to persons on the basis of duration and type of education, holding constant what they may have learned in school.

Educational allocation rules, that is, give to the schools social *charters* to define people as graduates and as therefore possessing distinctive rights and capacities in society (Meyer 1970*a*; see also Clark 1970). Thus the schools have power as an institutional system, not simply as a set of organizations processing individuals.

Impact of Allocation Rules on Socialization

The polemic contrast between socialization and allocation ideas—education as a socializing process versus education as a status competition—has concealed the fact that the two are not really inconsistent. Further,

allocation theory offers interesting and useful extensions of traditional socialization ideas.

Assume that educational allocation rules in fact hold in society. Students and members of their social networks (e.g., parents, peers, teachers, and counselors) are informed members of society—not simply passive objects of educational production—and know these rules with some accuracy. Graduates, of course, experience the rules through the distinctive experiences and treatments they receive in society. Now if we assume a most elementary idea of social psychology, that people adapt and are adapted by others to their actual and expected experiences, two major propositions follow:

> Proposition 5 (*Chartering*). Students tend to adopt personal and social qualities appropriate to the positions to which their schools are chartered to assign them.
>
> Proposition 6 (*Lagged Socialization*). Adults tend to adopt qualities appropriate to the roles and expectations to which their educational statuses have assigned them.

These propositions argue that education functions for individuals as a set of initiation ceremonies of great and society-wide significance (Ramirez 1975; Garfinkel 1956). These ceremonies transform the futures and pasts of individuals, greatly enhancing their value in all sorts of social situations. On the basis of their education, individuals are expected to treat themselves, and others are expected to treat them, as having expanded rights and competencies. Given allocation rules, educational labels are of the greatest significance for the social identity of individuals.

Proposition 1 and proposition 5 parallel each other and in many instances overlap in accounting for the same findings. It is often unclear to what extent given socialization effects are generated by the immediate socializing situation in a given school and to what extent they are produced by the institutional authority in which the school is embedded.

However, proposition 5, in contrast to proposition 1, offers a direct explanation of the most puzzling general research paradox in the sociology of American education. The level of schooling achieved has substantial effects on all sorts of personal qualities. But outcome variations among schools—even though these schools differ greatly in structure and resources—are very small. This finding shows up in studies of college effects (Feldman and Newcomb 1969), high school effects, and effects at the elementary school level. If schools have their socializing effects as ritually chartered organizations (Meyer 1970*a*; Kamens 1971, 1974) rather than as organized collections of immediate socializing experiences, then all schools of similar ritual status can be expected to have similar effects. Since for many personnel assignment purposes all American high schools (or colleges) have similar status rights, variations in their effects should be small.

But because all high schools are chartered to create "high school graduates"—a critical status in our society for college and occupational entry—all of them tend to produce marked effects on students. Proposition 5, in other words, argues that the most powerful socializing property of a school is its external institutional authority, derived from the rules of educational allocation, rather than its network of internal socializing experiences. Educators, who attend with great vigor to the accreditation of their schools, seem more aware of this process than do socialization researchers.

Thus, the educational contexts which vary substantially in the change and learning they produce in students do not usually include specific schools. They include contexts which are distinctively chartered:

1. Schooling per se. Life prospects (and hence changes in students) are vitally affected by being in an institution chartered as a school.

2. Type of school, when the types are differently chartered. Himmelweit and Swift (1969) and Kerckhoff (1975) show marked differences in outcomes for similar British students between grammar and secondary modern schools. American researchers have not looked for differences in expectations between initially similar students in general and vocational high schools. Some studies show distinct occupational effects of teachers, colleges, and engineering schools (Astin and Panos 1969).

3. Curriculum, when it is distinctively chartered. For instance, being in a college preparatory curriculum (in contrast to a vocational one) makes a considerable difference in the aspirations and expectations of American high school students (Alexander and Eckland 1975; see also Rosenbaum 1975).[4]

Proposition 6—the idea that education socializes adults by allocating them to expanded roles and role expectations—explains a second major paradoxical finding in the current sociology of education. The direct long-run effects of schools on graduates are thought to be rather moderate. But surveys of adults with regard to almost any dependent variable—attitudes, values, information, or participation—almost uniformly show that education plays a dominant role. For instance, Almond and Verba (1963) show with data on five countries that education is closely associated with political information, attitudes, and participation. Inkeles and Smith (1974) show the same result with data on six countries and are surprised to discover that the impact of education is much greater than that of work experience. Kohn's research (1969, and subsequently) shows exactly the same result, and again the author is surprised. But these findings make eminent sense. Educational allocation rules create a situation in which schooling is a fixed capital asset in the career of the indi-

[4] Intervening variables in all these effects would include the expectations of the students and those of their parents, teachers, counselors, and peers.

vidual, more durable than work or income, more stable than family life and relations, and less subject to market fluctuations than "real" property. Is it surprising that the attitudes and orientations of educated individuals continue to reflect such enhanced life prospects over long periods of time? They perceive these prospects and are surrounded by others who see them too.

Proposition 6 suggests that in explaining such long-run effects of education we do not need to look back to the details of the experience of socialization. Correlations between education and personal qualities can be maintained and increased by a structure or subsequent allocation which provides distinctive life experiences and anticipations for the educated. For instance, education can affect a person's sense of political efficacy by making him politically influential as well as by socializing him to a civic culture.

Further Implications of Allocation Theory

If taken seriously, and not simply used as a cynical critique of education, allocation theory would completely reorganize current research styles in the sociology of education. Allocation rules, unlike simple socialization effects, reign over both the students and the nonstudents, the educated and the uneducated, the graduates and those who never attended.

Research implication 1: effects on nonstudents.—Let us examine the following hypotheses.

> Hypothesis 1. The creation of social rules allocating status and competence to graduates leads to the socialization of students for expanded social roles.
>
> Hypothesis 2. But such rules *lower* the prospects of nonstudents, and in a sense desocialize them.

The more binding the allocation rules, the earlier and more convincingly are nonstudents committed to passive roles in society. This means that the society relying on credentials could well lower (below the previous floor) the modern competence of people of low education. Comparative contextual research is required to test this idea, since the independent variable is a property of the social system.

This argument has it that in a modern society education allocates its dropouts to failure. They (and their parents and friends) anticipate and adapt to this.

> Hypothesis 3. Similarly, *subsequent* to the period of schooling, nongraduates are socialized through life experiences to the meaning of their failure just as graduates are socialized to the meaning of their success. The lagged differentials created by education should be greater the more firmly the principle of educational allocation is established.
>
> Hypothesis 4. Those admitted to chartered educational organizations find

> their prospects enhanced even before attendance, while those rejected find their prospects lowered. They adapt their personal qualities in anticipation, even prior to attendance. These differentials should be greater the stronger the allocative position of the school.

For example, Benitez (1973) finds that students admitted to a national elite high school in the Philippines seem to gain in self-esteem and "competence" even before their socialization begins. Wallace's (1966) data suggest a similar interpretation.

Comparative research on effects such as these should help distinguish allocation theory from traditional socialization ideas.

Research implication 2: aggregate effects.—A major implication of allocation theory is that inferences to the aggregate effects of education made from individual data on the basis of traditional socialization ideas are almost completely illegitimate. Researchers in the economics of education conventionally infer aggregate economic effects of education from income differentials between the educated and the less educated (see, e.g., the papers in Blaug 1968, 1969). It is assumed that these income differentials reflect real added value—the socialization gains of the educated. But if education is simply an allocation system, the gains of the educated may simply occur with equivalent losses for the uneducated. The expansion of education and educational allocation may have no effect on the aggregate product at all (Collins 1971).

Similarly, researchers on the political effects of education often infer that, because the educated occupy politically central positions, education must have helped create these positions (see the papers in Coleman 1965). But if education is simply a system of allocation, huge positional and attitudinal differences between the educated and the uneducated may exist with no aggregate effect at all. Igra (1976), in fact, shows (using Inkeles's data) that increases in the aggregate development of societies *lower* the political participation of individuals of given education (though the political information of individuals is found to be enhanced). Such "frog-pond effects" at lower levels of analysis are discussed by Davis (1966), Meyer (1970*b*), and Alexander and Eckland (1975).

The main arguments of allocation theory are added to those of socialization theory in figure 2. Allocation ideas are discussed with some frequency in the current literature, though their implications for research remain little explored.

The Limitations of Allocation Theory

Allocation theories, by conceiving of education as an institution, add a good deal to traditional socialization theory. But they do so in a very narrow way.
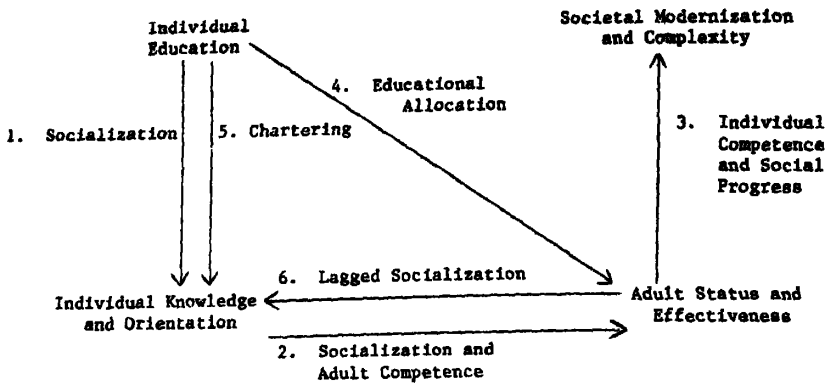
FIG. 2.—Allocation theory and its implications for socialization theory. (Pure allocation theory suggests that *3* is irrelevant. If a given set of adult competencies are simply allocated by education, no net societal gain in number of competent individuals need occur.)

Education is seen in these theories as possessing its power because it is built into the rules and understandings which guide all sorts of personnel allocation processes in society. But its impact is considered only for those individuals being processed by the system—the students and nonstudents who are being sorted. And even this impact is defined in a limited way: these people are understood to respond only to their own role prospects as they are affected by education. Does the fact that all the other individuals around him are being magically transformed by powerful initiation ceremonies have no effect on a given student? And has it no effect on other members of society?

The problem here is that allocation theories ordinarily see education as allocating individuals to a fixed set of positions in society: a distribution of positions determined by other economic and political forces. Bowles and Gintis (1976) propose slight additional effects—education is thought to socialize people to accept as legitimate the limited roles to which they are allocated. Spence (1973) and Thurow (1975) see some marginal gains to society through more efficient selection by education. But the main development of allocation theory defines the *structure* of society as little affected by education.

Allocation theory, then, can be seen as a special case of a more general argument according to which education constructs and alters the network of positions in society in addition to allocating individuals to these positions. We simply need to abandon the assumption that the positions to which education allocates people cannot be built, expanded, and altered by education itself.

It is becoming more common to speak of education as legitimating the structure of modern society (Bowles and Gintis 1976), or of modern so-

cieties as in some essential way "schooled" (Illich 1971). If we want to understand the societal impact of education, not just its effects on the careers of individuals, we need to understand what this means.

THE GENERAL CASE: LEGITIMATION THEORY

Allocation theory is a special case of institutional theories of educational effects: it considers the effects of education as an institution (*a*) only on the individuals being processed and (*b*) with the structure of society held constant. We now turn to the general case: theories of the institutional impact of education on social structure itself—on the behavior of people throughout society.

Modern extended and institutionalized systems of education build into society certain rules which actors take for granted, know others take for granted, and incorporate in their decisions and actions.[5] For instance, institutionalized educational systems create a situation in which social gatekeepers (e.g., personnel officers)—even if they read and believe Ivar Berg's book—nevertheless know that they must hire people on the basis of educational credentials.

Two closely related aspects of modern educational systems are relevant here as independent variables: (1) they are extended as systems of classification, categorizing entire adult populations by level and specialty; and (2) they are institutionalized, with their classifications often controlled by the state and enforced in daily life by rules about credentials written into law and applied in organizational practice. Almost everywhere, education is made compulsory and universal by national law, often in the national constitution (Boli-Bennett 1976). In most countries its structure is closely regulated by the nation-state (Ramirez 1973; Rubinson 1973).

Why does this occur? Whatever the economic origins of the process, the fact that it is usually accomplished and regulated by the state—unlike many aspects of economic development, which are left to individuals and subunits—suggests that its immediate origins lie in the political system: society as corporate organization (Swanson 1971) rather than as a system of exchange. *Formalized educational systems are, in fact, theories of socialization institutionalized as rules at the collective level.* The three core propositions used above to summarize traditional socialization theory *become* the structural basis of the educational system. Proposition 1 —the idea that the schools teach critical skills and values—becomes institutionalized as the basic educational classification system: Education pro-

[5] Actors may also internalize these rules as personal commitments, but this is less important—the critical aspect is that they internalize them as social facts and social realities (institutions which rely on personal beliefs, or even permit the question of personal beliefs to be relevant in social action, are less highly legitimated in important senses than are those which operate as realities).

ceeds in a sequence (irreversible by ascriptive definition) from kindergarten through postdoctoral study and covers a defined series of valued substantive topics. The student is a "high school graduate" and has had compulsory units of history and English and mathematics. It is an institutionalized doctrine, since for many purposes one must treat the student as having acquired this knowledge by virtue of the units or credits completed, not by direct inspection. Proposition 2—the idea that schooled qualities are carried into adult effectiveness—is institutionalized in the basic rules for employing credentialled persons which dominate personnel allocation in modern society. If one hires an executive, a civil servant, or a teacher one must inspect educational credentials—it is optional whether one inspects the person's competence. A teacher or a doctor who graduated from school in 1930 is still frequently treated as a socially and legally valid teacher or doctor. Proposition 3—the idea that educational allocation creates social progress—is institutionally embedded in our doctrines of progress: it consists of modernity, professionalization, and rationalization. The possession of the best certified and educated people is a main index of the advanced status of a hospital, a school, often a business organization, and indeed a society itself.

Educational systems themselves are thus, in a sense, ideologies. They rationalize in modern terms and remove from sacred and primordial explanations the nature and organization of personnel and knowledge in modern society. They are, presumably, the effects of the reorganization of modern society around secular individualism which is a main theme of Marx and Weber. Our problem here, however, is to discuss their effects.[6]

Legitimating Effects of Expanded and Institutionalized Education

Legitimating effects of education can be discussed in four general categories created by the intersection of two dichotomies. First, education functions in society as a legitimating *theory of knowledge* defining certain types of knowledge as extant and as authoritative. It also functions as a *theory of personnel*, defining categories of persons who are to be treated as possessing these bodies of knowledge and forms of authority.

Second, education validates both *elites* and *citizens*. Discussions of the legitimating function of education often emphasize only its role in supporting elites and inequality (e.g., Bowles and Gintis 1976; Carnoy 1972). But the overwhelmingly dominant kind of education in the modern world

[6] The discussion which follows deals exclusively with the *effects* of institutionalized education on other aspects of society. Obviously, important causal effects also run the other way (see Meyer and Rubinson [1975] for a review). Empirically disentangling the reciprocal effects requires data on societies over time.

	Elite Education	Mass Education
Education as Theory of Knowledge	1. The Authority of Specialized Competence	3. The Universality of Collective Reality
Education as Theory of Personnel	2. Elite Definition and Certification	4. The Extension of Membership: Nation-building and Citizenship

FIG. 3.—Types of legitimating effects of education

is mass education (Coombs 1968), closely tied to the modern state and notion of universal citizenship (Marshall 1948; Bendix 1964; Habermas 1962).

These two distinctions define four types of legitimating effects of education, as specified in figure 3. I discuss them in turn.

1. The Authority of Specialized Competence. Education does not simply allocate people to a fixed set of positions in society. It expands the authoritative culture and the set of specialized social positions entailed by this culture. Thus the creation of academic economics means that new types of knowledge must be taken into account by responsible actors. The creation of psychiatry means that former mysteries must now be dealt with in the social organization. The creation of academic programs in business management brings arenas of decision making from personal judgment, or luck, to the jurisdiction of rationalized knowledge. Social problems call for human-relations professionals (occasionally even sociologists). Safety or environmental problems call for industrial or environmental engineering.

The point here is that, quite apart from the immediate efficacy of these bodies of knowledge, they are authoritative and must be taken into account by actors at the risk of being judged negligent or irrational. The business manager who plans by the seat of his pants—unblessed by economic projections—has no excuse for ignoring the best advice. The political leader who sees social problems as beyond analysis or cure is reactionary and primeval. The emotionally disturbed person who rejects psychiatry is displaying irrationality.

Thus the knowledge categories of the educational system enter authoritatively into daily life. Mysteries are rationalized, brought under symbolic control, and incorporated into the social system. Society and its subunits are buffered from uncertainty (Thompson 1967):

Proposition 7. The expansion (and institutionalization) of education expands the number of functions that are brought under social control and that responsible actors must take into account.[7]

2. *Elite Definition and Certification.* Education as an institution creates and defines particular categories of elite personnel. This has two aspects. (*a*) Education consists of allocation rules and initiation ceremonies designating which persons possess the authority and competence for various elite roles. This is the core idea of allocation theory. (*b*) But institutionalized education also defines the nature and authority of the elite roles themselves—helping to create the categories of personnel as well as to designate the particular occupants of these categories. In this way, expanded modern educational systems function as a personnel theory in society, justifying in modern cultural terms the expansion and specialization of modern elites.

Education, that is, not only creates "economic knowledge" which must be taken into account by rational actors. It is also a structure helping to create the role of economist, to justify economists' authority claims in society, and to define precisely who is an economist. Education thus creates, not only psychiatry, but psychiatrists; not only modern management ideology, but M.B.A.'s. The rational actor must take into account medical knowledge, and to do so he must consult a doctor. Thus, the modern organizational structure of society incorporates legitimated bodies of knowledge by incorporating the designated personnel.[8]

We take too narrow a view if we see this process as involving only a few specialized occupations. The most important rules concerning credentials are more general: the set of rules which connect the educational status of *college graduate* (and *high school graduate*) with all sorts of formal and informal elite positions. These rules define a generalized body of elite knowledge and specify its legitimate carriers.

It now becomes clear why views of educational allocation as "zero-sum" —allocating a fixed set of social statuses—are wrong. Education helps *create* new classes of knowledge and personnel which then come to be incorporated in society:

[7] This assertion, incidentally, parallels an idea of Schumpeter (1950, chap. 12) about the way in which the intellectual optimism of modern capitalistic society generates its own institutionalization and destruction. The intellectuals rationalize more and more social functions, which are then brought under collective social and political control and removed from the market.

[8] Imagine, for example, the consequences that would flow from the rise of routinely accredited university programs and degrees in astrology. Organizations would incorporate astrologers, the state would fund their programs and consult or incorporate them. Of course a justificatory literature would grow. The same basic processes have gone on with many occupational groups.

Proposition 8. The expansion (and institutionalization) of education expands the number of specialized and elite positions in society. It defines and justifies their occupancy by particular people.[9]

The point here is that institutionalized education does more than simply allocate some to success and others to failure. The educated learn to claim specialized functions and to legitimate the specialized functions of others. The less educated learn that they are part of a social world of rights and duties elaborated far beyond the traditional community. This is one of the core meanings of the modern social status *citizen*.

3. *The Universality of Collective Reality.* Mass education creates a whole series of social assumptions about the common culture of society and thus expands the social meaning of citizenship, personhood, and individuality (modern ideas, all). It establishes a whole series of common elements for everyone.[10] (*a*) It creates the assumption of a national language or languages and defines universal literacy. (*b*) It reifies a given national history. (*c*) It constructs a common civic order—common heroes and villains, a common constitutional and political order with some shared cultural symbols and with legitimate national participation. (*d*) It validates the existence of a common natural reality through science and a common logical structure through mathematics and in this way constructs a myth of a common culture intimately linked to world society. (*e*) It constructs broad definitions of citizen and human rights as part of the modern world view.

Regardless of what people actually learn in school about their language and culture, nationally institutionalized mass education creates the assumptions of a national culture. For many purposes, both elite and citizen actors must take them into account:

Proposition 9. The expansion (and institutionalization) of education expands the content and jurisdiction of the elements taken for granted as part of collective reality.

4. *The Extension of Membership: Nation-Building and Citizenship.*

[9] This proposition is impossible in conceptions of social status as simply a rank position and thus as fixed in sum. But there is no reason to assume that the total amount of status (or for that matter power) in society is fixed. Independent of their ranks, statuses (and whole status distributions) may vary in the expansion of their substantive rights and powers. I have argued above that education expands the status rights attached to many positions in society, without necessarily altering the rank structure. This conception of status reflects Weber's original formulation.

[10] I provide here a conventional list of the putative effects of mass education. But my argument is that, actual effects aside, they enter into social life as taken for granted assumptions. Many Americans are not literate in the national language. But we treat each other, expect elites to treat us, and organize our public life as if we all were. According to proposition 5, the existence of these effects as social assumptions greatly increases the likelihood that the schools actually produce them.

Beyond defining and extending national culture, mass education defines almost the entire population as possessing this culture, as imbued with its meanings, and as having the rights implied by it. Mass education defines and builds the nation (Marshall 1948; Bendix 1964). It allocates persons to citizenship—establishing their membership in the nation over and above various subgroups. And it directly expands the definition of what citizenship and the nation mean and what obligations and rights are involved. Mass education helps create a public: as education expands, ideas about public opinion as a vital force in society rise (Habermas 1962; Bergesen 1977). Individuals come to be defined as possessing the competencies and the moral orientations to participate in an expanded collective life:

> Proposition 10. Mass education expands the number of persons seen as possessing human and citizenship responsibilities, capacities, and rights. It also expands the prevailing definitions of these roles and their associated qualities.

In expanding both the meaning of citizenship and the set of persons who are seen as citizens, education plays a dual role. Certainly it opens up new possibilities for citizens—in particular, new claims for equality which can be made on society. It also, however, redefines individuals as responsible subordinate members (and agents) of the state organization, and opens them to new avenues of control and manipulation.

Research Designs in Legitimation Theory

Legitimation ideas propose societal effects of education. They can be studied in several ways.

1. Most directly, data comparing societies over time can be examined. For instance, is it true, in comparing societies, that those with expanded mass education tend to create sooner and more completely the welfare, policing, and participatory apparatuses of citizenship?

2. The same questions can be looked at with time-series data pertaining to a single society. For example, what has been the effect of the expansion of higher education in the United States, independent of other factors, on the number of types of professionals who have privileged status (as "expert witnesses") in the courts?

3. The same questions can be studied at the individual level as well. Legitimation theory argues for the effects of the extension and institutionalization of national educational systems on the judgments, perceived realities, and actions of *given* individuals—ordinary persons, rule makers, and critical social gatekeepers. Studies can therefore compare similar individuals in societies differing in educational structure. Do persons of given education, in more schooled societies, see personal and social problems as more likely to require educated expertise? Do they see, as I ar-

gued above, a larger number of social functions as requiring explicit (and undoubtedly educated) collective social management? Comparative survey research can help examine such questions.

4. It is also possible to approach these questions experimentally. Education, it is argued, restructures social reality for given individuals. To explore this, subjects can be confronted with hypothetical societies, similar in many respects but differing in the expansion and authority of education. Would subjects be more likely to propose to use economists and other social scientists to help with business or political planning if we describe for them a society in which elite education is highly developed and institutionalized? Subjects might even attribute authority to nonexistent professions if those are described as rooted in educational programs.

These research design approaches can all be used to deal with the following central empirical hypotheses of legitimation theory.

1. Basing a particular elite in the educational system helps create and expand its authority. One can study empirically the differential rise in societies of personnel workers, social scientists, physicians, or psychiatrists as these groups are affected by differential educational institutionalization. This can be done with comparative, survey, or experimental techniques.

2. More generally, expanded elite educational systems produce and support *more* and larger elites with jurisdiction over more social functions. We can test this hypothesis by seeing whether more problems requiring collective action are defined in societies with expanded elite education, and by seeing whether the management of such problems is more likely to be reserved to educated elites in such societies.

3. Mass education expands the national culture. Both elites and masses, in societies with more mass education, should be more likely to perceive widespread literacy, attention to public problems, information, and involvement. This should hold true even when the actual levels of these variables are held constant. Mass education is an institution, and like all institutions creates forms of pluralistic ignorance: it supports the widespread social assumption of an informed and attentive public. In expanding the national culture, mass education also creates and expands the assumption of homogeneity. In societies with more mass education, both masses and elites should be found to perceive more common interests and ideas in the population and less conflict and diversity. This should hold even when actual diversity is held constant.

4. Mass education, similarly, expands citizenship, both in size and content. Elites, in societies with more mass education, should be found to perceive masses as making more demands, having more rights, and posing more threats than in other societies. Elites planning new regimes in such societies should be found to employ more strategies of control through mobilization rather than through traditional authoritarianism. They should

also attend to the creation and manipulation of "public opinion." Mass education may be one of the elements supporting the modern "activist" version of the classic military coup and regime. Again, one can study such a process comparatively, with survey data, or experimentally (presenting subjects with hypothetical societies).

These research suggestions make clear the nature of legitimation effects: Modern educational systems formally reconstruct, reorganize, and expand the socially defined categories of personnel and of knowledge in society. They expand and rationalize the social realities that enter into the choices of the socialized and the unsocialized, the allocated and the unallocated. Education is, as has often been noted, a secular religion in modern societies: as religions do, it provides a legitimating account of the competence of citizens, the authority of elites, and the sources of the adequacy of the social system to maintain itself in the face of uncertainty.[11]

The Impact of Educational Legitimation on Allocation and Socialization

The socializing impact of education as an institution is discussed above (propositions 5 and 6) in the review of the allocation theory version of the larger idea of legitimation. The intervening discussions make necessary two extensions in the arguments presented there:

1. It is now clear that rules of educational allocation are not simply arbitrary social constructions which happen to have power over people. These rules are part of the basic institutional ideology of modern society: they represent equity, progress, and technical sophistication. As part of a larger institutional system, that is, the rules of educational allocation are highly *legitimate*, not merely instances of the exercise of power. This legitimacy intensifies the operation of rules of educational allocation, and intensifies the effects of these rules on individuals being socialized and allocated:

> Proposition 11. The more institutionalized the modern system of education, the more intensified the causal relationships of allocation and socialization.

[11] Modern education not only expands each society structurally; it also brings societies into closer organizational similarity with each other. Societies come to be made up of more and more similar elites—often in professional communication with each other—and masses with more and more shared social rights. This organizational homogeneity means that information—and exploitation—can proceed very rapidly. New ideas and techniques are not *alien*. They are the stock-in-trade of an already incorporated profession and can thus be adopted with less resistance. So, in the modern world, the presence of locally controlled, but organizationally similar, educational systems in almost all nation-states makes possible the rapid cultural penetration of techniques (and political revolution). And it makes possible new kinds of dependence (e.g., a "brain drain").

Educational allocation rules become more common, and their socializing consequences increase in intensity, under conditions of high educational institutionalization. The lagged socialization of the allocated (proposition 6) becomes, not simply an adaptation to their increased power, but an affirmation of their authority and an account of a legitimate moral biography. Similarly, the process by which students acquire chartered qualities (proposition 5) takes on additional meaning because of its legitimacy. Students and nonstudents are learning more than their own futures. They are also learning that the practical categories and topics of education give legitimate meaning to these futures (see also Bowles and Gintis 1976). For instance, the college student learns a little sociology because he is taught it (traditional socialization) and because he knows graduates may be expected to know a bit about it (chartering). Both processes are intensified by the legitimating reality of sociology: students (and nonstudents) learn that it *exists* as a body of knowledge and a personnel category, entirely over and above their personal acceptance of the utility of the field. Thus students acquire their sociology with a dutiful passivity which reflects the understanding that whether or not they accept this discipline their degrees—valid throughout society—will reflect so many units of sociology.

Thus the objectified moral authority of the schools—over and above their raw power—undoubtedly intensifies socialization over and above that found in routine *training* organizations (Bidwell and Vreeland 1963).

2. In broader versions of institutional theory than allocation ideas, the effects of education are no longer fixed in sum: education may expand and alter the role structure of society. This means that there is no reason to believe that the socializing effects of allocation rules would be fixed in sum as is implied by hypotheses 1–3 above, in "Further Implications of Allocation Theory." If education expands the status order, anticipatory gains for students and their socialization consequences do not need to be balanced by losses for nonstudents. The new roles being created can simply be added to the status structure and to the socialization process. More commonly, the creation of a given new elite role also creates expanded rights and duties for others. Thus the expansion of medical authority in modern societies involves creating and expanding the role of doctor. But other people do not simply become nondoctors—they become patients. Education expands roles and sets them into proper relation with the rest of the society.

Once institutionalized education is seen as a legitimating system—not just a mechanism for allocating fixed opportunities—it can have many net consequences on both allocation and socialization of people being processed, just as on the rest of society.

The Impact of Educational Legitimation on Educational Organizations

The legitimating effect of educational organizations—but also much of their socializing and allocating power—is derived from their highly institutionalized status in society. Operating at the institutional level as an authoritative theory of personnel and knowledge in society, the schools constitute a crucial ritual system: a system of initiation ceremonies (personnel) and of classifications of information (knowledge).

This makes it clear why schools often seem to act as ritual organizations, sacrificing "effectiveness" for classificatory rigidity (Meyer and Rowan 1975; Kamens 1977). Their larger social effectiveness (and their claim to resources) inheres precisely in this ritual structure: the apparatus of classes and levels and degrees and subjects. By emphasizing their formal ritual structures, schools maximize their links to their main source of authority, their main resources, and quite possibly their main effectiveness. Dramatizing their structures as socially legitimated and legitimating initiation ceremonies informs students (and others) both about the payoffs to which they can adapt and about the fact that those payoffs are highly proper, deriving from the core meaning and values of society (Clark 1970). Ritualism, thus, by the process stated in proposition 11, reinforces the immediate effectiveness of schools in dealing with students.

Summary of legitimation theory.—Legitimation theory suggests two general ideas concerning the effects of schooling. First, institutionalized education, as a theory of personnel and knowledge, affects society directly, apart from the training and allocation of students. Second, institutionalized education creates and intensifies the individual effects of socialization and allocation. In figure 4 these two main themes are added to the explanatory structure presented earlier.

CONCLUSION

Schools may teach people useful skills and values. Whether they do or not in particular cases, they certainly *allocate* people to positions of higher social status, and this affects the anticipations and socialization of the students (and nonstudents) as well as the experience and later socialization of the graduates (and nongraduates).

The allocating power of the schools is one aspect of their status as social institutions creating and validating categories of personnel and knowledge. The schools increase the number and legitimacy of these categories—far beyond levels possible with more primordial myths of the origins of personnel and knowledge—and thus expand the whole rationalized
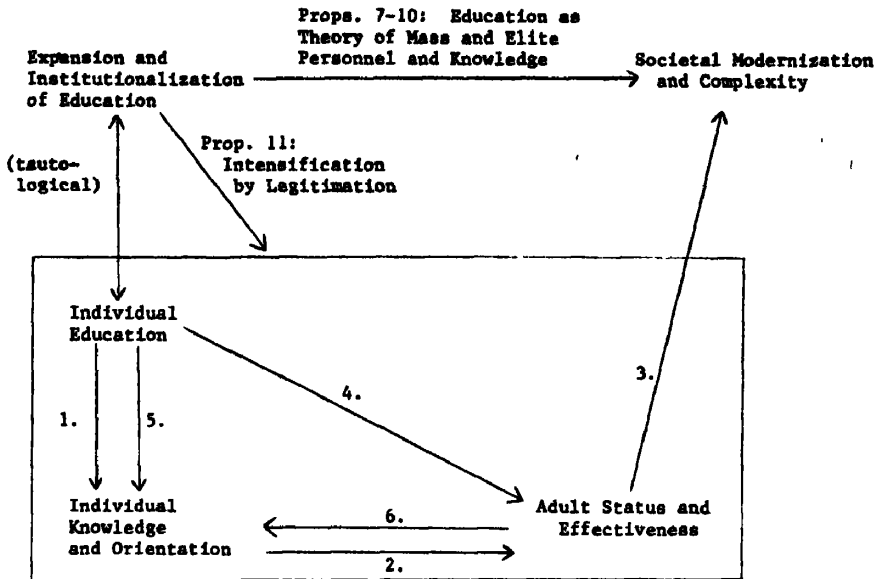
FIG. 4.—Legitimation effects of education

modern social structure. These legitimating effects of schools reconstruct reality for everyone—the schooled and the nonschooled alike. They also intensify the effects of allocating and socializing processes.

So a student is in a position of experiencing (*a*) the immediate socializing organization, (*b*) the fact that this organization has the allocating power to confer status on him, and (*c*) the broader fact that this allocation power has the highest level of legitimacy in society. The education he receives has a very special status and authority: its levels and content categories have the power to redefine him legitimately in the eyes of everyone around him and thus take on overwhelming ceremonial significance.

Research on such questions must examine the effects of education *as an institution,* considering effects of variables quite beyond the level of the classroom, the peer group, or the school as an organization. Either experimentally or with cross-societal (or time series) analyses, we need to consider the contextual effects of variations in the extension and institutionalization of education on the perspectives of students and nonstudents, graduates and nongraduates, citizens and elites. If education is a myth in modern society it is a powerful one. The effects of myths inhere, not in the fact that individuals believe them, but in the fact that they "know" everyone else does, and thus that "for all practical purposes" the myths are true. We may all gossip privately about the uselessness of education, but in hiring and promoting, in consulting the various magi of our time,

and in ordering our lives around contemporary rationality, we carry out our parts in a drama in which education is authority.

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Revolution and the Rebirth of Inequality: A Theory of Stratification in Postrevolutionary Society[1]

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This paper offers a theory and an explicit mathematical model of a peasant revolution's effects on inequality and status inheritance. We predict that, when an exploited peasantry revolts and overthrows the traditional elite, (1) in the short run, peasants are better off, and both inequality and status inheritance decline. But human capital becomes more valuable, and revolution does not benefit the poorest of its supporters as much as those who were better off before. (2) In the long run, peasants still benefit. But revolution proves new opportunities for those with education, ability, luck, or other resources; therefore economic inequality, educational inequality, and status inheritance grow steadily among the peasantry. In many circumstances, inequality and status inheritance will also grow in the society as a whole and may eventually exceed their prerevolutionary levels. The theory applies not only to revolutions but also to the long-run effects of any economic or social change that reduces exploitation or increases economic opportunities.

> Humanity left to its own does not necessarily re-establish capitalism, but it does re-establish inequality. The forces tending toward the creation of new classes are powerful.
>
> [MAO TSE-TUNG, 1965]

Probably the most shattering and dramatic transformation of human society is the violent overthrow of traditional elites by a revolution of the oppressed masses. Most such revolutions have occurred in the mainly rural, peasant-dominated societies in which the majority of mankind has lived. Local landlords have been dispossessed and chiefs deposed ever since exploitative governments arose in advanced agrarian societies. Large-scale peasant revolutions appear throughout history but particularly in the modern period

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(e.g., in the Peloponnese in 227 B.C., England in 1381, France in 1789, Mexico in 1910, Russia in 1917, China beginning in 1921, Bolivia in 1952, and Cuba in 1958). For the old elite, the consequences of a successful revolution are clear. But for the mass of ordinary people, they are not. Revolutions generally promise peasants justice and at least some relief from rent, taxes, usury, and traditional restrictions on their movement. They surely benefit from that relief and, at least in the short run, from the more open and equalitarian society that results. But whether some benefit more than others and why is unclear. The long-term effects are even less clear. Does equality endure, or does inequality reemerge, perhaps in new and more virulent forms? Does social mobility grow or decline? Who benefits from the forces unleashed by revolution and how? In this paper we propose a theory about the effects of revolution on inequality and status attainment. We show that, in the short run, a revolution can be expected to reduce economic inequality and status inheritance, as anticipated, but also to benefit its well-to-do supporters more than its poorer ones and to make human capital more important for all. In the long run, peasants will still be better off, but stratification reemerges. Economic inequality and status inheritance grow steadily, in some circumstances eventually exceeding their prerevolutionary levels. We first present the theory informally and then develop an explicit mathematical model of the underlying process.

SCOPE

Our theory deals with the predominantly rural, premodern, peasant-dominated societies in which most revolutions have occurred.[2] We claim that it applies to any revolution meeting the following conditions: (1) a politically and economically dominant traditional elite has previously been able to expropriate a large fraction of the surplus produced by peasants (e.g., by control over land, forced labor, discriminatory taxation, usury, or through monopoly privileges in agriculture, trade, or government), and (2) the revolution has liberated peasants from their traditional exploitation (e.g., by destroying the old elite's economic privileges, reducing taxes or interest rates, redistributing land, allowing freer access to opportunities in farming and business, expropriating or destroying accumulated capital). We call this combination of events a radical revolution, and we limit consideration of shor-tterm effects to revolutions of this kind.

The predictions about long-term effects (Hypotheses 4 through 8 below)

[2] We use the term "peasant" broadly, to include not only the ideal type, "rural cultivators whose surpluses are transferred to a dominant group of rulers that uses the surplus both to underwrite its own standard of living and distribute the remainder to groups in society that do not farm but must be fed for their specific goods and services in turn" (Wolf 1966, pp. 3–4), but also farm laborers and other landless rural workers, small traders, and other members of the exploited rural masses.

are more general; they apply not only to radical revolutions but also to any social changes which reduce exploitation or increase economic opportunities. Such changes include economic "revolutions" which liberate people from stifling restrictions or increase their productivity by technical means: specifically, the gradual changes which destroyed feudalism, the early phases of the Industrial Revolution, the Green Revolution in agriculture, the introduction of cash crops or a market economy in nonmarket societies, and the like. They also include political changes which have increased opportunities for blacks and women in the United States, untouchables in India, the Ainu in Japan, and other minorities.

We deal with the apolitical mass of the rural and small-town population, deliberately excluding the revolution's political and military leaders, the revolutionary intelligentsia, and other revolutionary elites. Nonetheless, their ideology and the policies of the government they establish are extremely important. The peasants' goals will generally be what they regard as simple justice—personal (or communal) control over their land, minimal taxation, and the right to sell their produce on the open market. That leads to a predominantly market economy with peasants (or peasant communities) functioning essentially as small capitalist entrepreneurs accumulating income and property. In that case our model applies with full force. But the revolutionary elite may oppose the return to a classical peasant economy, instead pursuing more radical and collectivist goals. If successful this will mean, as Wolf (1966, 1969) and others have noted, the end of a conventional peasantry and the rise of a rural working class, usually employed in state-owned communal farms. Our model still applies in this case, but the changes will be slower and somewhat attenuated, in ways we specify.

SHORT-TERM EFFECTS

Inequality

We are dealing with radical revolutions which, by definition, at least partly free peasants from their traditional exploitation and thereby improve their economic position at the expense of the traditional elite. Transferring resources from the rich to the poor clearly reduces inequality (as we define it)[3]

[3] We have in mind the notion that inequality is greater where differences between rich and poor are large and widespread, i.e., where the differences between one person and another are large on the average. The standard deviation is a familiar measure of this aspect of inequality and is now widely used to measure inequality in education, occupation, and income (e.g., Jencks et al. 1972; Mincer 1974); it also links naturally with the formal theory that underlies our predictions. In the past, inequality was more commonly measured by the Gini or other coefficients based on the Lorenz curve. Like the standard deviation, the Gini is a measure of variance, but it is based on the absolute value of the difference between individuals' scores rather than the standard deviation's squared difference; the two are comparable and monotonically related (Paglin 1975, p. 601). The Gini adjusts for differences in the mean, while the standard deviation does not. Thus, for example, if everyone's

in the society as a whole, which is of course typically one of the revolution's main goals. In practice, the redistribution is often extensive. Radical revolutions often redistribute land, the fundamental fixed asset in peasant societies, and hence redistribute income. They usually redistribute liquid capital as well, expropriating or destroying rents, savings, debts, pensions, and monopolies; such redistribution reduces inequality, especially in the rare cases where the proceeds are directly redistributed to the poor. In many revolutions the expropriation is partly inadvertent. Property is abandoned during the crisis, and the collapse of the old government often leads to dramatic inflation which destroys the value of savings, salaries, and rents; these are more damaging to the old rich. Taxes and rents which fall most heavily on the poor are often reduced or eliminated. In precapitalist societies, labor taxes extracted by the state or by landlords are often the main form of exploitation, and abolishing them increases the time peasants have to work for their own benefit, leading to further equalization (e.g., Burke 1971, pp. 317–33; Klein 1969). In modern times, revolutionary governments usually establish new health, education, and welfare programs which result in major transfers of resources to the poor and further reduce inequality.

Human capital.—In the short run, we predict that radical revolutions will make human capital more valuable. In practice the range of opportunities for utilizing education, knowledge, technical skills, and other forms of human capital increases greatly. (1) Especially in previously isolated and traditional rural areas, rapid changes in marketing and the expansion of the money economy upset traditional economic arrangements and reward the adaptability, rationality, and cosmopolitan orientations that education provides (e.g., Schuman, Inkeles, and Smith 1967). Literacy and elementary bookkeeping skills are valuable even in very primitive economies (e.g., Pirenne 1937, p. 13; Kelley and Perlman 1971, pp. 216–20). (2) New political and economic power creates new opportunities for cultural brokers and go-betweens (politicians, lawyers, expediters, etc.) to mediate between peasant communities and nonpeasant society (e.g., Bailey 1963). To do so requires knowledge, contacts, and linguistic and political skills. Modern revolutions generally create numerous new positions in schools, health and welfare agencies, the government bureaucracy, and nationalized industry. Economic

income doubles, the Gini is unaffected, but the standard deviation is doubled. Doubling income also doubles the gap between rich and poor: a poor man has twice as large a gap to overcome if he is to become a rich one, and his son has twice the handicap; and that increases inequality in a familiar and reasonable sense of the term. So we prefer a measure, like the standard deviation, that reflects such changes. With this exception, the choice of measure makes no real difference in practice. The correlation between the Gini and the corresponding measure based on the standard deviation, computed over the income distributions of 56 countries, is .84 (computed from Paukert 1973, table 6); other popular measures are highly correlated with the Gini and so presumably with the standard deviation (Alker and Russett [1964] report correlations averaging .87 computed over various data of practical interest).

growth, a goal of almost all modern revolutions, expands the market economy and increases employment in professional, managerial, and clerical jobs and in transportation (Moore 1966; Kuznets 1965); and success in these requires educational, technical, and linguistic skills. (3) Educational credentials may become more important quite apart from any real connection with performance, since requiring fixed levels of education is an effective and convenient way of restricting access to jobs (Collins 1971), especially in the expanding bureaucracies. (4) In societies in which there are several languages (or the educated classes speak a different dialect), skills in the dominant language often become more valuable after the revolution. They give access to new opportunities in education and commerce and are useful in dealings with the bureaucracy. With increasing contact between urban and rural areas and the atrophy of the old landlord's role as intermediary, facility in the national language helps in dealing with the police, bureaucracy, merchants, and employers.

These new opportunities will, we predict, make education, technical and linguistic skills, and other forms of human capital more valuable, giving a larger return in occupational status and income. Some will be able to take direct advantage of their skills by self-employment, taking up more attractive and profitable opportunities than were available before the revolution. To match these new opportunities in self-employment, employers will have to offer more to attract skilled employees. Also the growth in the number of jobs requiring education and linguistic skills increases the demand for skilled personnel, and, since the supply can increase only slowly, skilled workers will use their improved bargaining position to extract better wages.

Who benefits?—Radical revolutions benefit most of their supporters, since their surplus is no longer expropriated by the old elite. But we predict that revolutions do not benefit the poorest as much as those who already possessed human or physical capital. Those with human capital, already better off before the revolution, have a great advantage in the new bureaucratic, commercial, and political jobs (e.g., in the Soviet Union [Khrushchev 1970, pp. 18–21]) and in commercial agriculture. In addition, there are typically substantial differences in the amount and value of land peasants worked before the revolution, and they are often able to maintain or strengthen their customary rights afterward (e.g., in Bolivia [Carter 1964]); then with their surplus no longer expropriated, well-to-do peasants benefit more from their advantages.

Status Inheritance

Because a radical revolution leads to the redistribution of wealth, we predict that it leads to less inheritance of status—that is, more pure social mo-

ility[4]—for those who came of age just after the revolution. Since many rerevolutionary elite parents lose their wealth, they have less of an ad-antage to pass on to their children, whereas some poor parents gain new esources and have more to give theirs. So on the average there is less varia-ion in the wealth that parents can pass on to their children and hence less tatus inheritance.

But status inheritance does not disappear. Some economic inequalities are kely to remain after even the most dedicated and efficient attempts at re-istribution. Human capital remains; education, literacy, technical and lin-uistic skills, and the like retain or even increase their value and cannot be edistributed. The old elite and others who were better off before the revolu-ion have more of these resources and are able to pass some of their skills n to their children. So an effective means of transmitting status from one eneration to the next remains; in the short run, a revolution will reduce tatus inheritance but not eliminate it.[5]

ummary

Hypothesis 1.—In the short run, a radical revolution produces a more equal istribution of physical capital and, for those coming of age just afterward, ess status inheritance.

Hypothesis 2.—In the short run, a radical revolution causes a shift in the asis of stratification, making human capital (education, knowledge, techni-al and linguistic skills, etc.) a more valuable source of occupational status nd income.

Hypothesis 3.—A revolution does not immediately benefit the poorest of ts supporters as much as it benefits those who possess human capital or have een able to retain physical capital.

"Status inheritance" refers to a lack of pure (Yasuda 1964) mobility; we measure it by he product-moment correlation between fathers' and sons' status. It focuses on the extent o which sons' status is *influenced* by fathers' status—on the amount of rigidity in the status tructure—regardless of whether sons rise above their fathers or sink below them. If sons om high-status families always maintain their advantage over sons from lower-status milies, there has been complete status inheritance, even when all sons have higher (or wer) status than their fathers. This is the aspect of social mobility that interests us, an terest widespread in the field (e.g., Blau and Duncan 1967; Treiman and Terrell 1975). o avoid confusion, we have not used the more general term "social mobility."

In exceptional circumstances, a revolution might even increase it, if physical capital layed a minor role before the revolution and human capital had a greatly increased payoff fterward. But that is unlikely in the predominantly peasant societies we are considering. urthermore, physical capital plays a major role in transmitting human capital from one eneration to the next—wealthy parents are better able to pay school fees and support heir children during their schooling—so economic redistribution will reduce the inheri-ance of human capital, which tends to decrease status inheritance; see eq. (3) below.

LONG-TERM EFFECTS

Stratification among Peasants

A radical revolution allows peasants to obtain a higher return on their physical capital since, by definition, it reduces exploitation. (1) By reducing rents or taxes on land, it allows peasants to retain more of what they produce. The destruction of corvée labor obligations—the crucial tax in many agrarian societies—allows peasants more time to work their own land for their own benefit (e.g., an additional one to three days per week in medieval Europe and three or more days in 20th-century Bolivia [Pirenne 1937, p. 64; Burke 1971, p. 328]). (2) Revolution is likely to reduce the costs peasants pay for goods and services by destroying traditional monopolies on trade, credit, and justice. Monopolies allowed traditional elites to charge exorbitant prices; even where the revolutionary government makes no deliberate attempt to reduce prices, competition is likely to drive them down. (3) Prior to the revolution, peasants' opportunities are often restricted to the least profitable sectors of the rural economy. However, the destruction of serfdom, corvée labor, and other laws tying peasants to the land opens up new opportunities. They can sell their own produce and take up wage-paying jobs in addition to agriculture, which in some cases increases their income dramatically (e.g., Burke 1971, pp. 318–31). Some become traders and merchant middlemen, replacing the old elite's commercial monopolies. (4) Economic change may have the same effects, with or without revolution. The introduction of new cash crops or new agricultural techniques, the opening of new markets, and the like all provide new and often profitable opportunities. Ending economic discrimination against blacks, untouchables, the Ainu, etc. opens up opportunities for them.

These new opportunities will, we predict, lead eventually to greater economic inequality among peasant proprietors and the mass of the previously exploited population.[6] Even in prerevolutionary times, peasants differ in their physical capital (e.g., size and quality of usufruct landholdings), in human capital (e.g., agricultural or linguistic skills, education, experience with the outside world), and in ability, diligence, motivation, luck, and the like. By expropriating the surplus and restricting opportunities to use capital effectively, the old system prevented fortunate peasants from getting the full benefit of their advantages and so restrained the growth of inequality. Revolution removes the restraints, allowing them to take full advantage of

[6] Especially for the peasantry (tied to the land, they were usually subject to effective control and stringent restrictions) and residents of small rural towns (also subject to effective control) but less for residents of large urban areas and perhaps not at all for workers on large plantations and in rural industries (revolution does not basically change their opportunity structure, and their wages are often subject to political control afterward).

their resources.[7] In the long run, that creates steadily growing inequality among peasants and other previously exploited groups. The fragmentary evidence now available supports this prediction (Chevalier 1967, pp. 178, 180–84; Craig 1969, pp. 290–91; Lenin [1920] 1967, p. 339; Petras and Zemelman 1972, pp. xii, 95–97). This leads to what might be called the kulak stage—the rise of a newly enriched sector of the peasant population and the emergence of an essentially capitalist rural stratification system.[8] Since fortunate peasants have increasingly large advantages to pass on to their children, we predict that revolution will in the long run lead also to steadily increasing status inheritance among them. The same reasoning applies to those economic revolutions and social changes that reduce exploitation, and in fact there is evidence that they increase both inequality (e.g., in agriculture following the Green Revolution [Havens and Flinn 1975]) and status inheritance (e.g., among American blacks in the past decade [Featherman and Hauser 1976]).

Human capital.—In the long run, a radical revolution leads to greater inequality in human capital among the peasantry and previously exploited masses. (1) Revolution provides additional reasons for acquiring human capital. Education, linguistic skills, and other forms of human capital are always valuable, and if anything, revolution makes them more so. Peasants can expect greater benefits from education after the revolution, since they have new opportunities to use it and can keep more of what they earn. Investing in education therefore becomes more attractive on straightforward economic grounds (e.g., Mincer 1974; Burke 1971, pp. 324–30). Economic revolutions often have the same effect (e.g., Patrick and Kehrberg 1973). (2) Modern revolutions supply the means. Whether from conviction or because of peasants' new political power, revolutionary governments generally

[7] In principle peasants might devote their new opportunities solely to leisure rather than accumulation, working only long enough to earn their customary wage. But in practice they are poor enough and materialistic enough not to do that; earlier claims that they would do so have been generally abandoned, now that systematic data are available (Miracle and Fetter 1970).

[8] The social and economic restraints which prerevolutionary peasants created in order to restrict inequality will in practice be eroded, if not destroyed, by revolution and the expansion of economic opportunities. In much of traditional Latin America, for example, the fiesta system effectively exchanged wealth for prestige, inhibiting the growth of economic inequality (Cancian 1965). As a man's career progressed, he took on an orderly system of lesser fiesta offices which paralleled his growing power and influence, culminating in his fifties with a major political role in the traditional community government and sponsorship of a major and expensive fiesta. After a revolution peasants are usually unwilling to exhaust their savings in this way, in part because the economy provides alternative attractions but also in part because the prestige obtained by sponsoring a fiesta has declined. Much of the prestige came from the intimate association with power in the traditional system. Revolution breaks that association down, creating new sources of power separate from the traditional offices and usually dominated by younger, more cosmopolitan leaders with little involvement in the fiesta system.

expand the school system, making education available where it was not before. (3) Educational inequality increases because some children benefit more than others. Able and motivated children have an advantage, as do children from privileged families. Throughout the world, well-educated, high-status families are much more successful in getting their children educated (e.g., in tribal societies [Kelley and Perlman 1971], in socialist societies [Anderson 1975; Lane 1971, pp. 107–20] and in industrial societies [Treiman and Terrell 1975]); they provide encouragement and role models, teach linguistic and academic skills, force their children to work harder, and the like. Schooling is usually expensive, both in direct costs (fees, supplies, clothing, etc.) and indirect costs (income the student could otherwise have earned); prosperous families can better afford these costs.

This growing inequality in human capital will, we predict, in the long run lead to greater economic inequality and more status inheritance among peasants. Since education and other forms of human capital are quite valuable, greater inequality in human capital leads to greater inequality in income and wealth. That, we have argued, leads to greater status inheritance. Educational changes also increase status inheritance directly. As educational inequality grows among parents—that is, as the gap between well- and poorly educated parents increases—it becomes more of an advantage to be born into a well-educated family.

Government intervention.—A revolutionary government can try to restrain these forces by limiting the private accumulation and inheritance of capital. Populist and middle-class revolutionary parties are unlikely to have either the ideological justification or the dedicated cadre with which to do so, although many socialist and communist governments make the attempt. But it is unlikely to succeed. Expropriating large landowners, large capitalists, and foreign investments and thereby securing the "commanding heights" of the economy will not be enough, since accumulation by the mass of upper peasants and the educated middle class leads, we have argued, to inequality. To restrain these groups, private capital will have to be abolished throughout the economy. In practice this is usually accomplished by socializing the industrial economy and collectivizing the land and sometimes by the physical extinction of the kulaks. Many people have something to lose from such actions, and they are not without recourse. Small businessmen have money and can threaten to withdraw valuable services; the upper levels of the peasantry know they have much to lose; the educated middle class and party workers newly ensconced in the bureaucracy will want to secure their advantage by accumulating wealth. To fully overcome the opposition of these groups requires from the party's cadres a level of commitment, dedication, and resistance to temptation that is difficult to maintain over the years; it also requires an extensive and efficient bureaucratic apparatus which can

extend its control to the very grass roots, an apparatus few societies have ever possessed. China's cultural revolution may have been in part an attempt to overcome this kind of opposition and prevent the reemergence of inequality (see, e.g., Yüeh 1976). Even in China, however, the costs were great, opposition was strong, and success uncertain; other examples are not easy to find.

But the abolition of private capital is not in itself enough to prevent the long-term growth of inequality, since much (indeed most) inequality arises from differences in education, skills, language, and other forms of human capital which are almost immune to redistribution. Human capital is crucial: to run even a moderately complex society requires an educated elite—business, industry, and government require a variety of administrative and technical skills, and even farming and small trading are greatly facilitated by literacy, bookkeeping, and specialized technical skills (see, e.g., Becker 1964; Mincer 1974; Stinchcombe 1961). Although it is sometimes claimed that schools impart few skills of any genuine importance but merely screen or certify or are otherwise dispensable, that claim is inconsistent with detailed evidence for modern industrial societies and with the clear importance of education in societies with very different economic and institutional structures (for industrial societies, see Layard and Pscharopoulos [1974]; and Welch [1975, pp. 65–69]; for socialist societies, Anderson [1975]; for feudal societies, Pirenne [1937, p. 13]; for tribal and developing societies, Lenski [1966], Kelley and Perlman [1971], and Kelley [1976, table 1]). Ignoring these skills in favor of political or equity considerations is exceedingly costly (see, e.g., Khrushchev 1970, pp. 18–21); to date only China has systematically and persistently attempted it after the revolutionary government was firmly established and the threat of counterrevolution past. Nor can governments effectively prevent human capital from being passed from one generation to the next without draconian changes in the family. The knowledge, values, culture, and language skills acquired in elite homes give children an enormous and enduring advantage in socialist as well as capitalist societies (Anderson 1975; Duncan, Featherman, and Duncan 1972, chaps. 3, 5, 6; Lane 1971, chap. 5); discriminatory admissions policies for higher education and government can reduce the advantage somewhat but not eliminate it, save at enormous cost. Thus a revolution able to abolish private property will slow the long-term growth of inequality and status inheritance but will not prevent it.

Stratification in the Society as a Whole

In the long run, a radical revolution will, we have argued, create more inequality and status inheritance among peasants and the previously ex-

ploited rural masses. But its effects on the society as a whole are less clear. We will argue that inequality and status inheritance first decrease and then remain low for a period; in most circumstances they then increase steadily and, in some circumstances, eventually exceed their prerevolutionary levels.

Economic development increases inequality. Even if everyone retains the same relative position, development increases the absolute size of the gap between rich and poor and therefore increases inequality as we define it; if, for example, the introduction of new cash crops doubles everyone's income, it also doubles the gap between poor peasants and rich merchants, so the peasant has twice the obstacle to overcome if he is to live as well as a merchant, and a peasant's son has twice the handicap to overcome if he is to catch up with a merchant's son. In addition, anyone with physical capital, human capital, or other advantages will be better able to take advantage of new opportunities opened up by economic development and that increases inequality by any definition.

The benefits that revolution provides for peasants and the exploited rural masses will at first decrease inequality in the society as a whole. Peasants' income, wealth, and human capital almost always begin well below the average for the whole society, while the commercial and administrative sectors in rural towns and most urban groups are markedly better off initially. The revolution reduces exploitation, improving the economic position of all peasants and moving them closer to the mean. That reduces inequality.[9] Most peasants go no further. But those with physical or human capital or other resources will continue to improve their position, especially if the revolution is one which produces economic development. As they draw closer to the mean, inequality continues to decline. But as they surpass the mean in increasing numbers, inequality first stabilizes and then (depending on how many surpass it and by how much) may increase.[10] So there is a standard sequence following the revolution. Inequality first declines and then stabilizes. If peasants continue to improve their economic position, the decline lasts longer, but eventually inequality begins to increase again and eventually may exceed its prerevolutionary level.

How far along this sequence a society proceeds depends not only on what happens to the peasants but also on how high the mean is to begin with and how it changes subsequently. Most prerevolutionary peasant societies are very poor, with a small surplus extracted by a tiny elite (Lenski 1966,

[9] The standard deviation, our measure of inequality, is of course a simple function of the (squared) deviation from the mean. As peasants approach the mean, the deviation decreases; and as they draw further away from it, it increases.

[10] Inequality actually begins to increase somewhat before they reach the mean. Improvement of their position will usually raise the mean for the society as a whole, and poor peasants will then be further behind, which increases inequality.

chaps. 8, 9). The average is low, and, other things being equal, that makes it easier to surpass, and the society will then go through the sequence quickly, often reaching the stage where inequality increases. In richer societies (e.g., eastern Europe following the communist revolutions), peasants have further to go, and the society passes along the sequence more slowly. The average also depends on what happens to the urban population and the postrevolutionary elite, but that reflects the power and ideology of the revolutionary leadership, the society's economic and administrative capacity, international political and economic restraints, and a variety of other factors beyond the scope of our theory.

There may be further redistribution after the revolution; this too affects inequality. Particularly where there is no sustained economic growth, gains by rich peasants are someone else's losses. If they gain entirely at the expense of the elite, there will be more equality. But in practice, their gains will most probably be at the cost of poor peasants and lower and middle classes in the towns. As rich peasants take over marketing, credit, and middleman functions, they displace middle- and lower-class urbanites, and liberated peasants compete for desirable urban jobs. Successful peasants will produce cash crops more efficiently, undercutting poor peasants' market positions and driving them off the land. When rich peasants begin to pass the mean, inequality will eventually increase as long as their gains are mainly at the expense of groups below the average (or, of course, if they are at no one's expense).

A revolution's effects on inequality in the society as a whole thus depend crucially on the speed of economic development, the economic position of urban groups and the postrevolutionary elite, and government policies toward accumulation. We predict that inequality will increase most dramatically if the revolution generates economic development (which directly increases inequality) and if the entire society was poor to begin with (since rich peasants exceed the mean sooner). Since modern revolutions in poor societies (e.g., Mexico in 1910, Bolivia in 1952) almost always promote economic development, we predict that they will eventually create more inequality than existed before the revolution unless governments make strenuous efforts to prevent it. The scattered evidence now available suggests that inequality does increase (e.g., Wolf 1956). Economic revolutions—the decay of feudalism, the early stages of industrial revolution, the introduction of cash crops and a money economy in premarket Asian and African societies the Green Revolution in agriculture, etc.—lead to economic development We predict that therefore they will cause inequality in the long run; the evidence indicates that they do (Kuznets 1965, pp. 275–77). In contrast, we predict that classical peasant revolutions in traditional societies in which urban areas remain much richer than the countryside and no economic de

velopment results will reduce inequality (e.g., Punjab in the late 19th century). Changes in status inheritance in the society as a whole will, we predict, parallel changes in inequality for the reasons set out earlier. The only clear data now available support this prediction.[11]

Summary

Hypothesis 4.—In the long run, peasants are better off after a radical revolution.

Hypothesis 5.—By allowing peasants to utilize their resources more fully, radical revolutions set loose forces which tend in the long run to produce steadily increasing economic inequality among them.

Hypothesis 6.—In the long run, radical revolutions produce increasing educational inequality among peasants.

Hypothesis 7.—Among peasants, radical revolutions create forces which tend in the long run to produce more status inheritance through both economic advantage and education.

Hypothesis 8.—In the society as a whole, inequality and status inheritance following a radical revolution will first decrease, then stabilize, then (*a*) remain low if nonpeasants remain well off and there is no economic development in the countryside but (*b*) steadily increase and perhaps in time exceed prerevolutionary levels in poor societies in which there is substantial economic development.

Hypotheses 4–8 apply not only to radical revolutions but also to any social changes which reduce exploitation or increase economic opportunities, with the poor and exploited taking the role of peasants.

FORMAL MODEL

Short-Term Effects

We have defined "radical revolutions" as those which reduce the exploitation of peasants. Since peasants are poor and exploiters rich, that clearly reduces the standard deviation, our measure of inequality. For people coming of age shortly after the revolution, before inequality has had a chance to reemerge, the decline in inequality should, we will show, lead to less status inheritance (i.e., more pure mobility). A detailed argument is given elsewhere (Kelley, in press). Briefly, we assume that a son's (or daughter's) occupational status is determined only by his human capital (education, knowledge, skills in

[11] These data are from the communist revolution in Poland. The correlation between father's and son's occupational status (dichotomized into manual/nonmanual) in Poland was .63 for cohorts coming of age before World War II, .58 for those coming of age during the war, .43 for those at the beginning of the communist period, and .51 for later (1956–68) cohorts (computed from Zagorski [1971], $N = 3{,}260$).

language, war, crafts, etc.), his physical capital (wealth, land, cattle, ect.), and various other things (luck, strength, ability, etc.) which are uncorrelated with his father's occupational status. We assume that all relationships are, to a reasonable approximation, linear and additive, so that

$$\text{STATUS}_s = h\ \text{HUMANCAP}_s + w\ \text{WEALTH}_s + \text{OTHER}_1\ , \quad (1)$$

where STATUS_s is the son's occupational status, HUMANCAP_s is his human capital, WEALTH_s is his wealth and other physical capital, h and w are constants, and OTHER_1 measures other factors. His human and physical capital are in turn determined by his father's human and physical capital and by ability, motivation, luck, and other factors uncorrelated with his father's occupation, that is,

$$\begin{aligned} \text{HUMANCAP}_s &= h_{fh}\ \text{HUMANCAP}_f + h_{fw}\ \text{WEALTH}_f + \text{OTHER}_2\ , \\ \text{WEALTH}_s &= w_{fh}\ \text{HUMANCAP}_f + w_{fw}\ \text{WEALTH}_f + \text{OTHER}_3\ , \end{aligned} \quad (2)$$

where HUMANCAP_f and WEALTH_f are father's human and physical capital, the h's and w's are constants, and the OTHERs measure other factors uncorrelated with father's capital or status. The correlation, $r_{fs,ss}$, between father's and son's occupational status, the usual measure of status inheritance, is obtained by substituting these in equation (1), multiplying both sides by father's status, summing over the whole population, and dividing by N, giving

$$r_{fs,ss} = r_{fs,fh} \frac{\sigma_{fh}}{\sigma_{ss}} (h_{fh}h + w_{fh}w) + r_{fs,fw} \frac{\sigma_{fw}}{\sigma_{ss}} (h_{fw}h + w_{fw}w)\ , \quad (3)$$

where the subscripts fs, fh, and fw refer to father's status, human capital, and wealth, respectively, and ss to son's status. (We have assumed without loss of generality that all variables are measured as deviations from their means.) The OTHERs have dropped out, since they are uncorrelated with father's status. All terms in equation (3) are positive, since we assume that having capital is always beneficial (therefore the lowercase constants are all positive) and that the correlations between father's status and his human capital and physical capital, respectively, are positive; we also assume that the range of occupations open to sons is fixed by the occupational structure or changes only slowly, so that σ_{ss} is approximately constant. It follows directly that, other things being equal, $r_{fs,ss}$ will be smaller—that is, there will be less status inheritance—whenever human capital or physical capital is more equally distributed in the father's generation, since either σ_{fh} or σ_{fw} is smaller. That[12] is the second part of Hypothesis 1. The converse, that greater

[12] This is somewhat offset by the increasing importance of education (i.e., increasing h) predicted by Hypothesis 2. In practice the effect of education is likely to be smaller than

inequality in father's human or physical capital leads to more status inheritance, is used later in Hypotheses 7 and 8.

Hypotheses 2 and 3 follow from the familiar model of occupational attainment as a sorting process (e.g., Thurow 1975). Candidates for jobs differ in various ways (human capital, wealth, ability, motivation, etc.) which make some of them more suitable than others. Jobs also offer various rewards (money, prestige, interest, etc.) which make some more desirable than others. Getting a job is then like a marriage market; except for a good deal of luck and confusion, the most suitable candidates get the best jobs (they want those jobs, and employers prefer them to less suitable candidates), and the less suitable candidates get the poorer jobs (the better jobs are already taken, and the better candidates are already employed). Hypothesis 3 follows on the assumption that revolution does not change this basic process but simply provides new slots and removes the old elite from the top of the queue, opening up new places for everyone else. Hypothesis 2 claims essentially that revolutions produce changes in the kinds of jobs available and in the criteria for filling them, which make candidates with education and various skills more desirable to employers (or more successful in self-employment) and hence able to obtain better jobs and higher pay.

Long-Term Effects

We now develop a model of a radical revolution's effects on income and the ccumulation of wealth and hence on inequality and status inheritance. Hourly rates of pay for unskilled workers before the revolution, pay_u, are letermined by various production and supply conditions, employers' monopoly powers, geographic restrictions (which, e.g., prevent peasants from taking ip trading and urban jobs or moving to high-wage areas), and the like. We ssume that wage rates also depend on human capital (HUMANCAP) and n motivation, ability, luck, and the like (call them OTHER) which can to reasonable approximation be combined into a single aggregate resource HUMANCAP + OTHER) with wages proportional to it.[18] The return to

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ie effects of σ_{fw} and σ_{fh}. It involves only half the terms in eq. (3). Increasing h will also icrease σ_{fw} (because it increases the returns the father gets from his educational re-ources), and the jobs for which education matters are a small fraction of the total in most easant societies.

Without changing our conclusions, the aggregate could be any function which increases henever any of its components increases and whose variance increases whenever any of eir variances increase. Assume, plausibly, that having one resource (education, skills, otivation, ability, luck, etc.) does not systematically entail a loss of others, so that the rrelations among resources are positive or zero. Then any (positively weighted) average them, a multiplicative Cobb-Douglas function of them, and a variety of other specific nctions all have the required characteristics.

this combined resource reflects both the economic return and the costs imposed by restrictions on the type and location of work peasants are allowed to undertake, employers' monopoly powers, and the like. Wage rates for all workers are then $[pay_u + pay_s \cdot (HUMANCAP + OTHER)]$, all workers receiving pay_u and skilled workers receiving a bonus proportional to their resources. For simplicity, we assume that this is the same throughout the life cycle, but other reasonable assumptions (e.g., Mincer 1974, chap. 1) would lead to the same qualitative conclusions. By reducing the costs imposed on pay_u and pay_s, a revolution increases both components of workers' pay.

Earnings depend on the number of hours worked for pay. Yearly earnings, EARNED, will be hours worked for pay times the wage rate, less any per capita taxes (e.g., headtaxes), or in all

$$\begin{aligned} EARNED &= hours\,[pay_u + pay_s \cdot (HUMANCAP + OTHER)] \\ &\quad - headtax \\ &= (hours \cdot pay_u - headtax) \\ &\quad + [hours \cdot pay_s \cdot (HUMANCAP + OTHER)]\,. \end{aligned} \tag{4}$$

The third and fourth lines separate earnings into a part that is the same for everyone (unskilled wages minus head taxes) and a part that depends on the worker's human capital and other resources. Labor taxes (common in quasi-feudal societies) simply subtract some hours from those worked for pay; taxes that are, taken all together, approximately a fixed proportion of income (e.g., some combination of progressive, flat-rate, and regressive taxes) also in effect subtract working hours. Revolution in an exploited peasant countryside will reduce taxes, effectively increasing the number of hours worked for pay, and hence increase earnings and the variance in earnings; in other words, it will increase inequality.

Wealth is simply accumulated savings from the past plus inherited wealth, both invested at interest. Assume that people save approximately a fixed percentage, save, of both their wage income and the interest, int, they earn on accumulated capital. Then wealth in the nth year of someone's working life, $WEALTH_n$, depends simply on accumulated wealth from the past, $WEALTH_{n-1}$; on savings from interest, $save \cdot int \cdot WEALTH_{n-1}$; and on savings from the year's wage earnings, $save \cdot EARNED$, giving,

$$\begin{aligned} WEALTH_n &= WEALTH_{n-1} + save \cdot int \cdot WEALTH_{n-1} \\ &\quad + save \cdot EARNED \\ &= (1 + save \cdot int)\, WEALTH_{n-1} + save \cdot EARNED\,. \end{aligned} \tag{5}$$

By induction, wealth at time n can be expressed as a function of inherited wealth, WEALTH_0, and the accumulated savings from each year in the past:

$$\begin{aligned}\text{WEALTH}_n &= (1 + \text{save}\cdot\text{int})^n\,\text{WEALTH}_0 + (1 + \text{save}\cdot\text{int})^{n-1}\text{save}\cdot\\ &\qquad \text{EARNED} + (1 + \text{save}\cdot\text{int})^{n-2}\text{save}\cdot\text{EARNED}\\ &\qquad + \cdots + \text{save}\cdot\text{EARNED}\\ &= [(1 + \text{save}\cdot\text{int})^n\,\text{WEALTH}_0]\\ &\qquad + \left[\frac{(1 + \text{save}\cdot\text{int})^n - 1}{\text{int}}(\text{hours}\cdot\text{pay}_u - \text{headtax})\right] \qquad (6)\\ &\qquad + \left[\frac{(1 + \text{save}\cdot\text{int})^n - 1}{\text{int}}\,\text{hours}\cdot\text{pay}_s\cdot(\text{HUMANCAP}\right.\\ &\qquad\qquad \left. + \text{OTHER})\right].\end{aligned}$$

The second line is obtained by summing the geometric series and then substituting the earnings expression from (4) and simplifying.[14] It gives current wealth explicitly in terms of the accumulated advantages from inherited wealth (first term), plus the accumulated savings everyone will have from his labor (second term), plus the accumulated savings from the bonus paid to workers with human capital and related resources (third term).

Total yearly income from all sources in the $(n + 1)$th year, INCOME_{n+1}, depends on the same factors. It is simply the interest earned on the wealth accumulated in previous years plus the amount earned by working during the current year or, from equation (4),

$$\begin{aligned}\text{INCOME}_{n+1} &= (\text{int}\cdot\text{WEALTH}_n) + (\text{hours}\cdot\text{pay}_u - \text{headtax})\\ &\qquad + [\text{hours}\cdot\text{pay}_s\cdot(\text{HUMANCAP} + \text{OTHER})] \qquad (7)\end{aligned}$$

The first term is the return from accumulated wealth, the second is the income everyone earns by working, and the third term is the bonus earned by workers with human capital or other resources.

A radical revolution increases peasants' wealth and income (Hypothesis 4). It reduces taxes (headtax is smaller afterward, or, for proportional taxes, peasants in effect have more hours to work on their own account); that reduction increases the second component of both wealth and income. By such action as destroying the old elite's monopolies, removing restrictions on geographic mobility, and allowing peasants to take up a wider range of jobs, the revolution increases both pay_u and pay_s, thereby increasing the second and third components of both wealth and income. New op-

[14] The sum $1 + r + r^2 + \ldots + r^{n-1}$ is $(r^n - 1)/(r - 1)$ in general or $[(1 + \text{save}\cdot\text{int})^n - 1]/(1 + \text{save}\cdot\text{int} - 1)$ in this case.

portunities and incentives for peasants to invest in human capital increase the third component. Revolution may allow peasants to use their physical capital more effectively; this result increases the interest rate and therefore increases all three components of wealth and the first component of income. Anything that increases wealth of course increases the first component of income in subsequent years.

Inequality is the standard deviation of wealth (eq. [6]) or income (eq. [7]). It will be greater whenever any of the coefficients of the first or third component are higher.[15] Even when everyone benefits from an increase, people with inherited wealth, human capital, or other resources will benefit more than others, and inequality will increase. So an increase in the rate of saving or the interest rate produces greater inequality in wealth (those who inherited wealth get a larger return from it, and people with human capital get more from savings) and, in subsequent years, in income. An increase in the pay of skilled workers or in the number of hours worked for pay greatly increases inequality in wealth and income, since those benefits go disproportionately to people with human capital and other resources. As we have seen, revolution increases all of these. With higher returns available on human capital, peasants have more reason to invest; the variance in human capital is likely to increase as well, if only because education depends on ability and family background. Both increase the variance in the third component of wealth and income and hence inequality. Inequality also increases when the variance in inherited wealth goes up; more inequality among postrevolutionary fathers ensures that. These are the claims underlying Hypothesis 5.

Revolution makes investment in education more attractive by increasing the returns, pay_s, reducing the taxes paid on these returns, and increasing profits from investing them. Hence fathers devote more of their wealth to paying for their sons' education and invest more time and effort in transmitting their cultural skills; sons more fully exploit their ability and other resources, which enable them to acquire education (thus in eq. [2], h_{fh}, h_{fw}, and $OTHER_s$ will be larger). Since sons' abilities and fathers' human and

[15] Both expressions are of the general form $a \cdot WEALTH + b + c \cdot (HUMANCAP + OTHER)$, where a, b, and c are constants, and WEALTH and (HUMANCAP + OTHER) are variables. The standard deviation of the sum is a function of the means and standard deviation of each variable and the covariance between them. We assume that inherited wealth is either positively correlated with human capital and other resources or uncorrelated; it follows that wealth in the nth year also has a positive or zero correlation. Hence the standard deviation will be an increasing function of a, of c, and of the variance in wealth, human capital, and other resources. Since b is the same for everyone, it does not contribute to inequality in the nth year (the time specified in eqq. [6] and [7]), but it does contribute to inequality for the whole population. People who have worked for only a few years will have had only a few years in which to save, but people who have worked for many years will have many years of savings and so more accumulated wealth; hence even an across-the-board increase in wages produces some inequality (Paglin 1975). This effect will, however, be small compared with the others, so we ignore it.

physical capital are themselves unequally distributed (especially after the revolution), the variance in sons' education (computed from eq. [2]) will increase. This is the claim made in Hypothesis 6.

Hypothesis 7 follows from Hypotheses 5 and 6 together with equation (3). Revolution increases inequality among fathers, that is, σ_{fh} and σ_{fw}; and that increases status inheritance.

Hypothesis 8 follows from a simple model. As a rough approximation, we can imagine that there are four classes in the society as a whole—peasants and other members of the rural masses who have no appreciable human capital, peasants and others with human capital, the working- and middle-class population of the towns, and the urban elite—and that within each class everyone has the same wealth and income. The average income (or wealth) in the society as a whole is simply the weighted average for all four classes, and income inequality is the standard deviation of income, which is a simple function of the squared deviation from the mean. Before the revolution, all peasants are poor, the elite is rich, and the town population is somewhere in between. Revolution makes the elite poorer and peasants richer, bringing everyone closer to the mean and reducing inequality. Peasants with human capital then begin to improve their positions steadily, which further reduces inequality when they are still below the mean, has little effect when they are near the mean, and eventually increases inequality if they rise above the mean. Whether they get that far depends on how high the mean was to start with, how much richer they become, and what happens to the mean while all this is going on. If the mean is low at the beginning, if the revolution leads to steady economic development in the countryside (so that peasants with capital continue to grow richer), and if the town dwellers' and urban elite's wealth do not grow, then many rich peasants will exceed the mean. Inequality in the society will increase and may eventually exceed its prerevolutionary value, particularly when town dwellers and the elite are a small part of the total population. Without steady economic development and with a large gap between peasants and the rest of the society, inequality will remain low. These are the claims of Hypothesis 8. In other situations there are conflicting tendencies, the outcome depending on which are stronger. More complex models with more classes and wealth distinctions within classes lead to the same qualitative conclusions.

DISCUSSION

A violent social revolution which frees peasants from their traditional exploitation will, we argue, in the short run improve their standard of living, reduce inequality in the society as a whole, and reduce inherited privilege. At this stage the peasants and the radical revolutionary intelligentsia have the same goal, the overthrow of the traditional elite and the end of exploita-

tion; but that does not last. Peasants with more land, human capital, or other resources are able to exploit their advantages more fully after the revolution. Furthermore revolution inadvertently sets loose forces which, if unchecked, will in the long run allow inequality and inherited privilege to grow steadily and, in some circumstances, exceed their prerevolutionary values. The result will be a relatively rigid rural stratification system of a familiar capitalist type, with rich peasants playing the role of capitalist entrepreneur. Thwarting these forces is never easy and often impossible. Liberal policies or even moderate socialist policies which allow a mixed economy will hardly repress these deeply rooted forces. They involve the property of modest family farmers and the opportunities for small traders, small businessmen, government bureaucrats, and the holders of human capital to accumulate wealth and pass it on to their children. By giving in to these forces, accepting the new status quo, and abandoning any further redistribution in the countryside, a revolution can maintain the allegiance of the new elites, leaving a peaceful, prosperous, and highly stratified countryside. The Bolivian and Mexican revolutions, for example, seem to have taken this conservative course. The only viable alternative is a radical attempt to root out even the smallest vestiges of private property, as in China or Cuba. Among other things, this requires extensive economic planning, a large and effective bureaucracy with unusual commitment and incorruptibility, the power to overcome strong opposition in the countryside, and a willingness to bear the substantial human costs involved. But even that does not solve the whole problem. If differences in education, skills, language, ability, or other kinds of human capital remain, they will eventually (albeit more slowly) lead to inequality, and unless children are reared apart from their parents, to inherited advantage. Human capital is much harder to control than is physical capital. Confiscating land and machines is easy compared with controlling it, and even in simple societies it is at least as important to the economy. To date, only China has been willing to bear the costs of a determined assault on human capital, and even there, both the outcome and the will to continue the assault are far from clear. In short, a revolution must either quietly turn conservative, allowing inequality and inherited privilege to grow steadily in the countryside, or turn radical and embark with uncertain prospects on an arduous struggle to remake the entire economy and culture.

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Racial Stratification and Socioeconomic Change in the American North and South[1]

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Recent change in the life-cycle processes of educational, occupational, and earnings attainments is analyzed among black men native to the South, those native to the North, and black male migrants from the South to the North. Native northerners begin from relatively superior social origins and are better able to capitalize on these in the attainments of education and occupation than are either southern-born group. Between 1962 and 1973 the stratification experiences of the northern-born blacks rapidly converged with those of the white majority, so that by 1973 their system of stratification was more like that of whites than that of southern-born blacks. The processes of status allocation among the southern-born in 1973 were like those of northern natives in 1962. In this sense the integration of blacks into the majority stratification system began first and has proceeded furthest among blacks born in the North. Men living in the North, regardless of nativity, enjoy higher earnings than men living in the South. Migrants to the North earned about $400 more in 1972 than did comparable northern natives. This advantage is not accounted for by longer schooling or higher returns to education, occupation, or number of weeks worked, since the natives are equal or superior to the migrants in these factors. In all, changes over the recent decade have supported the internal differentiation of the black population, the development of more distinct socioeconomic strata, the greater stability of inequalities between generations of black men, and gains toward socioeconomic integration. These changes have been more characteristic of the North than of the South.

The best known statement of traditional race relations theory (Myrdal 1944) characterized the American South of 30 years ago as a racially divided paternalistic society ruled by the "white man's theory of color caste." The twin pillars of racist ideology and economic self-interest provided the basis for the theory of color caste. The society was preserved

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through an apartheid-like system of etiquette (Jim Crow) and through racially segregated labor markets in which blacks competed for the most undesirable sorts of jobs. Cross-cultural research indicates that such a stratification system was a relatively common consequence of initial interracial contact in a situation of colonial conquest and settlement by Europeans with a racist ideology (Banton 1967; Kinloch 1974; Schermerhorn 1970; van den Berghe 1967).

The maintenance of the racial boundary is of paramount importance to the survival of such a society. But it is increasingly difficult to maintain as the social structure changes from a paternalistic agricultural system with prescribed social positions into a modern urban-industrial society in which social positions (at least among the majority population) are achieved by universalistic criteria (Banton 1967). As Myrdal foresaw, industrialization and urbanization in the South, as well as the migration of blacks to the more developed North, permitted some members of the minority group to achieve relatively high educations, occupations, and earnings, and also fostered conditions permitting economic and political action in pursuit of racial equality.

One way to measure black progress toward the achievement of social and economic equality is to compare the processes of achievement of the minority and majority populations. The available evidence to date indicates that lower black attainments have resulted not so much from the lower social origins of blacks as from a system of intergenerational status allocation (stratification) in which a black receives fewer returns to favorable characteristics of socioeconomic background or to subsequent investments in human capital than does a comparable white (e.g., Siegel 1965; Duncan 1968). Black men have experienced a perverse sort of egalitarianism—neither the disadvantages of lower socioeconomic origins nor the advantages of higher social origins and education weigh as heavily in the status attainments of blacks as they do in those of whites.

However, racial differentials in processes of status allocation have diminished in recent years, indicating that blacks have moved in the direction of economic integration with the majority population. By 1973 net differences in completed schooling which could be linked directly to race (versus socioeconomic background) had decreased to about one-half year for men in the experienced civilian labor force (ECLF). Among men aged

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25–34 in the ECLF there would have been no racial difference in education had socioeconomic origins been equal (Hauser and Featherman 1976). The gap in occupational status which separates black and white workers declined to about 17 points on the Duncan scale (Duncan 1961), and the correlations among statuses and between socioeconomic background and subsequent statuses were less different for the two major races. Thus, Featherman and Hauser note: "The tighter articulation between family background and achievement has fashioned a pattern of intergenerational stratification for younger blacks which resembles that among younger white men. . . . At the same time, the effect of education on occupational status has increased absolutely and relative to that of the family since 1962, and there is growing inequality in the statuses of black men of similar origins and schooling" (1976, p. 639). In this fashion the social stratification between generations of the black population is beginning to follow a process that tends to characterize majority populations in many industrialized nations, including the United States (Featherman, Jones, and Hauser 1975; Featherman and Hauser 1976).

But are these changes in racial stratification likely to appear in all segments of the black population—particularly by region? For instance, blacks in the North had gained employment in the industrial sector of the economy earlier than in the South (Taeuber and Taeuber 1965; U.S. Bureau of the Census 1971). This fact, together with the more recent development of an urban-industrial economy in the South, provides presumptive evidence that the application of universalistic criteria of achievement among southern blacks might lag behind that of northern blacks. Furthermore, the North was never structured as a strict two-color system; it included many ethnic and immigrant groups from abroad, and blacks probably have been viewed as a special group within this pluralistic system (Newman 1973). Finally, a theory of cultural lag complements these inferences from structural evidence about regional development and leads to the expectation of more rapid achievement of economic integration with the majority among northern than among southern blacks (e.g., Middleton 1976).

Because of the differing histories of race relations and of development of a modern industrial economy in the North and South, migration from the South to the North has been viewed as one way in which American blacks have upgraded their relative socioeconomic position (Myrdal 1944; Banton 1967; van den Berghe 1967). Such migration resulted from both push factors (e.g., few job opportunities and seasonal employment in the rural South) and pull factors (e.g., the lure of better paying jobs in northern industry, especially during war) (U.S. Bureau of the Census 1971, chap. 14; Eldridge and Thomas 1964). Migration from less developed regions into the new urban centers is a common concomitant of

national economic development in other countries as well (Goldscheider 1971; Balan, Browning, and Jelin 1973). In both the United States and Mexico migration is one mechanism removing such ascriptive restrictions on achievement as place of origin by enabling a man to take advantage of opportunities not available in his community of origin (Blau and Duncan 1967; Balan et al. 1973).

Lieberson (1973) draws attention to the importance of distinguishing such migrants to the North from blacks who are native to the North. While nearly all blacks of southern residence were born in the South, a large and temporally varying proportion of northern blacks have been born in the South. Since the social origins, as well as the culture and personalities, of southern-born blacks may differ from those of northern-born blacks, and since such factors in turn are relevant to lifetime social and economic attainments, nativity should be considered when making comparisons between southern and northern blacks or when drawing comparisons among northern blacks over time (Lieberson 1973). Such a distinction is crucial if residential segregation of blacks from whites is less characteristic of the second than of the first "generation" (in terms of nativity, northern residents of southern birth constitute the first generation; see Lieberson 1973), as is the case among many other ethnic groups. In that instance, the labor markets accessible to the two groups may vary and tend to produce generational differences in occupational status and earnings.

Both regional and generational differences among blacks in the process of stratification produce pronounced inequalities in attainments within the black population. In fact, in recent cohorts the variability (inequality) of schooling is greater among blacks than among whites (Hauser and Featherman 1976). Native northern blacks enjoy longer schooling, higher occupational attainments, and a more status-differentiated labor market than do migrants to the North, although larger proportions of the former tend to be out of the labor force (Lieberson and Wilkinson 1976; Long and Heltman 1975). Among men of the same age and schooling, migrants to the North appear to enjoy somewhat higher earnings than do the northern natives (Lieberson and Wilkinson 1976; Crain and Weisman 1972; Long and Heltman 1975; Weiss and Williamson 1972, 1975).

However, since World War II the industrial and occupational compositions of the American North and South have become more similar (McKinney and Bourque 1971). Much of this convergence reflects strikingly rapid secular shifts of employment within the South—out of agriculture and into construction, manufacturing, and trade; from jobs as farmers and farm laborers to those as craftsmen and foremen, machine operators, and professional and technical workers. As the economies of the two regions converge, the processes of social stratification which allocate persons into positions in the economy and its socioeconomic hierarchy should become

more homogeneous. This line of reasoning follows from theories of "convergence" and the "thesis of industrialism," which propose that a major driving force behind the division of labor, inequality, and the mechanisms of status inheritance and mobility between generations is the nature of a society's economy (e.g., Feldman and Moore 1962). If the social structures (viz., the economy and the associated system of status allocation) of two societies tend to converge owing to the technical and organizational requirements of their common economies, then it would not be unreasonable to expect regional variation in social stratification to disappear as the regional economies lose their distinctive characters. In particular, economic integration of blacks into the majority status system should be as evident in the South as in the North, and the recent socioeconomic differentiation *within* the black population should be general across regions.

In this paper we estimate basic life-cycle models of stratification as a first step toward understanding changing differences in the system of status allocation among American blacks in the last decade and toward a more credible theory of the relationships among economic expansion, internal differentiation, and racial-ethnic stratification in modern (post-industrial) society. We will demonstrate that between 1962 and 1973 black men in both the North and the South made progress toward integration into the majority stratification system. Progress has been most rapid among northern-born blacks who, by 1973, experienced a process of stratification like that of the white majority. Southern-born men who migrate to the North are in part able to escape an environment relatively unfavorable to achievement as regards earning attainments but fail to achieve occupational status commensurate with their years of schooling. Blacks who remain in the South have experienced the slowest progress. By 1973 the stratification system characteristic of southern blacks was comparable in many respects with that of northern-born blacks in 1962. Our evidence indicates that the South has historically been slow in upgrading its educational system in response to the demand for better educated labor that has followed its postwar (World War II) economic development. This institutional lag means that by 1973 the status allocation system experienced by southern blacks closely resembled that for northern-born blacks a decade earlier.

DATA

The data for this analysis are drawn from the 1962 and 1973 surveys, "Occupational Changes in a Generation" (hereafter, OCG-I and OCG-II), which were carried out in conjunction with the March demographic supplement to the Current Population Survey (CPS) in those two years (Blau and Duncan 1967; Featherman and Hauser 1975). The 1962 survey had

a response rate of 83% to a four-page questionnaire which was left behind by the CPS interviewer. More than 20,000 men in the civilian noninstitutional population responded. In 1973, the eight-page OCG questionnaire was mailed out six months after the March CPS and was followed by mail, telephone, and personal callbacks. The respondents, comprising 88% of the target sample, included more than 33,500 men aged 20–65 in the civilian noninstitutional population. Also, in the 1973 sample blacks and persons of Spanish origin were sampled at about twice the rate of whites, and almost half the black men were interviewed personally. In this paper we draw age-constant intertemporal comparisons among black men in the postschooling, economically active years; therefore, we limit our analysis to men aged 25–64 in the experienced civilian labor forces of March 1962 and March 1973.[2]

LEVELS OF SOCIOECONOMIC BACKGROUND AND ATTAINMENTS

Southern-born men had lower socioeconomic backgrounds than native northerners in both 1962 and 1973, with this relative disadvantage increasing over the period (table 1).[3] Native northerners more typically came

[2]The population frequencies of these men by region of residence and nativity in 1962 and 1973 are shown in table 1. All statistics presented in this paper are based on a sample size weighted to reflect true population proportions and adjusted by a sampling design factor to reflect departures from a simple random sample. Our use of regional labels follows conventions of the U.S. Census: North is Northeast, North Central, and West, while South is South.

[3] Paternal education is scaled in years completed according to the following recode of class intervals: no school, 0.0 years; elementary (1–4), 3.3 years; elementary (5–7), 6.3 years; elementary (8), 8.0 years; high school (1–3), 9.9 years; high school (4), 12.0 years; college (1–3), 13.8 years; college (4), 16.0 years; college (5 or more), 18.0 years. Number of siblings is the number of brothers and sisters (not counting respondent). Farm origin is a dummy variable, with a score of 1 indicating that respondent's father was a farmer, farm manager, farm laborer, or farm foreman. Broken family is a dummy variable, with 1 indicating that the respondent was not living with both parents most of the time up to age 16. Respondent's education is in single years, as reported to the CPS. Father's and respondent's occupation are scored according to Duncan's socioeconomic index for occupations (Duncan 1961). We believe that occupational socioeconomic status is the major dimension along which occupational positions persist from generation to generation (Featherman et al. 1975). "Father's" occupation refers to the mother or other household head where the father was absent. Number of weeks worked by the respondent during the previous year is scaled in weeks according to the following recode of class intervals: None, 0.0 weeks; 1–13, 7.0 weeks; 14–26, 20.0 weeks; 27–39, 33.0 weeks; 40–47, 43.5 weeks; 48–49, 48.5 weeks; 50–52, 51.0 weeks. Earnings are expressed in constant (1972) dollars. Years of work experience are estimated by the difference between age at the time of the survey and age at first job, as a proxy for increments to "human capital" via on-the-job training over the work career, assuming constant annual discount and investment rates. To represent decay of human capital as a function of age (owing to declining health, physical and mental capacities, and the disincentives to retrain at older ages), we square the experience proxy. See Hauser and Featherman (1974) and Mincer (1974) for theoretical rationales for these constructions.

TABLE 1

MEANS AND STANDARD DEVIATIONS OF BACKGROUND AND ATTAINMENT VARIABLES BY RESIDENCE GROUP, BLACK MEN AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1962 AND MARCH 1973

	1962			1973		
VARIABLE	South	First-Generation North	Native North	South	First-Generation North	Native North
Father's occupation	14.39 (9.92)	16.58 (14.06)	21.45 (17.36)	13.82 (11.14)	15.69 (13.04)	23.73 (19.24)
Father's education	4.96 (2.30)	6.79 (4.12)	7.72 (3.89)	5.73 (3.62)	6.65 (3.78)	8.98 (3.66)
Farm background	0.59 (0.49)	0.43 (0.50)	0.25 (0.43)	0.48 (0.50)	0.40 (0.49)	0.08 (0.28)
Broken family	0.29 (0.46)	0.34 (0.48)	0.34 (0.48)	0.32 (0.47)	0.31 (0.46)	0.37 (0.48)
Number of siblings	5.51 (2.94)	4.93 (3.05)	4.45 (2.95)	5.28 (2.90)	5.34 (3.02)	4.05 (2.83)
Age	42.47 (10.67)	42.43 (10.34)	40.10 (10.62)	41.67 (11.24)	42.34 (10.60)	38.85 (10.14)
Age squared	1,917.16 (921.28)	1,907.03 (894.43)	1,719.79 (923 94)	1,862.92 (975.89)	1,905.20 (916.63)	1,611.45 (846.12)
Education	6.79 (4.14)	8.54 (3.54)	10.39 (3.22)	9.30 (3.83)	10.30 (3.04)	11.79 (2.71)
Occupation	15.42 (14.12)	17.97 (13.39)	25.36 (19.66)	22.64 (19.33)	25.29 (18.80)	36.46 (23.18)
Experience	25.08 (11.84)	24.24 (11.29)	21.79 (12.44)	22.67 (13.03)	23.00 (12.23)	18.77 (11.54)
Decay	768.80 (625.62)	714.43 (591.13)	628.10 (655.91)	683.45 (671.71)	678.45 (621.47)	485.23 (519.74)
Weeks worked	41.47 (15.00)	41.43 (15.47)	40.19 (17.18)	47.16 (9.75)	45.78 (11.84)	47.07 (10.72)
Earnings ($)	2,895 (2,304)	4,554 (2,577)	5,141 (4,160)	6,217 (4,595)	8,355 (5,096)	9,141 (5,455)
Population totals (thousands)	1,736	1,031	511	1,916	1,077	564

NOTE.—See n. 3 for definitions; standard deviations in parentheses.

from homes in which the father had more years of schooling and a higher-status occupation than did southern-born men.[4] The proportion with farm origins declined from 25% to 8% among the northern born over the period, while the percentage with farm backgrounds decreased from 59 to 48 among men still in the South and from 43 to 40 among first-generation northerners. Among the southern born, migration to the North indicated higher social origins, but this advantage declined over the period.

The native northerners had the highest education, and men currently residing in the South had completed the least schooling at both times. All groups increased their mean schooling between the surveys: The gains of both southern-born groups were about two years, and that of northern-born men was about a year and a half. These shifts narrowed educational differences among the groups, and within each group the variability of educational attainments declined over the period. Reductions in between- and within-group variability in schooling among blacks mirror national trends in educational inequality reported elsewhere (Hauser and Featherman 1976).

The changes in occupational attainments were of a very different sort. While each of the southern-born groups experienced about a seven-point increase on the Duncan scale, the native northerners gained 11 points. Northern-born blacks had an average Duncan score of 36.5 points in 1973—over 11 points higher than that of migrants to the North and 13 points higher than that of southern blacks. In contrast, the score of northern-born blacks is only about six points below the occupational status of white men nationally. Besides an increase in the differences among the residence groups over the period, the variation in occupational status within each group increased by about a quarter.

The earnings of the three groups (expressed in constant 1972 dollars) increased substantially over the 11 years between surveys, more than doubling for southerners and nearly doubling for the other two groups. Within-group variation in earnings also doubled, except for northern natives among whom it increased by one-third. It is in terms of mean earnings that the northern migrants do best relative to northern natives. This contrasts with their mean schooling, which is intermediate between the average educations of southerners and northerners, and with their mean occupational standing, which is nearly identical with the southerners' averages in both years.

Between 1962 and 1973 the average number of weeks worked increased

Intracohort comparisons within groups between surveys suggest that reported father's occupational level is artificially inflated. Some of the seeming decrease in father's occupational status between 1962 and 1973 is therefore an artifact. We found no reason to believe that this artifact differentially characterizes the residence groups. Therefore, the relatively improved social origins of native northerners vis-à-vis the southern-born men are not artifactual.

by about six weeks for southerners and northern natives and about four weeks for migrants. At the same time variation in number of weeks worked decreased by one-quarter to one-third. We infer that blacks in the labor force were more firmly integrated into the national economy than in the previous period, regardless of their current residence or region of origin. By 1973 the slight advantage of migrant blacks over native northerners with regard to the number of weeks worked had been reversed. Unemployment is a cyclical phenomenon, and unemployment statistics from the 1960 and 1970 censuses generally show more sustained employment among northern migrants in the labor force (Lieberson and Wilkinson 1976; Long and Heltman 1975).

The changes in weeks worked, taken together with increased socioeconomic attainments among each of the groups, suggest that the structural integration of blacks into the majority reward system is a pervasive phenomenon. The native northerners started from a relatively favorable socioeconomic position, and their integration is more extensive. By 1973 blacks native to the North had achieved virtual parity in mean schooling with whites in the nation as a whole; their average occupational status differed from that of whites by only six points; but their earnings were about $2,000 below the national white average. In comparison, the southern-born blacks made their most rapid gains in educational attainment and their slowest gains in occupational status (though they still gained on whites in this regard).

Such gross contrasts do not permit us to explain why these different rates of progress occurred, of course. It may be that the process which allocates persons to high or low statuses differs among the groups, with such differences changing over time. Alternatively, native northerners may be capitalizing on their more favorable social origins and achieved characteristics. To clarify these matters we shall examine separate regression models of attainments for each of the three groups. First, however, we turn our attention to the structure of mobility from father's to son's occupation for each of the groups (table 2).

OCCUPATIONAL ORIGINS, DESTINATIONS, AND THE STRUCTURE OF MOBILITY

Both southern-born groups experienced substantial structural mobility (minimum or net mobility)—about 44%—due to the changing occupational distributions from father to son. The minimum mobility required of native northerners to effect parity between the distributions of sons' and fathers' occupations is less than half that figure, indicating considerably greater similarity between the occupational distribution of fathers and sons among this group than among the men of southern birth. Since the three

TABLE 2

OCCUPATIONAL MOBILITY STATISTICS FOR BLACK MEN AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1973

	CHANGING MAJOR OCCUPATIONAL GROUP (%)*				
RESIDENCE GROUP	Minimum†	Observed‡	Expected§	Circulation Mobility‖	Mobility Index#
Observed matrix:					
South	44.6	84.5	89.2	39.9	89.5
First-generation North	43.8	87.6	88.9	43.8	97.1
Native North	21.8	82.4	84.1	60.6	97.3
Matrix adjusted to southern margins:					
South	44.6	84.5	89.2	39.9	89.5
First-generation North	44.6	89.0	89.2	44.4	99.5
Native North	44.6	87.0	89.2	42.4	95.1

* The major occupation groups are defined as professional, technical and kindred, managers, officials, and proprietors; sales and clerical; craftsmen; operatives; service; farmers and farm managers; farm laborers; and nonfarm laborers.

† Net mobility; index of dissimilarity comparing row and column marginals.

‡ Percentage off main diagonal.

§ Percentage off the main diagonal under model of independence of rows and columns.

‖ Circulation mobility = (observed − minimum).

\# Mobility index = $\left(\frac{\text{observed} - \text{minimum}}{\text{expected} - \text{minimum}}\right)$.

groups have roughly similar levels of observed mobility, circulation mobility (i.e., the arithmetic difference between observed and structural mobility) is about 50% higher among native northerners. There is rather little tendency toward inheritance of a father's major occupational classification among any of the groups beyond that expected on the basis of chance.

A large part of these differences is attributable to the dissimilar origin and destination margins of the groups, particularly to the percentage in each which stems from farm origin. Group variation in that percentage produces characteristic patterns of recruitment to sons' occupations, but these group patterns of inflow attenuate or disappear when the mobility tables of the northern groups are adjusted to the margins of the southern matrix as a standard. While the mobility indices for these standardized (adjusted) matrices appear to indicate differences in the degree of intergenerational occupational inheritance (see last column of the lower panel of table 2), a rigorous test of differences among the groups in intergenerational mobility or stability of occupation fails to reject null ($\chi^2_{LR} = 6.71$ with 2 df).

A log-linear analysis of the observed intergenerational matrices (table 3) indicates a group-constant association of origin and destination occupations across generations that is due in part to a tendency toward occupational inheritance and in part to noninherited socioeconomic association of origin and destination occupations, controlling for marginal variation

TABLE 3

LOG-LINEAR TESTS OF VARIATION IN MOBILITY FROM FATHER'S OCCUPATION TO OWN OCCUPATION, BLACK MEN AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1973

Model*	χ^2_{LR}†	df	P	Δ‡	χ^2_H/χ^2_T§
A. Total occupation matrix:					
1. [F][O][R] (baseline)	714.08	175	.000	19.34	100.00
2. [FR][OR] (occupation margins vary by group)	264.85	147	.000	11.57	37.09
3. [FR][O] (origin margin varies by group)	448.31	161	.000	15.89	62.78
4. [OR][F] (destination margin varies by group)	530.63	161	.000	16.90	74.31
5. [FR][OR][FO] (occupation margins vary by group and place-constant interactions)	65.16	98	>.5	5.68	9.13
6. A4 vs. A2 (net [FR])	265.78	14	.000	5.33	37.22
7. A3 vs. A4 (net [OR])	183.46	14	.000	4.32	25.69
8. A5 vs. A2 (net [FO])	199.69	49	.000	5.89	27.96
B. Occupation matrix with main diagonal blocked:					
1. [FR][OR]	175.43	123	.000	8.71	24.57
2. [FR][OR][FO]	55.98	82	>.5	4.84	7.84
3. B2 vs. B1 (net [FO])	119.45	41	.000	3.87	16.73
C. Hierarchical decomposition:					
1. B3 vs. A8 (net [FO] due to inheritance)	80.24	8	.000	2.02	11.24
2. B2 vs. A5 (net group differences in inheritance)	9.18	16	>.5	0.84	1.29

* F = father's occupation (professional, technical, and kindred; managers, officials, and proprietors/sales and clerical/craftsmen/operatives/service/farmers and farm managers/farm laborers/nonfarm laborers); O = own 1973 occupation (same as father's occupation); R = residence group (South/first-generation North/native North).

† Likelihood ratio χ^2.

‡ Index of dissimilarity.

§ χ^2 null as a percentage of total baseline χ^2.

among the groups. Fewer than 6% of the cases are misclassified by a model allowing marginal variation by group and group-constant association of origin and destination-occupation distributions. We find no group differences in occupational mobility, whether as regards gross father-son associations, the nature of inheritance of occupations, or in the likelihood of upward or downward mobility as compared to stability.

This observation of the essential invariance in the occupational association of fathers and sons among the three groups of blacks comes as no particular surprise, given a similar finding about apparent differences between black and white mobility matrices (Hauser, Featherman, and Hogan 1977, chap. 8), and intertemporal changes in the mobility of U.S. men (Hauser et al. 1975*b*). Thus, one reason for superior occupational attainments of northern natives is their superior occupational origins (Hauser et al. 1975*a*); another is their superior educational attainments.

PROCESSES OF EDUCATIONAL ATTAINMENT

Models of educational attainment (table 4) display the familiar relationship of years of schooling to social origins (e.g., Hauser and Featherman 1974). In all groups in both years, higher social origins (e.g., father's occupation and education) are translated into the ability to continue schooling, whereas a farm background, broken family, and large number of siblings are handicaps to extended schooling. For each group the negative impact of farm background was reduced by about a year or more

TABLE 4

REGRESSION ANALYSIS OF EDUCATIONAL ATTAINMENT BY RESIDENCE GROUP, BLACK MEN AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1962 AND MARCH 1973

Variable	1962 South	1962 First-Generation North	1962 Native North	1973 South	1973 First-Generation North	1973 Native North
Father's occupation	0.058	−0.016	0.003	0.026	0.006	0.007
	(0.025)	(0.019)	(0.021)	(0.010)	(0.009)	(0.009)
Father's education	0.268	0.224	0.238	0.342	0.225	0.145
	(0.074)	(0.066)	(0.094)	(0.033)	(0.033)	(0.049)
Farm background	−2.429	−2.607	−3.117	−1.665	−1.524	−1.853
	(0.494)	(0.536)	(0.832)	(0.230)	(0.258)	(0.555)
Broken family	−0.998	−0.620	−0.439	−0.536	−0.733	−0.532
	(0.519)	(0.545)	(0.708)	(0.235)	(0.250)	(0.309)
Number of siblings	−0.128	0.027	−0.103	−0.060	−0.075	−0.145
	(0.082)	(0.087)	(0.120)	(0.038)	(0.040)	(0.054)
Intercept	7.065	8.466	9.870	8.275	9.946	11.265
R^2	.236	.226	.382	.236	.217	.164
Error of estimate	3.657	3.163	2.633	3.355	2.705	2.497

NOTE.—See n. 3 for definitions; approximate SEs in parentheses.

between 1962 and 1973. Consequently, over the same period in which farm origin was becoming a less common characteristic across groups, it was becoming a less important handicap to the educational achievement of blacks. In both years blacks in the South were able to capitalize on higher father's occupational status (and, conversely, to be more heavily penalized by the circumstances of low birth), whereas neither group in the North had been able to do so. This differential did decline between 1962 and 1973, and in any case it may be a reflection of the differential prevalence of a male head of household and of an occupation to be reported for the head. The increment to schooling from an additional year of father's (head's) education was about a quarter-year in 1962 for each group. By 1973 the southern men attained about a third of a year of schooling for each additional year of father's education, while among native northerners this relationship was only half as strong.[5] In all groups, educational inequality declined within categories of social origin (see errors of estimate in table 4), and absolute variability in schooling decreased (see standard deviations in table 1). Among blacks native to the North, inequality among levels of social origins also was reduced, loosening the relevance of such ascriptive categories for educational achievement (see R^2 values in table 4). This group recapitulates the intercohort pattern in the majority population (see Hauser and Featherman 1976).[6]

Most of the educational advantage (as given in the comparison of mean years of school completed) of native northerners over southerners is due to their more favorable social origins (table 7).[7] To the extent that the processes of educational attainment differ among the groups and over time, the northern natives are more similar to the majority population (nonblacks) than are southern-born blacks. The differences between migrants

[5] These models of educational attainment, as well as those of occupational attainment, were also estimated for men aged 25–64 separately by residence group with the inclusion of control variables for age and age squared. The inclusion of such controls did not change the overall conclusions reached. Controls for age composition did tend to increase levels of explained variance substantially while also causing erratic fluctuations in the intercepts of the equations. We therefore chose to present the structural models of attainment excluding these controls (tables 5 and 6). In decomposing differences among the groups and intercohort shifts within groups (tables 7 and 8) we do include age controls, so that the small differences in age composition cannot contaminate our estimates.

[6] The education, occupation, and earnings attainment structural equation models were also estimated for men aged 25–34. The intracohort comparisons across residence groups in 1973 replicated the findings reported here for men 25–64. We do not report these findings in detail, nor do we attempt intracohort comparisons within residence groups between 1962 and 1973, because of the small samples such comparisons would entail.

[7] Without controls for age, differences in social origin account for 60% of the superior education of first-generation northerners in 1962 but only 51% by 1973. The comparable figures for northern natives are 58% and 84%. Standardization was carried out on the regression equations for southerners, into which the means for the other two groups (northern natives and first-generation northerners) were inserted.

TABLE 5

REGRESSION ANALYSIS OF OCCUPATIONAL ATTAINMENT BY RESIDENCE GROUP, BLACK MEN AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1962 AND MARCH 1973

	1962			1973		
VARIABLE	South	First-Generation North	Native North	South	First-Generation North	Native North
Father's occupation	0.043	0.055	− 0.029	0.179	0.119	0.139
	(.089)	(0.080)	(.147)	(.050)	(0.058)	(.071)
Father's education	0.208	0.170	− 0.015	0.352	0.378	− 0.447
	(.273)	(.281)	(.690)	(.168)	(.216)	(.392)
Farm background	− 2.386	1.858	0.366	− 0.056	0.419	1.281
	(1.856)	(2.378)	(6.428)	(1.143)	(1.657)	(4.437)
Broken family	− 0.602	1.888	− 0.548	− 0.812	− 0.225	0.483
	(1.877)	(2.256)	(4.959)	(1.143)	(1.565)	(2.435)
Number of siblings	0.150	− 0.240	− 0.500	− 0.350	− 0.159	− 0.177
	(.297)	(.359)	(.844)	(.186)	(.250)	(.429)
Education	1.122	1.040	2.675	2.364	2.433	4.616
	(.231)	(.336)	(.886)	(.154)	(.262)	(.469)
Intercept	6.900	6.762	0.614	− 1.684	− 3.396	−16.805
R^2	.156	.088	.205	.299	.208	.302
Error of estimate	13.131	13.047	18.371	16.236	16.824	19.567

NOTE.—See n. 3 for definitions; approximate SEs in parentheses.

TABLE 6

REGRESSION ANALYSIS OF EARNINGS ATTAINMENT BY RESIDENCE GROUP, BLACK MEN AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1962 AND MARCH 1973

	1962			1973		
VARIABLE	South	First-Generation North	Native North	South	First-Generation North	Native North
Father's occupation	−3	−16	−31	−14	35	−18
	(13)	(13)	(32)	(12)	(16)	(17)
Father's education	7	59	5	−0	4	69
	(40)	(46)	(150)	(41)	(59)	(95)
Farm background	−295	−712	95	−512	632	819
	(268)	(387)	(1,368)	(281)	(444)	(1,078)
Broken family	253	−441	−642	−75	−284	22
	(272)	(368)	(1,038)	(280)	(419)	(597)
Number of siblings	−30	1	37	−9	−41	24
	(43)	(58)	(176)	(46)	(67)	(104)
Education	187	−99	120	228	300	409
	(37)	(62)	(202)	(47)	(83)	(136)
Occupation	26	23	42	66	50	57
	(9)	(13)	(28)	(8)	(11)	(15)
Experience	36	7	74	30	135	106
	(45)	(65)	(155)	(39)	(61)	(85)
Decay	−0	−0	−2	−0	−2	−2
	(1)	(1)	(3)	(1)	(1)	(2)
Weeks worked	65	101	112	120	140	169
	(8)	(11)	(30)	(13)	(16)	(27)
Intercept	−1,982	1,135	−1,638	−2,916	−4,443	−7,253
R^2	.409	.396	.338	.258	.234	.280
Error of estimate	1,811	2,072	3,684	3,978	4,499	4,712

NOTE.—See n. 3 for definitions; approximate SEs in parentheses.

TABLE 7

COMPONENTS OF DIFFERENCES BETWEEN SOCIAL AND ECONOMIC ATTAINMENTS OF FIRST- AND SECOND-GENERATION NORTHERN MEN AND SOUTHERN MEN, BLACKS AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, MARCH 1962 AND MARCH 1973

Attainment and Components	1962		1973	
	First-Generation North	Native North	First-Generation North	Native North
Education:				
Social origins and age..	0.84 (48)	2.07 (57)	0.34 (34)	1.98 (80)
Residual..............	0.91 (52)	1.53 (43)	0.66 (66)	0.51 (20)
Total difference.....	1.75 (100)	3.60 (100)	1.00 (100)	2.49 (100)
Occupational status:				
Social origins and age..	1.98 (78)	3.83 (39)	1.80 (68)	8.31 (60)
Education....	0.95 (37)	1.75 (18)	1.66 (63)	1.17 (8)
Residual..............	− 0.38 (−15)	4.36 (44)	− 0.81 (−31)	4.34 (31)
Total difference....	2.55 (100)	9.94 (100)	2.65 (100)	13.82 (100)
Earnings:				
Social origins..........	262 (16)	505 (22)	260 (12)	1,063 (36)
Labor force attainments and attachment.....	191 (11)	389 (17)	12 (1)	459 (16)
Residual..............	1,207 (73)	1,352 (60)	1,865 (87)	1,402 (48)
Total difference.....	1,660 (100)	2,246 (100)	2,137 (100)	2,924 (100)

NOTE.—The structural equations for southern residence blacks are used for the standardization; percentages in parentheses.

TABLE 8

COMPONENTS OF CHANGE IN SOCIAL AND ECONOMIC ATTAINMENTS BETWEEN 1962 AND 1973, BLACK MEN AGED 25–64, EXPERIENCED CIVILIAN LABOR FORCE, BY RESIDENCE GROUP

Attainments and Components	South	First-Generation North	Native North
Education:			
Social origins and age...	0.39 (16)	− 0.01 (−1)	0.54 (39)
Residual............................	2.11 (84)	1.77 (101)	0.86 (61)
Intercohort change..................	2.50 (100)	1.76 (100)	1.40 (100)
Occupation:			
Social origins and age.................	1.08 (15)	− 0.25 (−4)	2.13 (19)
Education......	5.30 (73)	4.58 (63)	3.94 (35)
Residual............................	0.84 (12)	2.99 (41)	5.04 (45)
Intercohort change..................	7.22 (100)	7.32 (100)	11.11 (100)
Earnings:			
Social origins	240 (7)	−80 (−2)	188 (5)
Labor force attainments and attachment	1,517 (46)	1,366 (36)	1,998 (50)
Residual............................	1,565 (47)	2,515 (66)	1,814 (45)
Intercohort change..................	3,322 (100)	3,801 (100)	4,000 (100)

NOTE.—The structural equations for 1973 are used for the standardization; percentages in parentheses.

from the South and other southern blacks are more difficult to assess precisely, since we have no data on the region in which the migrants received their education.

We venture an interpretation of these data on educational achievement which argues that blacks native to the North have experienced greater structural integration into the majority socioeconomic system than have both southern-born groups. Whether one compares the mean length of schooling or the processes which convert the resources of socioeconomic background into education, it is the northern natives who more closely approximate the levels and processes of achievement among whites. Indeed, by 1973 the total gap in schooling between native black and white northerners was just over one-half year, and of this, virtually all (96%) of the difference represented the residual disadvantages of lower paternal occupation, education, and the like among blacks.

It would appear that the North has set the pattern for change in the socioeconomic system over the period of our inquiry, and it is in the North that such change has most clearly altered the socioeconomic relationships between the races. We marshall evidence in behalf of this thesis throughout the paper, but suffice it here to call attention to two conclusions from the analyses of educational achievement. First, in 1973 (but to a lesser degree in 1962) the cross-sectional comparisons among the residential groups resemble the intercohort trends reported elsewhere for the nation as a whole (i.e., Hauser and Featherman 1976; Featherman and Hauser 1976). That is, the intertemporal shifts in educational inequality and in the articulation of social origin and schooling are much like the time-constant intergroup comparisons of the black northern natives and the southern blacks: educational inequality is less in the North, both absolutely and as conditioned by social origins, and educational achievement is somewhat more random with respect to social origins in the North (save in 1962, when the impact of farm origins appears to weigh heavily in the value of R^2, relative to other characteristics of social origin).

Second, first-generation northern blacks are intermediate to the native northerners and to the southerners they left behind. That is, the regression coefficients for migrants to the North in the education equations of table 4 tend to lie between the coefficients for the other two groups. Further, the pattern of absolute and conditional inequality in schooling shows the first-generation northerners to have less variability than the southerners but more than the native northerners. Inasmuch as we know migration is selective but lack information on the timing of migration relative to the completion of schooling (so that we cannot infer the region of schooling for the migrants), it is impossible to comment further on regional bases for differential educational achievement. It is useful to anticipate ourselves at this point, however. The achievements of northern migrants in

earnings and occupational standing, relative to the other groups, suggest that the North continues to provide blacks with greater opportunities for socioeconomic integration. And this regional difference in the structure of opportunity has a bearing on black-white relations which is apart from any competitive advantage for achievement which falls to those "selected" as *migrants.*

It is important to note that intercohort upgrading of education between 1962 and 1973 in all regions and black groups stems primarily from exogenous secular increases in mean levels of schooling, rather than from any dramatic shifts in the distributions of social origins or major change in the relationships between socioeconomic background and schooling. The relevant decomposition appears in table 8, upper panel. Here we standardize on the 1973 regression equations for each group and insert the appropriate differences in means (1962 vs. 1973) into the equation. For example, among southerners, the intercohort shift of 2.50 years of education hardly reflects compositional change. Rather, 2.11 of the 2.50 years represent change in the regression equations linking social origins to schooling. But an examination of these equations in table 4 indicates that a major source of change in coefficients is in the regression constant. Where change in process has occurred, it has involved the declining role of farm background. Again, the rather stable set of net associations among characteristics of socioeconomic background and schooling within each of the three black groups is consistent with previous analysis of the national population at large (Hauser and Featherman 1976).

PROCESSES OF OCCUPATIONAL STATUS ATTAINMENT

We have described the upgrading of occupational status for each group, most apparent among native northerners, and the increase in variance in occupational status for blacks between 1962 and 1973. In part because blacks ceased to be clustered at the bottom of the occupational hierarchy as unskilled labor, by 1973 there was a stronger linkage of occupational status and both social origins and education (table 5).

In 1962 black men's social origins (as indicated by fathers' education and occupation, farm background, intactness of parental family, and number of siblings) were unrelated to their occupational attainments, once educational differences were controlled. Among both southern-born groups an additional year of schooling converted into one additional point on the socioeconomic index of occupational status, whereas the net effect of schooling was more than twice as large for blacks native to the North. The low levels of explained variance among both southern-born groups ($R^2 \simeq .08$–$.16$) in 1962 are attributable to the essential invariance of occupational attainment by social origins and education. This situation

contrasts with that of the native northerners, for whom the relationship between schooling and occupational achievement is stronger and for whom the absolute variance in occupational status is 40% greater than in the southern group.

By 1973 there was a much closer articulation of occupational status with social origins and education among blacks throughout the nation (Featherman and Hauser 1976), but the degree of this intercohort change varied by residence group. Across all groups the structure of family of origin (viz., sibship size, intactness) remained nonsignificant, as did the effects of a farm origin and paternal education, net of father's occupational status. But by this latter date the net effect of father's occupational status had increased. Table 5 shows the largest intercohort increase among blacks native to the North (— 0.029 vs. 0.139), but the effect of paternal occupational status is statistically significant across all groups, even among blacks of equivalent schooling. The biggest intertemporal change is in the returns to schooling in the form of occupational status, which more than double for each southern-born group and which nearly double for the northern natives. In 1973, among both southern-born groups an additional year of schooling increased occupational status by about 2.5 points, while the return was 4.6 points among the northern natives. Among all whites (nonblacks) aged 25–64 in the ECLF of 1973 the occupational return to an additional year of schooling is 4.3 points (Featherman and Hauser 1976).

Thus, the superior occupational attainments of native northern blacks compared with southern-born blacks are due to their relatively advantageous social origins, superior educational attainments, and greater capacity to translate that schooling into occupational attainments in the northern labor market. Because their occupational returns to education are similar to those of whites in the nation as a whole, and because their mean level of educational attainment is only slightly below the white average, the occupational level of these second- (and third-, etc.) generation northern blacks is only six points below the mean Duncan score of all nonblacks (42.58 vs. 36.46). We hasten to remind the reader that our comparison is among men in the ECLF and therefore excludes men not in the labor force.

An exact decomposition of the sources of the occupational status differences among the three black groups (table 7) indicates that the advantage of migrants over southerners, 2.6 points on the Duncan scale, is accounted for entirely by their superior social origins and schooling in both survey years. The regression models of table 5 indicate no significant differences in occupational returns to socioeconomic background and education between these two southern-born groups. The negative residual in the decomposition in table 7 stems from the slightly lower occupational

attainments of migrants to the North compared with men of equivalent social origins and mean schooling who remain in the South. This finding is rather surprising in that it indicates that movement of blacks from the South to the North failed to upgrade occupational attainments of the first generation, relative to comparable men who stayed behind.

In stark contrast stand the northern-born blacks. In 1962 only about 56% of their 10-point Duncan-score advantage over southerners was due to superior social origins and education; the remaining 44% represented higher occupational returns to additional schooling and other differences in the process of occupational stratification between the groups. By 1973 about 4.3 points of the higher mean status of these men are due to higher returns to education, while fully 9.5 points of the 13.8-point advantage are attributable to their superior social origins (8.3 points) and educational attainments (1.2 points). Native northerners are thus born into relatively advantageous social origins and are able to translate these into superior educational attainments, which in turn provide substantially greater occupational returns than accrue to either southern-born group.[8]

Rising mean education in the North and the South is important in accounting for the intercohort improvements in occupational standing from 1962 to 1973 among all categories of blacks, but especially among the southern-born men (see table 8). Increased returns to education (but also socioeconomic background, especially paternal occupation) account for about two-fifths of the upgrading of occupational achievement for both groups residing in the North. This decomposition of change suggests that the educational system in the South has lagged behind that in the North in providing blacks with the amount and quality of schooling which is requisite for employment in an urban-industrial market such as has obtained in the North for some decades. By 1973, the South had begun catching up with the North in industrialization, and the educational system and the schooling of blacks in the South seem to have changed commensurately. For example, the greatest intercohort shift in mean education across all groups is for the South (table 8). Such relatively rapid changes in the educational composition of the southern black population show themselves in the more forceful bearing of shifts in schooling on occupational change in the two southern-born groups than in the group native to the North. The northern school systems may have provided a set of credentials for black workers in an urban-industrial setting at an earlier date, so that the smaller intercohort shift in mean education was a lesser factor in the rather large rise (11.1 points on the Duncan scale) in mean occupational achievement for blacks native to the North. It would appear that northern labor markets

[8] Without controls for age, differences in social origins and education account for 107% of the superior occupational attainments of migrants in 1962 and 114% in 1973. The comparable figures for northern natives are 56% and 67%.

were first to respond somewhat universalistically to a growing supply of educated blacks. Indeed, we tend to see the South as lagging behind the North by almost a decade (compare the means and standard deviations for education and occupation in table 1 for native northerners in 1962 with equivalent statistics for southerners in 1973; compare the regression equations and related statistics in table 5 for these same two groups).

Again, the evidence suggests that blacks native to the North are first to experience shifts in their relative positions in the system of socioeconomic stratification—shifts which occur subsequently for blacks in other regions. This interpretation is consistent with the idea that structural integration of working blacks into the economy may follow (or at least be correlated with) economic development and change.

Migrants to the North—the first-generation northern blacks—are in apparent anomaly in this analysis and interpretation of occupational change. Relative to men of equal qualification and social origins in the South, the first-generation northerner suffers a loss in occupational standing by migrating. Yet this loss must be seen in a more complete context. On the one hand, had they remained behind these relatively more highly educated southerners might not have been able to secure jobs commensurate with their resources, given the now larger supply of labor and (presumably) a constant demand. On the other hand, migration to the North can be seen as an investment, at least for the schooling and returns to northern education for the children—the second generation. And as we report in the next section, regional differences in earnings may in themselves account for this apparent "willingness" of migrants to take an occupational status "loss."

PROCESSES OF EARNINGS ATTAINMENT

Our models indicate the presence in 1962 of few regional differences in the process of earnings attainments among the groups (table 6). By 1973 there were substantial differences in that process, and the three groups do differ in levels of earnings if one controls for social origins, education, occupation, years of experience, and number of weeks employed during the year. Neither survey shows social origins to have direct effects on a man's earnings regardless of his residence group.

In 1962 an additional year of schooling or an additional point of occupational status increased earnings significantly only among southern men. For both groups of northern men there is no clear evidence that either a higher education or higher-status job increased earnings. The number of weeks worked during the year is the only important determinant of a man's annual earnings, producing the high levels of explained variance in our models. An examination of equations which omit this variable produced roughly similar estimates of returns to occupation, education, and social origins, so

these models are not shown. At this earlier time an additional week's work increased earnings by $100 for a black working in the North but only $65 for one working in the South, indicating the crucial role played by regional differences in wage rates.

By 1973 both the effect of schooling on earnings and the effect of years of labor force experience had increased substantially for each group, as did constant-dollar returns to occupational status. Native northern blacks appear to have reaped greater returns to investments in education than either southern-born group. The northern natives earned less for each additional year of labor force experience than did the migrants, but the major contrast is the lack of any returns to experience among men remaining in the South. This might be related to differing industrial compositions of the regions and to lower levels of unionization in the South. The native northerners continued to enjoy higher weekly earnings than either southern or first-generation northern men, with the position of the migrants relative to the native northerners declining somewhat. The net earnings for an additional week of work during 1972 were $120 for southerners, $140 for first-generation northerners, and $169 for northern natives.

When the $786 earnings advantage of the natives over the migrants is decomposed (standardizing on the equations for the migrants), $772 is accounted for by their superior social origins and $438 by their superior attainments and labor force experiences. This leaves a residual of —$424, indicating that black natives earn less than would migrants to the North were they to enjoy similar characteristics. This process difference is not accounted for by the returns to education, occupation, or weeks worked, since such returns are at least as high for natives of the North. The small advantage of migrants to the North may be due to more hours of work per week (unspecified in our models) or to a greater likelihood of employment at a second job or at multiple part-time jobs. It may be that migrants to the North who fail to achieve relatively high earnings return to the South, leaving behind a pool of men doubly selected for success among first-generation northerners—men with whom northern natives compare unfavorably (Lieberson and Wilkinson 1976). At any rate, this is the same finding observed by Long and Heltman (1975) and others. Our more extensive models of earnings have failed to locate the source of this seeming anomaly. As part of the OCG-II project, a replicate survey of black men in the Milwaukee metropolitan area was conducted. This survey obtained a variety of social-psychological measures relating to attitudes toward work as well as a variety of other information that might prove useful in accounting for a man's earnings. We hope to make use of such information in future research in order to discover the sources of the higher earnings intercepts of migrants to the North over native northerners.

For 1962 about three-quarters of the earnings advantage of migrants

to the North over southerners (see table 7) is due to differences in the process of earnings attainments, with the remainder attributable to higher social origins and socioeconomic attainments. The comparable figure in the comparison with southerners is 60% among native northerners. By 1973 these patterns were more divergent, with 87% of the advantage of migrants to the North but less than half of the northern-born advantage due to differences in the earnings attainment processes. Whereas in the attainment of occupational status the first-generation northerners gained no advantage over that provided by their higher social origins and schooling by moving from the South, nearly all of their advantage in earnings over men remaining in the South is attributable to the higher returns to schooling and experience and to the wage structure obtaining in a northern setting.

About equal proportions of the increase in earnings from 1962 to 1973 are due to improvements in labor force attainment and attachment (including education and all other variables in our models except those indexing social origins) and to changes in the process of earnings attainment among men living in their region of birth (table 8). Among the migrants, fully two-thirds of the improvement in earnings over the period is due to intercohort shifts in the process of earnings attainment, including secular changes in earnings levels between the periods. The migrants, whose educational and occupational attainments are slightly higher than those of southerners, and whose average number of weeks worked are slightly fewer, gain roughly the same dollar increases in earnings due to upgrading in such characteristics as do the southerners ($1,517 vs. $1,366 from table 8). Their superior overall improvements in earnings are attributable to more favorable changes in process over time than those experienced by southerners, perhaps indicating that the integration of blacks into the earnings reward system is proceeding more quickly among the migrants.[9]

Again, we find in these data a basis for our argument that the South lags behind the North in the structural integration of black workers. A simple inspection of the mean earnings (table 1) shows a monotonic increase both within year from South to native North and across years and residence groups (from 1962 South to 1973 native North). Since these figures are in constant (1972) dollars, they reflect estimates of levels and growth in productivity, and as such they suggest that the South of 1973 is not unlike the North (as given in the data for natives) in 1962. A similar interpretation follows from an analogous reading of the lines in table 1 for mean education and occupational socioeconomic status. Doubt-

[9] We performed an analysis of the determinants of relative earnings position (log earnings) parallel to the earnings analysis. The results were roughly similar to those for earnings, but their addition here seemed uninformative and uninteresting. We therefore chose to omit these log earnings models from our discussion.

less these figures represent different industrial compositions and changes therein by region. But they are coincident with other important regional differences, as is demonstrated by our results for earnings among black men in the experienced labor force. Both absolute variability in earnings (see standard deviations in table 1) and variability conditioned on socioeconomic background, education, occupation, and labor experience (see errors of estimate in table 6) are greater in the North than in the South, with the clearest contrast between the men residing in their regions of birth. Opportunities for economic achievement for blacks are more prevalent in the North, and that in itself may be a sufficient inducement for migration to the North. But for second- and third-generation natives in the North, the process of earnings attainment is based on universalistic grounds to a far greater degree than in the South, even in 1973. Indeed the contrast in the coefficients for education and experience in table 6 is more striking for the two native groups in 1973 than in 1962. If the first-generation northerners suffer, it is largely in their economic (and occupational) returns to schooling. This may not signal the ineffectiveness of "universalism" for this group as much as it may the allegedly poorer quality of southern education for an urban-industrial market. (Alternatively, it may reflect the disutility of southern linguistic patterns in the North, the effect of ghetto segregation of recent migrants on their knowledge of and success in northern markets, and other hypothetical contingencies which "discount" the level of black schooling within the first generation.)

SUMMARY AND CONCLUSIONS

Previous research has demonstrated a convergence of the educational and occupational attainments of blacks with those of whites. Part of this convergence is attributable to improved social origins (in the case of education), but in part it is due to the development of a system of stratification across generations among blacks that is more like the process of status allocation characteristic of the majority population. Between 1962 and 1973 age-constant comparisons of blacks indicate an increase in mean levels of education, occupation, and earnings, with a lesser degree of educational inequality and a greater amount of occupational and earnings variability by 1973. This pattern provides suggestive evidence of increased internal differentiation and socioeconomic inequality within the black minority, raising the issue of the uniformity of the stratification system and the crystallization of rewards among different groups of blacks during the course of economic expansion. In particular, we have sought to address the question whether blacks have in recent years been able to escape an environment unfavorable to the attainment of education, occupation, and earnings by migration from the South to the urban-industrial centers of

the North. Furthermore, did such migration improve the operation of universalistic criteria in the attainment of occupational and earnings rewards? Finally, do the experiences of blacks who were born in the North differ, either favorably or unfavorably, from those who migrated to the North?

This paper has provided complex, but generally affirmative, answers to these questions. Native northern blacks have educational attainments superior to those of blacks in the South, but this advantage has been decreasing through time. By 1973 most of the educational advantage of the Northern natives was attributable to their more favorable social origins, in particular, to the smaller proportion which is of farm origin. The educational attainments of the southern-born men who migrate to the North were intermediate to those of the other two groups. A part of their advantage over men who remained in the South is a result of social origins, but a portion of the difference is an artifact of the self-selection of better-educated men as migrants. By 1973 the process of educational attainment characterizing native black northerners did not differ from that of the American white majority.

Blacks from all three groups experienced a similar degree of socioeconomic association of occupations between father and son. Because so many of the southern-born and so few of the northern-born men had farm backgrounds the occupational distributions of fathers and sons differed most among the former men, and the required and observed levels of occupational mobility were greatest among them. The heavy representation of men of farm background among the southern born results in part in their disproportionate concentration in low-status occupations.

Between 1962 and 1973 there was considerable upgrading of the occupational status of blacks in each group, although by far the largest improvements were among northern natives. Likewise there were increases in the variability of occupational status over the decade for each group. The greater articulation of social origins and education with occupational attainment characteristic of blacks as a whole is a pervasive phenomenon experienced by each group. But the changes, both in levels and in process, were most dramatic among the northern natives. Not only did these men enjoy higher social origins and education than those born in the South, but their ability to capitalize on such characteristics increased over the decade. By 1973 both the process of occupational attainment and the ultimate levels of attainment of native northern blacks were more like those of the American white majority than those of either southern-born black group. In contrast, the slight advantage of the migrants to the North over southerners is attributable entirely to more favorable social backgrounds.

Finally, the earnings models indicated an increased operation over the

interval of meritocratic criteria such as education, occupation, and number of weeks employed in the determination of earnings among each of the groups. By 1973 the major reason other than favorable origins and education for the higher annual earnings of men in the North was the superior wage and salary rates paid in that region. Migration to the North may have increased the operation of universalistic achievement criteria for earnings only slightly, but it did permit the migrants to enjoy the advantage of higher northern pay scales.

As the theories of race relations reviewed at the beginning of this paper led us to expect, southern black men have been able to increase their own earnings by moving to the North, even though they have failed to upgrade their occupational achievements. Additionally, men who move to the North and have children born to them there are able to assist their children in capitalizing on better social origins to complete more years of schooling. The second generation is able to translate these advantages into superior occupational achievements because they are more subject to universalistic criteria of achievement than are men born in the South, *whatever their region of residence.* They also enjoy higher rates of earnings characteristic of the North—rates similar to those paid men who migrate North.

The present analysis provides evidence that economic expansion has been coterminous with the internal differentiation of the black population, with the development of more distinct socioeconomic strata, with the greater stability of inequalities between generations of black men, and with gains toward the socioeconomic integration of experienced workers. That these changes have been more extensive in the North and have predated similar developments in the South is consistent with our speculations about the sources and direction of recent changes in the socioeconomic stratification of the races.

In terms of the dynamics of racial inequality, the northward migration of blacks provides some immediate benefits while also increasing the ultimate proportion of the next generation that is northern born. These second-generation northerners experience processes of stratification or reward allocation that are much more similar to those of the white majority than are those of any other blacks. It is among such men that the convergence of stratification processes between the races, as well as convergence of attainment levels, has been disproportionately concentrated. The same trends have been typical of southern-born blacks but are not nearly as pronounced. In the past several decades, the migration of blacks to the North has reduced interracial inequalities in the attainment of education, occupation, and earnings while increasing the degree of intraracial inequality.

Current trends toward the convergence of the industrial and occupa-

tional structure of the South with that of the North may open new opportunities for the structural integration of black workers (e.g., McKinney and Bourque 1971). To the extent that the consequences for integration of such convergence may take time to appear, our analysis would not have detected it.

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Some Comments on the Causal Analysis of Surveys with Log-linear Models[1]

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Recent developments in the analysis of cross-classified data have given social scientists powerful tools for investigating relationships among qualitative variables. Since investigators can analyze cross-classifications with concepts similar to those used in regression and path analysis, they may feel that the level of measurement problem has been solved. This study shows, however, that at least one approach, log-linear analysis, does not by itself solve every measurement problem. Unless the technique is applied carefully, it may produce highly misleading results. The conditions under which log-linear models can be misleading are explored within the context of causal analysis of surveys.

Like their colleagues in the natural sciences, social scientists want to build models. Unfortunately, because their subject matter is human behavior they face formidable problems, particularly in regard to measurement. Investigators frequently have to measure complex phenomena such as attitudes with rather crude classification schemes. In the past, purely categorical data have been difficult to analyze, especially when the analyst's goal was the construction of multivariate models. Faced with these difficulties social scientists have relied on relatively simple procedures—test-factor stratification, for example (Hyman 1955; Rosenberg 1968)—or ignored the level of measurement by treating category scores as if they were interval scales (Labovitz 1970).

Thus, recent developments in contingency table analysis seem to be a godsend. For they appear to put the statistical investigation of cross-classifications on roughly the same footing as the analysis of quantitative data. Leo Goodman, for instance, describes in a series of articles a model-building technique for multidimensional tables (see Goodman 1970, 1971, 1972*a*, 1972*b*, 1973*a*, 1973*b*, 1974*a*, 1974*b*, 1975). The methods he describes, often referred to as log-linear models, not only help one find bivariate relationships and higher-order interactions in complex tables—a useful enough function—but, he claims, they can be viewed also as analogous to the analysis of simul-

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taneous-equations models. At last, social scientists may feel, the exploration of polls and surveys has caught up with causal and path analysis.

The outpouring of papers, articles, and books attests to the growing interest in the study of cross-classifications. In addition to Goodman's, several related techniques have been advanced.[2] And there has certainly been no lack of an audience for any of them. Social scientists are rapidly turning to these procedures in an effort to resolve problems that previously went unanswered.[3] This acceptance, not surprising when given the ubiquity of categorical data, has been facilitated by the widespread availability of computer programs for analyzing complex contingency tables.

Hence researchers may feel that they now have the tools with which to interpret cross-classifications unambiguously, using the same concepts that they employ in studying interval-level data. Yet it may now be time to temper this enthusiasm with a note of caution.

Caution is required because the new techniques may actually solve many fewer problems than one might suppose. As will be seen, the difficulty lies not in the methods, which are perfectly well formulated and justified, but in the kinds of data to which they are applied. Contrary to the impression one gets from reading the articles describing these procedures, one cannot stop worrying about the level of measurement simply because one has a technique that appears to be developed exclusively for categorical data. To the extent that nominal and ordinal scales represent measurement error, the question naturally arises: What do and can these methods tell us about the underlying phenomena? Although they advance our understanding in some respects, they still leave many issues unresolved. In fact, some problems that have always plagued contingency table analysis persist. Consequently, we will find that unless these approaches are used carefully and selectively, they can be quite misleading.

My purpose here is to illustrate how the uncritical application of these techniques can lead to distorted, even erroneous, conclusions. In order to keep the presentation simple and brief, I concentrate on log-linear analysis,

[2] Among the better known approaches are those based on weighted least-squares—Grizzle, Starmer, and Koch (1969), Koch and Reinfurt (1971), Grizzle and Williams (1972), Koch, Imrey, and Reinfurt (1972), Lehnen and Koch (1947*b*), and Theil (1970); those based on information theory—Ku and Kullback (1968), Ku, Varner, and Kullback (1971), and Kullback (1973, 1974); those based on dummy variable analysis—Suits (1957), Andrews, Morgan, and Sonquist (1967), Andrews and Messenger (1973), and Miller and Erickson (1974); those based on graph theory—Davis (1975); and those based on Bayesian perspectives—Lindley (1964). There are in addition numerous miscellaneous approaches, among them those of Coleman (1964) and Lindsey (1973).

[3] Articles based on the applications of the new methods of categorical data analysis include Davis (1974*b*), Giles et al. (1976), Guest (1974), Johnson and Koch (1971), Knoke (1974), Kritzer (1975), Lehnen (1972), Lehnen and Koch (1974*a*), and Scheuren (1973). Also, innumerable papers have been presented at recent meetings of sociology and political science associations. Many social science conventions now have panels devoted to the analysis of contingency tables.

but my remarks pertain in a general fashion to the other methods as well. Although the ideas are not novel—Goodman himself has discussed some of them—they have not been adequately understood by the numerous investigators who have uncritically carried out his procedures. After making this assessment, we should have a clearer appreciation of the limits of their valid application.

MODELS FOR THE ANALYSIS OF CATEGORICAL DATA

Since Goodman gives a thorough description of log-linear models, I will only highlight the main points (also see Davis 1974*a;* Fienberg 1970).

Beginning with a multidimensional population cross-classification one builds a model for its expected frequencies—or some function of them such as the odds—by introducing so-called main and interaction effects. Consider for example a three-way $I \times J \times K$ table with expected frequencies F_{ijk}. A "saturated" model (that is, one containing a full set of terms) is

$$F_{ijk} = \eta \tau_i^A \tau_j^B \tau_k^C \tau_{ij}^{AB} \tau_{ik}^{AC} \tau_{jk}^{BC} \tau_{ijk}^{ABC} , \tag{1}$$

where the τ's measure main and interaction effects of the variables on the F_{ijk} and η is introduced to insure that the F_{ijk} sum to n, the table total (Goodman 1973*b*, p. 1142).

It is sometimes more convenient to work with the natural logarithm of the expected frequencies, G_{ijk}. Thus, corresponding to model (1) we have

$$G_{ijk} = \log (F_{ijk}) = \theta + \lambda_i^A + \lambda_j^B + \lambda_k^C + \lambda_{ij}^{AB} + \lambda_{ik}^{AC} + \lambda_{jk}^{BC} + \lambda_{ijk}^{ABC} , \tag{2}$$

where

$$\theta = \log (\eta),\ \lambda_i^A = \log (\tau_i^A), \text{ etc} . \tag{3}$$

"Unsaturated" models contain fewer parameters. The only models Goodman considers, "hierarchical models," have the property that if a model includes terms representing the interaction between, say, A and B (that is, λ_{ij}^{AB}), it must also include their lower-order relatives (that is, λ_i^A and λ_j^B).

Following Goodman and others, a model will be described not by the τ's or λ's, but by the set of "fitted marginals" tables used in estimating the expected frequencies under that model. Thus, the model

$$G_{ijk} = \theta + \lambda_i^A + \lambda_j^B + \lambda_k^C + \lambda_{ij}^{AB} + \lambda_{ik}^{AC} + \lambda_{jk}^{BC} \tag{4}$$

will be denoted simply $\{AB\}, \{AC\}, \{BC\}$ (see Goodman 1970 for details).

Some Remarks on These Models

Note first that the models pertain to population cross-classifications. In applying a model to a set of data one assumes that the variables are really dis-

crete, though presumably they can have any number of categories. As will be seen shortly, this seemingly innocuous assumption has extremely important implications.

The parameters in the equations are perhaps best interpreted as indicators of the strength and type of relationships existing among the variables. (Terms involving a single variable can be viewed as measures of skewness [Davis 1974*b*].) At the outset of a study, investigators will probably be most interested in the presence or absence of various terms rather than their specific numerical values, because the appearance of a λ parameter in a model is an assertion that the relevant variables are related in a particular way. Conversely, the absence of a term suggests (conditional) independence or the lack of a higher-order interaction. All of these statements have substantive significance, and one therefore wants to be sure that the inclusion of a term corresponds to the true state of affairs. After identifying the correct linkages, he can then estimate the numerical values of the coefficients.

But notice that the τ's or λ's are really families of parameters. Hence, it requires vectors and matrices to describe them. The required number can be reduced somewhat since, as in analysis of variance, the terms are defined so that, for example,

$$\sum_i \lambda_i^A = \sum_j \lambda_j^B = \sum_i \lambda_{ij}^{AB} = \sum_j \lambda_{ij}^{AB} = 0 . \qquad (5)$$

These constraints mean that the number of independent parameters for, say, λ_i^A is $(I - 1)$, not I. Nevertheless, only when the variables are dichotomous can the terms be presented as single numbers. Furthermore, if the variables have more than two classes, the interpretation of the terms becomes more awkward. One can, of course, substitute a given value into a model to see how it affects the expected frequency. For the most part, however, the parameters cannot be summarized by a single, easily understood index.[4]

Partly for these reasons, social scientists seem to prefer completely dichotomous data when using these techniques. The resulting models are much easier to calculate, interpret, and display graphically. But as will become apparent, the advantages do not outweigh the disadvantages.

Expected frequencies under a model can be estimated by using the maximum-likelihood methods described by Goodman or other estimation procedures (Grizzle et al. 1969; Theil 1970). The estimates, $\hat{F}_{ijk}$, can then be compared to the observed frequencies with either the familiar goodness-of-fit χ^2 or likelihood-ratio χ^2 statistics. As with statistical procedures, though, estimation involves some practical problems. On the one hand, use of a very large sample may lead to significant χ^2 statistics even when the magnitudes of the effects are trivial. On the other hand, the sampling theory rests on asymptotic results because making exact calculations is often very diffi-

[4] For further remarks on the interpretation of these coefficients, see Davis (1974*c*).

cult (Bishop et al. 1975). The question then arises: How large does the sample have to be for the results to be valid? In particular, several or even most of the estimated expected frequencies in a large cross-classification may be quite small. How do the small values affect the χ^2 approximations? These kinds of questions are usually answered by resorting to ad hoc rules of thumb, such as the sample size divided by the number of cells should be greater than five. For these reasons, it is tempting to collapse large tables in order to increase the observed frequency in each cell.

Similarly, there may be so many observed zeros in a table that one cannot estimate the parameters. For the parameters of an unsaturated model to be computed, the $\hat{F}_{ijk}$ must all be greater than zero. Normally, the estimation procedures yield positive estimates, but if zeros appear in marginal tables some estimates may be zero. Then one must either forgo the calculation of the parameters or turn to some procedure for smoothing the data. Perhaps the simplest method of eliminating zeros is to add a constant (e.g., .5 or $1/T$, where T is the total number of cells in the table) to each observed frequency. Goodman recommends adding .5 when computing saturated models. Making this adjustment for unsaturated models—a procedure he does not recommend—can produce large changes in the degrees of freedom and thus affect one's decision regarding the model's adequacy (see Bishop et al. 1975; Smith 1976). Once again, a common way of avoiding and minimizing the problem is to "combine" categories.

To one extent or another, these comments pertain to the consequences of reducing the size or dimensionality of a table. That "collapsing" variables, whether intentionally or unintentionally, can affect a model's parameters has been demonstrated by Bishop et al. (1975).[6]

THE EFFECTS OF COLLAPSING VARIABLES

A model always assumes a specific underlying population cross-classification. But we cannot take its appropriateness for granted simply because we happen to have a set of categorical data. Suppose we propose a model for what we believe is an $I \times J \times K$ table when in fact each variable has many more than I, J, or K categories. Then our substantive conclusions about the observed table may bear little or no relationship to the true state of affairs. Bishop, Fienberg, and Holland's theorem indicates why.

They define a variable as "collapsible" with respect to other variables if the λ terms pertaining to these variables are the same in the reduced table as in the original array. Consider, for example, an $I \times J \times K$ table for the variables A, B, and C. Variable C is collapsible with respect to A and B if λ_i^A, λ_j^B and λ_{ij}^{AB} are the same in the marginal $\{AB\}$ table as in the original one. In other words, if ignoring C by collapsing or combining its categories

[6] See also Bishop (1971) and, for some related comments, Duncan (1974, 1976).

to form a marginal table does not change the remaining parameters, then C is collapsible.

Collapsibility also arises when only some categories of a variable are combined. Instead of analyzing a $5 \times 5 \times 5$ table one might combine some of C's categories to create a $5 \times 5 \times 2$ table. As before, if the resulting λ's pertaining to A and B remain unaffected, variable C is considered collapsible.

How can one determine the collapsibility of a variable? Bishop, Fienberg, and Holland prove a theorem which is restated here as an algorithm: Divide the variables into three groups. The first contains the variables to be collapsed, the second contains variables that are independent of those in the first group, while the third consists of variables that are not independent of those in the first group. (Independence here means that the λ terms linking two variables are zero.) The rule for collapsibility is simply that the first group of variables is collapsible with respect to the λ terms of the second group but *not* with respect to the λ's involving *only* the variables in the third group.

To illustrate the algorithm, consider a simple three-variable model:

$$G_{ijk} = \theta + \lambda_i^A + \lambda_j^B + \lambda_k^C + \lambda_{ij}^{AB} + \lambda_{ik}^{AC} . \qquad (6)$$

Since $\lambda_{jk}^{BC} = \lambda_{ijk}^{ABC} = 0$, B and C are considered independent. Thus, we could collapse C without changing the λ_{ij}^{AB} terms. (The λ_i^A parameters in the marginal table would be different from their counterparts in the full table, however.) Suppose that instead of collapsing C we combined categories of A to produce a $\{BC\}$ marginal table. Because A is not independent of either B or C—that is, the second group is empty—all of the terms, λ_j^B, λ_k^C, and λ_{jk}^{BC}, would be affected.

All of these remarks pertain to tables of any dimensionality and to compound variables—that is, to situations in which we want to collapse the categories to two or more variables.

Strictly speaking, the theorem applies to changes in sets of effect parameters. Nevertheless, such changes can affect a model's overall significance or acceptability as measured by χ^2. In collapsing data, for example, one may effectively "create" an interaction which can no longer be ignored. That is, the interaction would have to be included in the model for the "residual" to be nonsignificant. But this interaction would then be purely an artifact of one's choice of cutting. Collapsing can therefore affect the significance level and hence the decision to accept or reject a model.

Collapsing variables, it is plain, can be justified only under certain circumstances. In a three-variable system, one cannot collapse a variable unless it is independent of at least one of the remaining variables. Models involving four or more variables follow a similar principle. As simple as these ideas seem, they nevertheless have extremely serious consequences. Let us consider these consequences from three perspectives.

Treating Continuous Data as If They Were Discrete

Bishop et al.'s theorem on collapsibility raises an interesting and important point. Although some phenomena may exist on discrete levels, many variables of concern to social scientists are doubtlessly continuous. Yet time after time they are treated as if they were discrete. Dichotomizing opinions on issues groups people into two categories while the underlying attitude may really consist of a large, perhaps infinite number of possible positions. A dichotomy in this situation represents an extreme form of collapsing, and its seriousness for one's substantive conclusions depends on the variable's relationships to other variables under consideration.

Simulated data provide a heuristic illustration of the problem.[6] A computer program generated several three-variable, linear models of spurious correlation in which A "caused" both B and C. (The N for each model was 900.) Thus, B and C were conditionally independent in the population. Since the data initially consisted of interval scales, each model was tested using the standard criterion that $r_{BC \cdot A}$ should equal zero except for sampling error. As table 1 indicates, the data satisfied the criterion in every instance.

The scores were then recoded or grouped into ordered categories, simulating ordinal scales one might encounter in a typical attitude survey. The number of categories varied from two to 10. The variables in the first set of data, for example, were all dichotomized to produce a $2 \times 2 \times 2$ table. After collapsing the data in this manner, an attempt was made to fit the model of conditional independence, namely,

$$G_{ijk} = \lambda + \lambda_i^A + \lambda_j^B + \lambda_k^C + \lambda_{ij}^{AB} + \lambda_{ik}^{AC} . \qquad (7)$$

The point of view adopted here is that the continuous data from which the tables have been composed represent the true state of affairs. Insofar as is possible, the results of any procedure, whether a log-linear or some other form of analysis, should reflect the true relationships. Categorized variables frequently stem from inability to measure the underlying phenomena more precisely. In other words, lack of knowledge or convenience all too often dictates the level of measurement. Most attitudes, for example, probably do not exist in a discrete pro or con state but rather in gradations from, say, strongly favorable through strongly unfavorable. In many instances, however, one's operational techniques cannot or do not capture the full richness of people's opinions. Hence, the investigator merely pigeonholes them into two or three groups.

If this is in fact the case, a price must be paid. An ideal statistical technique

[6] Using a standard random number generator for the Burroughs B6700 computer, data were generated according to the following equations: $A_i = e_{Ai}$, $B_i = bA_i + e_{Bi}$, and $C_i = cA_i + e_{Ci}$, where the error terms (e's) and the constants were selected to create predetermined levels of correlation between the variables. One advantage of simulated data is that the underlying structure among the variables is known.

TABLE 1

TESTING FOR CONDITIONAL INDEPENDENCE BETWEEN B AND C*
WHEN THE UNDERLYING MODEL IS A SPURIOUS CORRELATION†
FOR VARIOUS CATEGORIZATIONS OF A, B, AND C

A. EACH TEST BASED ON SEPARATE SETS OF DATA

Categories for C	B	A	$r_{BC \cdot A}$	LRX^2	df	Significance
2	2	2	.042	23.97	2	.001
2	2	3	−.003	11.56	3	.01
2	2	5	−.003	6.23	5	N.S.
2	2	10	−.056	7.37	10	N.S.
3	3	2	−.032	38.55	8	.001
3	3	3	.003	28.58	12	.01
3	3	5	.005	23.46	20	N.S.
3	3	10	.027	51.49	40	N.S.
5	5	2	.027	105.73	32	.001
5	5	3	−.024	88.14	48	.01
5	5	5	−.023	57.49	80	N.S.

B. EACH TEST BASED ON SAME SET OF DATA

Categories for C	B	A	LRX^2	df	Significance
2	2	2	10.56	2	.05
2	2	3	10.98	3	.05
2	2	5	10.56	5	N.S.
2	2	10	10.85	10	N.S.
3	3	2	46.60	8	.001
3	3	3	21.07	12	.05
3	3	5	13.45	20	N.S.
3	3	10	22.49	40	N.S.
5	5	2	95.70	32	.001
5	5	3	78.74	48	.01
5	5	5	77.45	80	N.S.

* Model: $\{AB\}\{AC\}$.
† $\rho_{AB} = \rho_{AC} = .75$, $\rho_{BC} = .56$.

would be impervious to measurement errors of this kind. But unfortunately neither log-linear analysis nor, probably, any other method can totally avoid them, and one may never be completely sure his results reflect the true state of affairs. The results in table 1 indicate the problem. (The analysis was carried out first on separate sets of data—table 1A—and then on the same set of data—table 1B. The latter findings show how the same set of data categorized in different ways can lead to different conclusions.)

When each variable has at least five categories, the conditional independence model holds, as we might expect given the underlying structure of the data. Yet when some of the variables, particularly A, have fewer classes the

model begins to break down. Without additional information we might infer a direct link between B and C, since apparently none of the two-factor interactions can be eliminated.

Table 1 suggests the conditions under which collapsing variables will lead to such errors. If A, in this case the control variable, has at least five categories the "correct" model, conditional independence, fits, even though the remaining variables have as few as two categories. Hawkes (1973) argues that control variables should not be grouped. Actually, a more general statement is needed. In the three-variable case, collapsing is not justified unless the variable to be collapsed is independent of at least one of the remaining variables. At any rate, it is somewhat heartening to note that if we retain five or more classes in the appropriate variables we may be able to avoid faulty inferences. This finding agrees with related research on ordinal partial correlation (Reynolds 1974).

The essential point, then, is that one should not take the level of measurement for granted. It is always necessary to consider possible distortions introduced by categorizing truly continuous variables and to attempt to maximize the precision of the operational indicators. This dictum applies especially to "exploratory" studies in which the researcher is not sure which variables are interrelated.

It is equally important to think about variables that might have been left out of the analysis. For these omitted variables are in a sense collapsed. In examining a three-dimensional cross-classification among A, B, and C, one is effectively dealing with a collapsed table, a table which possibly conceals the influence of additional variables. Of course, this point applies to all multivariate procedures, not just the analysis of categorical data.

Collapsing Variables for Convenience

Even if one contends that his nominal and ordinal scales really represent underlying discrete distributions, the effects of collapsing remain a problem when categories are manipulated for convenience. As mentioned earlier, dichotomizing variables greatly simplifies a log-linear analysis. For example, nearly every article Goodman has published on the subject involves only dichotomous variables. In some cases, he has collapsed variables that originally had more than two categories. His reasons are understandable, because dichotomizing data makes it easier to describe and present the results.

Yet the unwary reader may be left with the impression that the number of categories does not matter. He may feel that Goodman's or similar procedures permit him to manipulate data in any way he pleases without loss of information. Unfortunately, the number of categories will usually affect one's substantive conclusions, as can be seen in the following example.[7]

[7] For related remarks, see Duncan (1976).

In comparing log-linear models with dummy variable analysis, Knoke (1975) used data from the National Opinion Research Center's 1973 General Social Survey. His first set of models, the ones reanalyzed here, pertained to four variables: (1) the respondent's attitude toward legalizing abortion, (2) religion, (3) education, and (4) frequency of church attendance. Although Knoke dichotomized each variable, the last three contained more than two levels in the original survey. Specifically, after excluding missing data, religion has three classes, education four, and church attendance five. Thus, we can compare his results using the dichotomies with those obtained from the uncollapsed variables.

From his results, which appear in table 2, Knoke (1975, p. 425) concludes that ". . . models which include two-variable marginal relationships or only one three-way interaction do not provide satisfactory expected frequencies according to the likelihood ration [*sic*] χ^2 criterion. Model *b*, however, which includes all six two-variable combinations as well as two three-way tables involving the dependent variable [opinion on legalized abortion] has an extremely good fit to the data. . . ."

But note that when the variables have not been collapsed much simpler models become acceptable. A good fit is obtained from the model containing six two-factor interactions, and even from a model (0) containing only five such terms. Three-factor interactions are actually not needed to account for the data.

One can see how Bishop et al.'s theorem might account for these results. Suppose that model (0) was in fact the true model for the $2 \times 3 \times 4 \times 5$ population table. Then collapsing, say, variable 4 (church attendance), which is not independent of any of the other variables, would affect the remaining interactions. The true model is of course unknown, but the lesson

TABLE 2

RETEST OF KNOKE (1975) DATA

		Knoke's Results			Reanalysis		
Model	Fitted Marginals	*LRX*²	df	Significance	*LRX*²	df*	Significance
0.........	{12} {13} {14} {24} {34}	...	...	...	94.57	80	N.S.
1.........	{12} {13} {14} {23} {24} {34}	17.39	5	.005	64.00	65	N.S.
2.........	{123} {14} {24} {34}	7.84	4	.1	53.53	55	N.S.
3.........	{134} {12} {23} {24}	9.60	4	.05	55.44	53	N.S.
4.........	{124} {13} {23}	17.21	4	.005	96.34	69	.05
5.........	{123} {134}	8.40	4	.1	90.53	52	.01
6.........	{123} {134} {24}	1.19	3	N.S.	45.41	44	N.S.
7.........	{123} {134} {124}	1.09	2	N.S.	41.48	36	N.S.

NOTE.—1 = Legalization of abortion, 2 = religion, 3 = education, 4 = church attendance.

* Due to the presence of cells with zero estimates, the degrees of freedom for each model except the first have been adjusted according to the procedures outlined in Bishop, Fienberg, and Holland (1975).

is clear. By simply combining categories one may create artificial interactions which will give a misleading picture of the underlying relationships.

The point is not to fault Knoke who was, after all, not trying to develop a model for attitudes toward legalized abortions, but simply comparing two techniques.[8] These comments merely demonstrate that one's conclusions depend to a certain extent on how one categorizes the data and that no matter how sophisticated log-linear analysis seems it does not obviate the problem.

The Analysis of Recursive Systems

Goodman (1973*a*, 1973*b*) introduced a modification of log-linear analysis which, he argues, is analogous to the usual analysis of recursive models. This development is especially welcome to those investigators who wish to perform causal analyses on categorical data. As in the previous instances, though, the method has to be applied carefully since it, too, involves collapsing variables.

Consider, as an example, a four-way table consisting of variables A, B, C, and D. Suppose variables A and B are causally prior to C and all three are prior to D. Under this assumption, reciprocal causation and feedback loops have been ruled out.

According to Goodman's method one first develops a model, H_1, for the $\{AB\}$ marginal table. The adequacy of this model can be measured with the likelihood ratio χ^2 statistic.

Next consider a model, H_2, for the three-way table $\{ABC\}$ which includes $\{AB\}$ in the set of fitted marginals. Again, this model can be tested by an appropriate likelihood ratio χ^2. Finally, consider a model, H_3, for the four-way $I \times J \times K \times L$ table, $\{ABCD\}$,which contains $\{ABC\}$ in the set of fitted marginals.

Now consider the hypothesis that H_1, H_2, and H_3 are all true. Goodman shows that the test for this combined model, call it H^*, can be calculated in either of two ways. First, the expected frequencies for H^* can be found from

$$\hat{F}^*_{ijkl} = \hat{F}_{ij}\hat{F}_{ijk}\hat{F}_{ijkl}/f_{ij}f_{ijk}, \tag{8}$$

where $\hat{F}_{ij}$, $\hat{F}_{ijk}$, and $\hat{F}_{ijkl}$ are the estimated expected frequencies under H_1, H_2, and H_3, respectively, and f_{ij} and f_{ijk} are the observed frequencies in the

[8] Note, however, that the tendency to dichotomize data is widespread throughout the social sciences, even when more categories are available. In another article, for example, Knoke (1974) dichotomizes data drawn from the Survey Research Center (University of Michigan) presidential election surveys. But these variables have more than two categories in their original form. A reanalysis of his models (not presented here for the sake of brevity) shows that with the uncollapsed variables one generally gets a simpler picture of the data.

two-way and three-way tables. The $\hat{F}^*_{ijkl}$ can then be substituted into the formula for the likelihood ratio statistic.

Alternatively, the χ^2 test for H^* can be partitioned as

$$LRX^2(H^*) = LRX^2(H_1) + LRX^2(H_2) + LRX^2(H_3), \tag{9}$$

with the degrees of freedom for H^* equaling the sum of the degrees of freedom for the separate tests of H_1, H_2, and H_3. Goodman also suggests diagrams for H^* which are akin to usual path diagrams.

Although Goodman's approach seems sound enough, it does require one to collapse variables in order to form the appropriate marginal tables. His justification for doing so is that "[s]ince there are no arrows pointing to A or B from C or D . . . the appropriate estimate of β^{AB} was obtained by an analysis of the two-way marginal table $\{AB\}$ . . . , as would be the case in the usual analysis (for quantitative variables) . . ." (Goodman 1973*a*, p. 1153).

The analogy seems weak and misleading, however. Causal analysis often involves the estimation of total correlations, but these are not computed by collapsing the remaining variables. Instead, they are calculated from the full set of scores for each variable.

That collapsing data can lead to ambiguities is shown by another example. Data consisting of dichotomous variables were simulated to fit the four-variable causal model shown in figure 1 (see table 3).[9] The e's represent random disturbances and each relationship is linear. Note that only the BC arrow is missing, meaning B and C are conditionally independent given A.

A satisfactory model for these data is H: $\{AB\}\{AC\}\{AD\}\{BD\}\{CD\}$ for which $LRX^2 = 1.02$, with six degrees of freedom. In view of these considerations, the inclusion of terms corresponding to $\{BC\}$ seems unwarranted.

Yet efforts to test the model "recursively" according to Goodman's scheme lead to an additional term representing a direct link between B and C. Table 4 provides the test. Clearly H^*, the composite model, is significant. Upon partitioning H^* one immediately sees that the source of the trouble
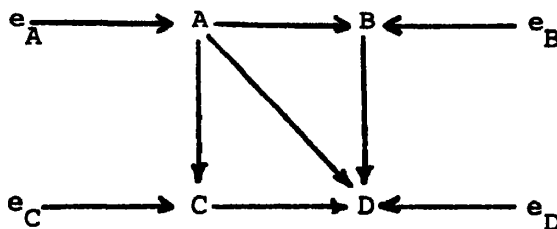
Fig. 1

[9] The simulation was carried out in the same general way as the one described in n. 6 above.

TABLE 3

DATA USED IN TESTING MODEL IN FIGURE 1

A	B	C	D +	D −
+	+	+	242	20
+	+	−	68	21
+	−	+	54	55
+	−	−	15	43
−	+	+	34	12
−	+	−	55	55
−	−	+	17	58
−	−	−	28	254

TABLE 4

TEST OF THE RECURSIVE MODEL IN FIGURE 1

Source	LRX^2	df
H_1 $\{ABC\}\{AD\}\{BD\}\{CD\}$	0.83	4
H_2 $\{AB\}\{AC\}$	19.22	2
H_3 $\{AB\}$	0.0	0
H^* composite model	20.05	6

NOTE.—H_1 pertains to the four-way table $\{ABCD\}$; H_2 to the three-way table $\{ABC\}$; H_3 to the two-way table $\{AB\}$.

lies in H_2, the model asserting conditional independence between B and C in the three-way table: $LRX^2 = 19.22$ with two degrees of freedom. To obtain a satisfactory fit for H^* one must modify H_2 to include a parameter linking B and C.

Certainly we know from Bishop et al.'s theorem that such a term might be expected to appear. Analysis of the $\{ABC\}$ marginal table—that is, collapsing D to compute $\hat{F}_{ijk}$—almost guarantees that we will find such a link. Obviously aware of the situation, Goodman nevertheless recommends this strategy for analyzing recursive systems.

Some of Goodman's own examples exhibit the same properties. For instance, Goodman (1973*a*) analyzes a four-way table dealing with the attitudes of 3,398 schoolboys. He presents numerous path diagrams based on the model H': $\{AB\}\{AC\}\{BC\}\{BD\}\{CD\}$ for which $LRX^2 = 4.06$, with six degrees of freedom. But another, simpler model—H'': $\{AC\}\{BC\}\{BD\}$ $\{CD\}$—also fits the observations: $LRX^2 = 8.78$, with seven degrees of freedom. The difference between the models is of course $\{AB\}$, which though statistically significant seems unnecessary for a full understanding of the data. Any attempt to eliminate it from a recursive model will fail, however, because the term always appears in the collapsed $\{AB\}$ table.

Thus, collapsing a variable may require one to fit parameters that would

otherwise be unnecessary. Since the true causal priorities among variables are unknown, the investigator has a problem: he finds linkages in the reduced table that are not present in the full array. An ideal technique would allow one to specify the causal assumptions and then test the model as given without also having to add parameters. For example, the usual causal analysis of the model in figure 1 (applied to quantitative data) would presumably allow one to detect the conditional independence of B and C.

SUMMARY

With this one exception, none of these remarks, it should be reemphasized, is intended to disparage the techniques Goodman describes. After all, those techniques were designed for discrete data and cannot be expected to work if the data are not discrete.

But therein lies the rub. Many if not most of the nominal and ordinal scales found in the social sciences probably only approximate what are really continuous variables or at least variables having many more than two or three classes. If this is the case, we may be asking too much. Even though the methods seem ideally suited for the analysis of cross-classifications, they may actually solve relatively few of our most nagging problems.

Incidentally, other forms of categorical data analysis are not much different in this regard. Grizzle et al. (1969) and Theil (1970), for example, use weight least squares to obtain estimates for the types of models Goodman considers. Yet since both approaches frequently lead to the same conclusions—and in many instances to the same numerical results—they share the same interpretative shortcomings. Likewise, efforts by Hawkes (1971) and Smith (1972) to develop and justify "product-moment" correlation and regression analogues for ordinal scales, even those involving numerous ties, have been criticized on precisely the same grounds: the methods, however well defined, cannot always detect the true underlying structure from which the observations are taken (see Kim 1975; Reynolds 1973, 1974). Nor does Quade's (1974) index of matched correlation, of which Davis's (1967) partial gamma is a special case, fare much better.

In conclusion, a researcher has to be pessimistic about using categorical data—variables that have in effect been collapsed—to detect causal structures. Furthermore, even with truly discrete data he should not let the sophistication of these methods beguile him into thinking that his measurement problems are over. Still, knowing the conditions and limits of their valid application, one can at least pursue future research with increased understanding.

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Testing for Interaction in Multiple Regression[1]

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Contrary to a recent claim, the inclusion of a product term in a multiple regression is a legitimate way to test for interaction. The unstandardized coefficient and the t-test for the product term are unaffected by the addition of arbitrary constants to the variables in the model. Certain other statistics are affected by this change, however, indicating that some hypotheses relating to interaction are not meaningfully testable unless variables are measured on ratio scales.

Sociological theories often imply that two or more variables interact in their effects upon some dependent variable (Blalock 1965). The variables x_1 and x_2 are said to interact in the determination of y if the effect of x_1 on y depends on the level of x_2 (which implies, symmetrically, that the effect of x_2 on y depends on the level of x_1). If all three variables are measured on numerical scales, it is common practice to test for the presence of interaction by including the product of x_1 and x_2 as an additional variable in a multiple regression. That is, one runs the model

$$\hat{y} = b_0 + b_1x_1 + b_2x_2 + b_3x_1x_2 \, . \tag{1}$$

If b_3 differs significantly from zero, the interaction is said to be significant.

Althauser (1971) has suggested that this method is invalid. He claims to show that the standardized coefficients corresponding to the b's in the above equation are strongly influenced by the correlation between either x_1 or x_2 and the product term x_1x_2. These correlations, in turn, depend on the sample means of x_1 and x_2. He concludes that the F-test for the coefficient b_3 is also affected by the sample means, a rather undesirable result since many sociological scales have arbitrary zero points and hence arbitrary means. This would seem to imply an arbitrary value for the test statistic as well.

I will argue here that, on the contrary, the inclusion of product terms in a multiple regression is a quite legitimate method for testing and estimating interaction effects.[2] While Althauser is correct in stating that the standard-

[1] After writing the initial draft of this paper, I learned that Arthur S. Goldberger had anticipated the central result in a personal communication with Robert Althauser. His formulation of the issue has helped to clarify my own. I am also grateful to George Bohrnstedt, Lowell Hargens, James House, and Scott Long for helpful suggestions.

[2] Because Althauser's approach is fundamentally different from the one taken here, I will not examine his arguments in detail. I only wish to note, first, that his statement about the F-ratio is not supported by a formal derivation. Second, his argument is marred by an error which occurs early in the paper and is carried through the remaining derivations.

ized coefficient for the product term is affected by changes in the means, the F- and t-ratios for the product term are not affected. Moreover, the unstandardized coefficient for the product term is also unaffected. Nevertheless, there are problems in the formulation and interpretation of tests for interaction when one or more of the variables are measured on interval scales. These difficulties will be explored in some detail.

CHANGING THE ZERO POINT OF AN INTERVAL SCALE

It is well known that the usual regression model assumes that the variables are measured at least by means of interval scales, a matter of some concern to those who have only ordinal data. But even those sociological variables which approximate an interval scale often have arbitrary zero points. Most attitude and prestige scales, for example, are of this type. Variables like income or population size which do have a theoretically fixed zero point are referred to as ratio scales (Hays 1963).

The distinction between interval and ratio scales is ordinarily of little importance in regression analysis. Most researchers are aware of the fact that adding a constant to a variable (which amounts to changing the zero point) changes the intercept in the equation but leaves the slope estimate unchanged. This creates little difficulty because the intercept is rarely an object for interpretation or testing.

Something very similar happens when a product term containing an interval variable is included in a regression equation. Consider equation (1), and suppose that x_1 is measured on an interval scale.[3] This means that the zero point of x_1 can be altered by adding or subtracting an arbitrary constant without any loss of information. Define $z_1 = x_1 + c$ where c is an arbitrary constant. Substituting into (1) yields

$$\hat{y} = b_0 + b_1(z_1 - c) + b_2x_2 + b_3(z_1 - c)x_2 . \tag{2}$$

Multiplying out and combining terms gives

$$\hat{y} = (b_0 - b_1c) + b_1z_1 + (b_2 - b_3c)x_2 + b_3z_1x_2 , \tag{3}$$

which can be rewritten as

$$\hat{y} = b_0{}^* + b_1z_1 + b_2{}^*x_2 + b_3z_1x_2 \tag{4}$$

where $b_0{}^* = b_0 - b_1c$ and $b_2{}^* = b_2 - b_3c$.

Equation (4) shows what would happen if y were regressed on z_1, x_2, and z_1x_2. In other words, it shows the consequences of arbitrarily changing the

Specifically, his equation (A4) purports to be a general expression for the variance of x_1x_2, but in fact it only holds when x_1 and x_2 are independent. Yet those same variables are assumed to be correlated in later sections of the paper.

[3] The results which follow are true for both the population and the sample. To simplify the discussion, they are formulated in terms of the sample statistics alone.

zero point of x_1, a permissible transformation if x_1 is measured at the interval level. We find that two coefficients are changed, the intercept and slope for x_2 alone. But the slopes for x_1 and the product term x_1x_2 are unaltered by this transformation. In a similar fashion, it is easy to show that if the regression is rerun with arbitrary constants added to both x_1 and x_2, all the coefficients *except* b_3 are changed.

This demonstrates that the coefficient for the product term is unaltered by changes in the means of the variables resulting from the addition of a constant to all scores. The invariance also extends to tests of hypotheses about b_3. For example, as Althauser (1971) points out, the hypothesis that $b_3 = 0$ in the population can be tested by forming the F-ratio

$$F = \frac{(R_B^2 - R_A^2)(N - 4)}{1 - R_B^2} \qquad (5)$$

where R_B^2 is the coefficient of determination for (1), R_A^2 is for a model that excludes the product term, and N is the number of observations. It is well known that R_A^2 is unaffected by linear transformations of the independent variables in the regression. It is easily seen that the same invariance must also hold for R_B^2. Since only the right side of (4) is altered, the predicted value of y remains the same. This implies that R_B^2, which equals

$$\sum_{i=1}^{N} (y_i - \hat{y}_i)^2 \Big/ \sum_{i=1}^{N} (y_i - \bar{y})^2 ,$$

is unaltered by the transformation, and thus (5) is unchanged as well. Note that since the t-ratio for the hypothesis that $b_3 = 0$ is just the square root of (5), it also is unchanged.

The F-ratio for the coefficient of x_2, however, is changed by the addition of a constant to x_1 since $b_2 = 0$ and $b_2^* = 0$ are quite different hypotheses. In particular, if $b_2 = 0$ then b_2^* cannot equal zero unless there is no interaction (i.e., $b_3 = 0$) or unless $c = 0$ (a trivial case). Note also that by picking an appropriate value for c, one can make b_2^* take on any desired value. For instance, if $c = b_2/b_3$ then $b_2^* = 0$.

STANDARDIZED COEFFICIENTS

The preceding results for the unstandardized coefficients do not apply to the standardized (path) coefficients, which show a different pattern of change and invariance. The standardized coefficients for the model in (1) are given by

$$\begin{aligned} p_{y1} &= b_1 s_{x_1}/s_y \\ p_{y2} &= b_2 s_{x_2}/s_y \\ p_{y3} &= b_3 s_{(x_1x_2)}/s_y \end{aligned} \qquad (6)$$

where the s's are standard deviations of the subscripted variables. In particular, $s_{(x_1x_2)}$ is the standard deviation of the product term. I have already shown that b_1 and b_3 are unchanged by the addition of an arbitrary constant to x_1, and obviously s_y, s_{x_1}, and s_{x_1} are also unchanged by that transformation. Therefore p_{y1} is not affected. The transformation does alter b_2, however, so p_{y2} changes with the addition of a constant to x_1. The coefficient p_{y3} depends on $s_{(x_1x_2)}$, which turns out to be affected by changes in the mean of x_1 or x_2. In the special case in which x_1 and x_2 are independent,

$$\sigma^2_{x_1x_2} = \mathrm{var}(x_1)\,\mathrm{var}(x_2) + E^2(x_1)\,\mathrm{var}(x_2) + E^2(x_2)\,\mathrm{var}(x_1)\,. \qquad (7)$$

This result (Goodman 1960) is for the population but it has an analogue for a sample. It shows that the variance of the product increases as the means of x_1 and x_2 depart from zero. In the general case in which x_1 and x_2 are not independent, the variance of the product is a somewhat more complicated function of the means (Bohrnstedt and Goldberger 1969). We find, therefore, that the only standardized coefficient not altered by a change in the zero point of one of the variables is the coefficient for that variable entered singly. And if the zero point changes for all the variables in the product term, then all the standardized coefficients will change.

AN EMPIRICAL EXAMPLE

The reader may easily demonstrate these results to himself by changing the coding in sample regressions with product terms. I present here a simple example using real data from a survey of parapsychologists.[4] For the 119 cases, there were measurements on three variables: x_1, x_2, and y. Panel A of table 1 shows results from an ordinary least-squares regression of y on x_1 and x_2. The two independent variables had positive effects on y that were both significant beyond the .01 level (one-tailed test). Panel B shows the regression of y on x_1 and x_2 with the addition of the product term x_1x_2. The coefficient for the product is significant at the .05 level (one-tailed test).

Panel C shows the results of a similar regression with x_1 recoded such that $x_1^* = x_1 + 400$. As predicted, the unstandardized coefficient for the product term and its associated t-ratio are unchanged. Also unchanged are the unstandardized coefficient, standardized coefficient, and t-ratio for x_1^*. But the intercept and the coefficient for x_2 both increase by two orders of magnitude. The t-ratio for x_2 doubles in magnitude, while the standardized coefficients for x_2 and $x_1^*x_2$ reach the enormous values of -34 and 34, re-

[4] The population consisted of 119 members of the Parapsychological Association. The three variables are x_1, a 12-point scale measuring interest in parapsychology; x_2, the number of professional associations in which they reported membership; y, the number of times they had experienced discrimination because of their interest in parapsychology. Strictly speaking, it is not permissible to add a constant to x_2 since it is measured as a ratio scale. This was done in panel D of table 1 for the sake of example.

TABLE 1

REGRESSIONS OF y ON x_1, x_2, AND x_1x_2, SHOWING EFFECTS OF ADDING CONSTANTS TO x_1 AND x_2

	b	Standardized Coefficient	t	R^2
		A		
Intercept	− 2.19	...	−2.34	.173
x_1	0.395	0.27	3.18	...
x_2	0.314	0.29	2.48	...
		B		
Intercept	− 0.456	...	−0.35	.199
x_1	0.152	0.10	0.87	...
x_2	− 0.336	− 0.31	−0.96	...
x_1x_2	0.0887	0.66	1.91	...
		C		
Intercept	−61.4	...	−0.86	.199
x_1^*	0.152	0.10	0.87	...
x_2	−35.8	−33.56	−1.90	...
$x_1^*x_2$	0.0887	33.87	1.91	...
		D		
Intercept	0.121	...	0.13	.199
x_1^{**}	0.000	0.00	0.00	...
x_2	0.000	0.00	0.00	...
$x_1^{**}x_2^*$	0.0887	0.45	1.91	...

NOTE.—$x_1^* = x_1 + 400$, $x_1^{**} = x_1 - 3.788$, $x_2^* = x_2 + 1.714$.

spectively. Panel D shows results with both independent variables recoded as follows: $x_1^{**} = x_1 - 3.788$ and $x_2^* = x_2 + 1.714$. These values were derived from the results in panel B in order to produce the results shown here: unstandardized coefficients, standardized coefficients, and t-ratios for x_1^{**} and x_2^* all go to zero. But the unstandardized coefficient and t-ratio for the product term remain unchanged. Note also that R^2 is the same for all three models with product terms.

IMPLICATIONS

What do these results mean for the practicing researcher? First, although there may be better methods in some cases, the use of product terms to test for interaction is a legitimate method. The unstandardized coefficient and the t-test are not affected by changes in the means or zero points of the variables. Second, when one or more of the variables in the product term are measured on interval scales, it is useless to attempt to substantively interpret or test hypotheses about the coefficients for the other variables entered singly. If one of the variables has an arbitrary zero point, then those

coefficients are also arbitrary. From a purely statistical point of view, one can validly test whether any of the coefficients differ from zero. The point is that since the magnitude of the coefficients depends on an arbitrary constant, there can be no theoretical basis for hypothesizing that the coefficients are zero. A hypothesis about the values of those coefficients is equivalent to a hypothesis fixing the zero point of the interval scale. In Fararo's (1973) terminology, such hypotheses are not "empirically meaningful statements." Third, for similar reasons it is an exercise in futility to attempt to determine the relative importance of main effects and interaction by examining the standardized coefficients. Even when variables are measured on ratio scales, this will probably not be an informative comparison. Perhaps the best measure of the importance of the interaction is simply the increment to R^2 with the inclusion of the product term. Finally, it must be emphasized that the transformations that have been considered do not alter the models in any fundamental respect—they merely rearrange the information that they contain. Nothing that is theoretically meaningful is lost by this transformation, but it is important to keep in mind which statistics convey theoretically useful information and which do not.

HIGHER-ORDER INTERACTIONS

All these results can be generalized in a straightforward manner to regression equations containing product terms with three or more factors. Consider the model

$$\hat{y} = b_0 + b_1x_1 + b_2x_2 + b_3x_3 + b_4x_1x_2 + b_5x_1x_3 + b_6x_2x_3 + b_7x_1x_2x_3 \,. \tag{8}$$

Transforming this equation to one in $z_1 = x_1 + c$ gives

$$\hat{y} = b_0{}^* + b_1z_1 + b_2{}^*x_2 + b_3{}^*x_3 + b_4z_1x_2 + b_5z_1x_3 + b_6{}^*x_2x_3 + b_7z_1x_2x_3 \tag{9}$$

where an asterisk indicates that the coefficient has changed with the transformation. For equations with all possible interactions and main effects, the general rule is this: Coefficients for all terms involving the transformed (interval) variable are unaltered; coefficients for all other terms are changed (arbitrary). More complicated rules are needed when some of the possible terms are excluded. For example, if b_7 in (8) is set equal to zero, the transformation of x_1 will affect only b_0, b_2, and b_3, leaving b_1, b_4, b_5, and b_6 unchanged.

HIERARCHICAL TESTING

It is a common rule of thumb that testing for interaction in multiple regression should only be done hierarchically. That is, one should test for higher-

order interactions only when all lower-order interactions and main effects are included in the equation. If a rationale for this rule is given at all, it is usually that additive relationships somehow have priority over multiplicative relationships. When all variables are measured at the ratio level and there are strong theoretical reasons for excluding lower-order terms, this rule seems overly stringent. Indeed, the exclusion of lower-order terms in such cases can increase precision of estimation and the power of hypothesis tests (Wonnacott and Wonnacott 1970, p. 312). But when one or more variables in a product term are measured at the interval level, the hierarchical principle becomes essential.

Suppose, for example, that x_1, x_2, and y are measured at the ratio level and there exists a theory which states that the appropriate model is

$$\hat{y} = bx_1x_2 . \quad (10)$$

In this case it would be quite reasonable to run the regression corresponding to (10) in order to estimate b. But suppose that x_1 and x_2 cannot be measured directly and one has to settle for the interval measures $z_1 = x_1 + c$ and $z_2 = x_2 + d$ where c and d are unknown constants. Substituting these measures into (10) yields

$$\begin{aligned} \hat{y} &= b(z_1 - c)(z_2 - d) \\ &= bcd - bdz_1 - bcz_2 + bz_1z_2 \\ &= a_0 + a_1z_1 + a_2z_2 + a_3z_1z_2 \end{aligned} \quad (11)$$

where the a_i coefficients are defined by the line preceding them. This result indicates that, although the original hypothesis in (10) sets the lower-order terms equal to zero, that fact does not imply that the lower-order terms can be excluded when running the regression on interval-level variables. It should be clear that a_0, a_1, and a_2 can be zero or nonzero depending on the unknown values of c and d. Hence, there can be no theoretical justification for setting these coefficients equal to zero (i.e., excluding the terms from the equation). Moreover, if the lower-order terms are excluded, the coefficient for the product term and the R^2 for the model will vary with the unknown values of c and d. Thus, the earlier conclusions about invariance are true only when all lower-order terms are included.[5]

OTHER MODELS FOR INTERACTION

This line of reasoning leads to the conclusion that many potential models for interaction cannot be estimated when some of the variables are measured

[5] The model in (10) does impose a constraint on the a_i coefficients in the last line of equation (11). Specifically, it requires that $a_0a_3 = a_1a_2$. Testing and estimating the model under this constraint requires nonlinear regression techniques of the sort discussed by Draper and Smith (1966).

on interval scales. For example, consider

$$\hat{y} = b_0 x_1^{b_1} x_2^{b_2}, \tag{12}$$

of which (10) is a special case.[6] A frequently suggested approach to estimating this model (Wonnacott and Wonnacott 1970) is to take the logarithm of both sides to produce

$$\log \hat{y} = \log b_0 + b_1 \log x_1 + b_2 \log x_2 \tag{13}$$

and simply regress $\log y$ on $\log x_1$ and $\log x_2$. But notice that if $z_1 = x_1 + c$ is substituted into (13), there is no way to simplify so that c is absorbed into one of the other parameters. Since c becomes an additional unknown parameter, the model is underidentified and cannot be estimated. Again, if one goes ahead and regresses $\log y$ on $\log z_1$ and $\log x_2$, the coefficients and the R^2 will vary with c.

The same argument applies to ratios of scores when the denominator can only be measured at the interval level. Suppose the theoretical model is

$$\hat{y} = b_0 + b_1 x_1 + b_2 x_2 + b_3(x_2/x_1). \tag{14}$$

If one has only z_1 instead of x_1, the model cannot be validly estimated; the results will differ for every value of c. If, on the other hand, one has a ratio-level measure of x_1 but only an interval measure of x_2, the model can be transformed so that it is not affected by the addition of a constant to x_2:

$$\hat{y} = b_0^* + b_1 x_1 + b_2 z_2 + b_3(z_2/x_1) + b_4/x_1 \tag{15}$$

where $z_2 = x_2 + d$. In this case, d is absorbed into the parameters b_0^* and b_4 and will not affect estimates of the other coefficients.

In general, it appears that when all independent variables are measured at the interval level the only valid way to test for interaction in the framework of least-squares regression is to include product terms and all lower-order terms in the equation. One can check whether any particular model is invariant to scale transformations of the variables by (*a*) writing the model as though all the variables were measured at the ratio level and (*b*) substituting variables which add arbitrary constants to the ratio variables. If the constants can be absorbed into one or more parameters, the model is invariant in the sense of generating the same predicted values for y and, hence, the same R^2.

A quite different approach is to reduce the interval-level variables to sets of categories and test for interaction by analysis of variance, analysis of covariance, or the equivalent multiple regression using dummy variables. Consider, first, the case of one dichotomous variable and one continuous

[6] This model is well known to economists as a Cobb-Douglas production function which relates output y to labor x_1 and capital x_2 under the constraint that $b_2 = 1 - b_1$ (Wonnacott and Wonnacott 1970).

variable. Suppose that y, x_1, and x_2 are all measured on interval scales an
the model is

$$\hat{y} = b_0 + b_1x_1 + b_2w + b_3x_1w \quad (16$$

where $w = s$ if $x_2 > m$ and $w = t$ if $x_2 \leq m$ and $s \neq t$. Since w is a nomin variable, s and t could have any values so long as $s \neq t$. It can be show that recoding w to change the values of s and t could change any or all c the coefficients in the model. Yet, there is a sense in which the model remain unaffected by such changes. First, the R^2 will not change; second, the t-rati for the hypothesis that $b_3 = 0$ will be unaltered. Thus, the test for the pre ence of interaction does not depend on recoding the variables in the produc term.

By appropriate choice of s and t, moreover, one can get useful informatio from the coefficient estimates. The most common coding is to let $s = 1$ an $t = 0$ (or vice versa) which produces the following interpretation:

$$\hat{y} = b_0 + b_1x_1 \quad (17$$

is the regression of y on x_1 for only those cases where $x_2 \leq m$;

$$\hat{y} = (b_0 + b_2) + (b_1 + b_3)x_1 \quad (18$$

is the result that would be obtained if y were regressed on x_1 only for thos cases where $x_2 > m$. Thus, b_3 is the difference between the slopes for th two groups, and b_2 is the difference in the intercepts.

This well-known result is discussed at length by Gujarati (1970*a*, 1970*b*) who also considers the case in which x_2 is divided into more than two cate gories. The important points in this context are that (*a*) *any* coding of 1 that maintains $s \neq t$ will give an invariant test for the presence of interac tion; (*b*) if x_1 is measured on an interval scale, b_2 can be made equal to zer by adding an appropriate constant to x_1; (*c*) the standardized coefficient are essentially arbitrary; even with the one-zero coding they have no obviou interpretation.

The case in which both independent variables are reduced to sets of tw or more categories is equivalent to two-way analysis of variance with dis proportionate cell frequencies. The literature on this topic is too enormou to summarize here. For an introduction see Burke and Schuessler (1974) who point out the nonadditivity of sums of squares and the sensitivity o main effects to the arbitrary constraints imposed on the interaction effects

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The Importance of Being Beautiful: A Reexamination and Racial Comparison[1]

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This research note presents data from new samples of black and white women to compare with the findings presented by Elder and by Taylor and Glenn on the importance of feminine attractiveness versus education as a vehicle for upward mobility through marriage.

In the heterosexual marketplace, men and women bring their valued attributes to exchange for equally valued attributes of the opposite sex. So say both traditional wisdom and contemporary social exchange theory. Certain attributes are valued about equally by all in the market, while others are valued idiosyncratically by different individuals. Those valued by everyone (e.g., the economic potential of males) confer status, while those having idiosyncratic value for particular individuals (e.g., particular psychological traits) provide psychic satisfaction unrelated to status. This research note focuses on attributes known to have universal exchange value. It examines the exchange of physical attractiveness of women for economic status of men. More specifically, it examines the relationship between women's education and their attractiveness as vehicles for achieving status mobility by marriage, a relationship previously reported in papers by Elder (1969) and by Taylor and Glenn (1976). Both papers have shown that attractiveness is related to the occupational status of the husband. Both concluded that attractiveness was more valuable for the woman of low-status origin than for the woman of high-status origin, whereas education was more important than attractiveness for those of high-status origin. Taylor and Glenn sought to identify an interaction between education and attractiveness which suggests that some women use an "education strategy" and some rely on attractiveness rather than education in obtaining a mate of high status.

The present study examines data from samples of black and white married women which enable us to address the main points made in the previous two papers. Because of the similarity in available data, tables are

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·ganized according to the layout used by Taylor and Glenn, allowing :curate comparison of their findings with those of the present study.

The data for this study derive from area sample surveys in 16 U.S. cities, ›nducted in 1969–70 and in 1973–74. In 1969–70, area samples of 100 hite and 100 black ever-married women aged 15–44 were obtained from ıe low-income census tracts in each of the 16 cities, except in two where lacks were omitted because of the small black population. Census tracts ith race-specific median family incomes of under $6,000 according to the ›60 census were sampled. The same areas were resurveyed in 1973–74, ›ing the same sample size and sample design. In the analyses reported ı this paper, the two time samples were pooled. Blacks and whites were 'eated as two separate populations for sampling purposes. Therefore, all uestions addressed by this research can be answered for blacks and whites ›parately, and racial comparisons can be made.

A total of 3,309 whites and 2,910 blacks was interviewed. In order › better compare this analysis with that of Taylor and Glenn, data are 'splayed only for the 1,243 white and 733 black women 25–40 years of age .he age span in their sample) who were currently married and living with ıeir husbands and for whom a status level could be assigned for a male ›ad of household when the respondent was 10–14 years old. However, ıta from the entire age span lead to the same conclusions as the data ›esented.

Attractiveness was rated by interviewers in five categories from very un- .tractive to very attractive. Husband's occupational status is the score ' the husband's present occupation on the Nam-Powers scale, which is ısed on the education and income of particular occupations in the 1960 .S. census (Nam and Powers 1968). Father's occupational status (or atus of origin) is the score of the Nam-Powers scale of the male head ' household when the woman was 10–14 years of age. Mobility is hus- ınd's occupational status minus father's occupational status. Mean mo- ·lity scores are highest in a positive direction for those of lowest origin and ›gative for those of highest origin.

ESULTS

he hypothesis that attractiveness is predictive of upward mobility is 'st tested by the contingency analysis shown in table 1. Within origin ıtegories, upwardly mobile women *are* more attractive than those who are ›t upwardly mobile, but differences reach statistical significance for only ›me origin categories.

Both Elder and Taylor and Glenn found a woman's education to be better predictor of husband's status overall than her attractiveness. A re- 'ession analysis of the effects of a woman's father's status, her education,

TABLE 1

MEAN ATTRACTIVENESS SCORES BY STATUS OF ORIGIN AND MOBILITY
(Low Scores Indicate Greater Attractiveness)

Status of Origin	Black: Not Upwardly Mobile	Black: Upwardly Mobile	White: Not Upwardly Mobile	White: Upwardly Mobile
(Low) 1	2.92	2.79	a	2.91
2	2.79	2.55*	3.00	2.90
3	2.82	2.46**	2.93	2.81
(High) 4	2.52	a	2.75	2.50**

a Fewer than 15 cases.
* Difference by mobility significant at .05 level (one-tailed t-test).
** Difference by mobility significant at .01 level (one-tailed t-test).

and her attractiveness upon her husband's status was performed and is displayed in table 2. This table is organized to compare with Taylor and Glenn's table 1.

For whites overall, education is more related to husband's status than is attractiveness, with attractiveness having statistically significant but generally trivial relationships to husband's status. This is generally consistent with the findings of Taylor and Glenn. For blacks, education and attractiveness are about equally good predictors of husband's status, with education more important than attractiveness only for the oldest part of the sample.

Both previous studies report that for the attainment of a husband of high status attractiveness is more important for women of low-status origin, whereas education is more important for those of high-status origin. The same regression analysis as in table 2 was performed for each status-of-origin category. The results are displayed in table 3, which corresponds to Taylor and Glenn's table 2. For the data concerning white women, the superiority of education over attractiveness holds about equally for all origin categories. The betas for education are around .20 for each origin and all statistically significant. The betas for attractiveness are all under .10, and all fail to reach statistical significance. At first this statement appears to contradict the findings of Elder and of Taylor and Glenn. However, neither of these previous studies reported a *statistically significant* difference between origin categories in predictive power for either education or attractiveness. Therefore there is no contradiction in the *data,* but simply in the *interpretation* of nonsignificant differences.

Both previous studies eventually arrived at an argument that attractiveness and education could be seen as alternate paths to mobility through marriage. Taylor and Glenn offer an analysis of the relationship between attractiveness and husband's status for high- and low-education women

TABLE 2

REGRESSION OF HUSBAND'S OCCUPATIONAL STATUS ON WOMEN'S STATUS OF ORIGIN, EDUCATION, AND ATTRACTIVENESS

	BLACK			WHITE		
	r	beta	b	r	beta	b
Ages 25–40 (Black: $r^2 = .0865$,* $N = 717$; White: $r^2 = .10143$,* $N = 1,222$):						
Status of origin.	.14426	.08759	0.09577*	.22113	.13564	0.13185*
	(.0208)			(.0489)		
Education.....	.23393	.18177	1.69538*	.28371	.21561	2.21829*
	(.0547)			(.0805)		
Attractiveness..	−.20202	−.15516	−4.03869*	−.14573	−.07044	−1.82457*
	(.0408)			(.0212)		
Ages 25–29 (Black: $r^2 = .12593$,* $N = 254$; White: $r^2 = .06953$,* $N = 525$):						
Status of origin.	.07799	.02739	0.03031	.18307	.11792	0.1245*
	(.00608)			(.0335)		
Education.....	.27062	.22544	2.50418*	.23821	.19328	2.12291*
	(.0732)			(.0567)		
Attractiveness..	−.27140	−.23133	−6.2245*	−.08208	−.02315	−0.63315
	(.0731)			(.0067)		
Ages 30–34 (Black: $r^2 = .09212$,* $N = 215$; White: $r^2 = .15235$,* $N = 348$):						
Status of origin.	.26968	.24666	0.26192*	.26469	.14987	0.14713*
	(.0727)			(.0701)		
Education.....	.13434	.08620	0.82135	.33697	.23785	2.6186*
	(.018)			(.114)		
Attractiveness..	−.14220	−.09861	−2.33351	−.23858	−.13637	−3.61364*
	(.0202)			(.0569)		
Ages 35–40 (Black: $r^2 = .08551$, $N = 247$; White: $r^2 = .13725$, $N = 349$):						
Status of origin.	.08402	.00161	0.00182	.24871	.14731	0.12485*
	(.00706)			(.0619)		
Education.....	.26838	.23809	1.9459*	.32858	.25487	2.3375*
	(.072)			(.1078)		
Attractiveness..	−.17929	−.11979	−3.31207	−.16634	−.10171	−2.40973*
	(.0321)			(.0277)		

NOTE.—r^2 in parentheses.
* Statistically significant at .05 level.

separately and for each status of origin. They are unable to confirm a difference in the relationship by educational level for any status of origin. Data from the present study examining the same relationship are presented in table 4, which corresponds to Taylor and Glenn's table 3. In table 4, "high education" means "some college." Even though it created grossly unequal N's, this cutting point was used because both previous studies have emphasized the importance for women of going to college as an avenue of access to high-status men.

For white, low-education women of each category of origin, the b's are negative, indicating that the more attractive get the high-status husbands. For the high-education women, the slopes are positive, but not significant. The difference between the b's for high and low education women is significant in each status of origin and for the total white sample. We conclude

TABLE 3

A COMPARISON OF THE REGRESSIONS OF HUSBAND'S OCCUPATIONAL STATUS ON WOMEN'S EDUCATION AND ATTRACTIVENESS BY STATUS OF ORIGIN

STATUS OF ORIGIN	r		beta		b		R^2	N
	Education	Attractiveness	Education	Attractiveness	Education	Attractiveness		
				Black				
(Low) 1.....	.16313 (.0266)	−.23903 (.0571)	.117	−.214	0.958*	−5.308*	.07011*	342
2. ...	.31971 (.1022)	−.14326 (.0205)	.304	−.090	3.630*	−2.374	.11003*	209
3.....	.15731 (.0247)	−.21277 (.0453)	.124	−.191	1.242	−5.121*	.06005*	112
(High) 4.....	.29283 (.0857)	−.09080 (.00824)	.289	−.078	2.601*	−2.091	.09177	51
				White				
(Low) 1.....	.20861 (.0435)	−.08685 (.00754)	.200	−.030	1.734*	−0.762	.04436*	179
2.....	.26344 (.0694)	−.12175 (.0148)	.249	−.070	2.810*	−1.793	.07402	302
3.....	.20362 (.0415)	−.12269 (.0151)	.187	−.087	1.947*	−2.082	.04868*	339
(High) 4.....	.27152 (.0737)	−.16882 (.0285)	.242	−.090	2.836*	−2.438	.08104	387

NOTE.—r^2 in parentheses.
* Statistically significant at .05 level.

TABLE 4

REGRESSION OF HUSBAND'S OCCUPATIONAL STATUS ON WOMEN'S ATTRACTIVENESS BY STATUS OF ORIGIN AND EDUCATION
(Unstandardized Coefficients)

	ATTRACTIVENESS					
	BLACK			WHITE		
STATUS OF ORIGIN	Low Educ.	High Educ.	Diff.	Low Educ.	High Educ.	Diff.
(Low)...	−4.2927* (449)	−10.6254* (56)	6.3327*	−2.9557* (447)	4.0407 (37)	−6.9964*
(High)...	−4.8643* (130)	− 3.0342 (34)	−1.8301	−4.5135* (590)	3.1804 (148)	−7.6939*

NOTE.—N's in parentheses.
* Statistically significant at .05 level.

that for whites, if a woman does not go to college, her attractiveness is an important determinant of whether or not she gets a high-status husband. If a woman does go to college, her attractiveness is unimportant in determining whether or not she gets a high-status husband.

For blacks the picture is different. For low-education blacks, the relationship between attractiveness and husband's status has a negative and significant slope. For high-education blacks, the same relationship is significantly stronger in the same direction. That is to say, for high-education blacks, the predictive power of attractiveness for marrying a high-status husband is *greater* than it is for low-education blacks, or just the opposite of the findings for whites. When the relationship for blacks is examined within status origin categories, it becomes even more complicated. For low-origin blacks, the b is significantly more negative for high- than for low-education women, while for blacks of high-status origin, educational attainment does not discriminate different slopes. Put another way, if a black woman does not go to college, attractiveness is an important determinant of whether or not she obtains a high-status husband. But if she goes to college, attractiveness is an even more significant determinant of whether or not she gets a high-status husband, especially if she is of low-status origin.

CONCLUSIONS

The findings indicate three general conclusions.

1. Consistent with the findings of both previous studies, attractiveness is significantly related to status attainment through marriage. However, it does not explain much of the variance in mobility or in husband's status. For whites, education appears to be the stronger predictor overall, as the previous studies have found. For blacks, on the other hand, education and attractiveness are about equally good predictors.

2. For neither blacks nor whites does the relative strength of education and attractiveness for predicting status mobility through marriage vary systematically by status of origin. Both previous studies report that attractiveness is more important for low-origin than for high-origin women. But in fact neither previous study shows statistically significant differences in effects by status origin.

3. The attractiveness-mobility relationship is stronger for low-education than for high-education whites of whatever status origin. But it is stronger for high-education than for low-education blacks. For the small number of high-education blacks from low-status origins, the positive relationship between attractiveness and husband's occupation is particularly strong. Therefore the interaction of education and attractiveness suspected but not demonstrated by the earlier researchers is confirmed.

This we interpret as follows: If a white woman goes to college, her attractiveness has no bearing on the status level of her husband's occupation. If she does not, then her attractiveness has a strong bearing on whether or not she gets a high-status husband. This relationship holds whether she comes from a low-status or a high-status origin. For the black woman, attractiveness plays an important role in getting a high-status husband, whether or not she goes to college. But it plays a stronger role if she goes to college than if she does not. It is especially important for those of low-status origin who go to college.

When attention is focused on the substantive importance rather than the statistical significance of the findings, the nonimportance of the role of attractiveness is striking. In no cell in any table does attractiveness explain as much as 10% of the variance in mobility status. In most cells, it does not explain 5%. No one has ever doubted it played some role in heterosexual relationships. But its overpowering impact in first encounters stands in sharp contrast to this mere trace of influence on mobility. The original exchange theory from which we began has led us to shed only a feeble light on the achievement of status mobility through marriage. Perhaps feminine beauty, like a beautiful day or a beautiful flower, is a lifter of the spirit and a quickener of the blood, but only a minor disturbance in the serious business of allocating the other scarce resources of the world.

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New Policy on Commentary and Debate

Comments submitted for the section of Commentary and Debate should be limited to brief critiques, addressing specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* will not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted for consideration as articles.

Commentary and Debate

MAYHEW AND LEVINGER'S USE OF RANDOM MODELS

The use of random models of interaction, as exemplified by Mayhew and Levinger (*AJS* 81 [March 1976]: 1017–49), in the analysis of organizational and political processes raises serious questions. Elsewhere (Mayhew 1973) as well as here, the claim is made that there are at least two alternative interpretations for a set of data which conform to a random model: either the phenomena are indeed a random process, or else there are social forces producing the equivalent of random expectations. Mayhew and Levinger (p. 1033) suggest that: "Only when this second possibility has been ruled out can the first be given realistic consideration. That is, only after we have exhausted all other other contingencies would we conclude that inequality in power structure, like molecular motion, is best described by a purely random process." This is a puzzling claim, rather contrary to the usual practice in statistical analysis. Usually, we try to rule out chance before we feel we can turn to other explanations. But the argument in any case is not central to their concerns. More important, they think that the random model provides a sort of "baseline." There are some objections to their claims.

1. Their statement of the Michels-Mosca arguments requires reexamination. Although my remarks apply to the material on both Mosca and Michels, I shall confine myself here to the Michels argument. In order to use a mathematical model, they conceptualize an elite as simply a minority

(any number less than half the size of any group). But such a definition of elite is surely insufficient. Elites are indeed small in numbers, but that is of little interest in itself. An elite, if the concept is to have any distinctive sociological value, has two major features:

a) Others must look up to the group (the elite), wish to emulate it or join it, and the elite must themselves have some sense of group pride, a sense of being elects who deserve their role and take it seriously (Nadel 1956). This hardly follows from the mere fact of being a minority.

b) The sheer fact that a small group rules is of little significance, for that is almost always the case. Except in the simplest kind of situations (small groups for minor matters requiring little expertise), there must always be a minority since only a minority can act in some reasonable time to accomplish an end for the group. The town-hall type of democracy is highly limited, requiring all sorts of conditions for successful operation. So students of democracy from de Tocqueville to Schumpeter and on to Lipset and others freely concede that participatory democracy will not work for large numbers. But if that were all there was to the elite argument, democracy would be impossible and we would have to adopt the position that Mayhew and Levinger indeed come to in the Appendix to their article, where democracy is dismissed as a "mirage" or a "form of propaganda." But they leave out the central point. The question is not whether a minority rules: it usually does. The question is, Is it possible for the majority to *reject* the minority and replace it with another one (Parry 1969, p. 31)?

This second feature of elites is clearly the major structural one (the first being social psychological), and the authors hardly touch it. They seem to show awareness of it at one point where they concede that their model does not explain the "stabilization of social structure over time," except for the possible operation of a "learning paradigm"—a highly inefficient process, even if it occasionally exists. It is precisely in "stabilization" that the heart of the matter lies. The point is not that a mere minority rules but that they will not let anybody else rule. The random model requires only that there be a minority ruling. That could easily be attained by a perfectly democratic model in which *everyone* takes his turn at becoming part of the ruling minority. At one time, the Israeli kibbutzim tried something of the sort, and there are occasional examples of choice of leaders by lot (the newly formed Society for the Study of Symbolic Interaction recently ran its election of officers in that way). So it is possible, but it would hardly be an example of what Michels (or Mosca) was talking about—just the opposite.

2. To show that any given result *could* occur by chance is really not persuasive. A golfer can hit a hole in one by chance; the Kon-Tiki expedition shows that it is possible to make it from South America to the South Seas in a raft. Proof requires something a lot stronger. Mayhew and Levinger do claim they have something stronger, namely, that the probability

is high that chance alone will generate various social results. If so, it seems peculiar that oligarchies spend so much time defending themselves. One is driven to a paradox first suggested by Gouldner (1955, p. 506) when he offered an "iron law of democracy." Gouldner drew this conclusion from the claim of Michels that there is a *tendency* toward oligarchy. If there is such a tendency, clearly conditions a good deal of the time must be the opposite—that is, democratic. Apparently, democracy is the "usual" condition; otherwise the "tendency" would have long since worked itself out and there would be no evidence of the law of oligarchy operating.

The random model offered by Mayhew and Levinger suggests the same conclusion from a different argument. If chance alone will result in the emergence of an elite, then there must be something operating to upset it all the time, or else elites would not have to spend all their time defending their positions. Elites might be highly amused to learn that their position might be due to random processes.

Having made these critical remarks, let me hasten to say that I am not condemning mathematical approaches as such. Mathematical models do offer useful baselines. Mathematical conditions may suggest constraints, for example. Thus Kephart's (1950) classic work on family size is an example of "what is possible" in the way of relationships in families of various sizes. Similarly, the work of the classical theorists of scientific management on span of control was useful in raising the question of how many persons or relationships a person could supervise. But even there, the work of later organizational theorists shows that such work was very risky, since in reality these limits are never attained. Mathematical models are also highly useful in positing possible relationships which may then actually be tried out (Forrester 1961). The appropriateness of the chance models offered by Mayhew and Levinger is not so clear.[1]

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[1] ED. NOTE.—Mayhew and Levinger have been invited to reply to this comment and the two following ones.

MATHEMATICAL MODELS IN THE STUDY OF POWER: COMMENT ON MAYHEW AND LEVINGER

Mayhew and Levinger (*AJS* 81 [March 1976]: 1017–49) have reported a model which attempts to explain the exclusion of individuals from effective participation in social organizations which appear to be growing larger in scale in modern societies. Beginning from apparently neutral assumptions about the decision-making process in face-to-face groups, the authors provide logical support for the theorems that (1) "the expected concentration of power is an increasing function of group size" (p. 1026), (2) "the expected relative size of the controlling component is a decreasing function of group size" (p. 1028), and (3) "the expected concentration of power within the controlling component is a decreasing function of group size" (p. 1031).

The main purpose of the model is to provide a baseline for use in empirical research. The paradox of the present model, which provides its significance as a research tool, is that the mere assumption of random interaction (i.e., equal probabilities of participation by every group member in every decision) seems to lead to a baseline model which predicts increasing concentration of power as groups become larger. Thus it is concluded that " 'conspiracy' theories of elite formation are not required to account for the occurrence of oligarchy in conditions of focused, face-to-face interaction. Oligarchic structures will develop, as we have seen, in the absence of concerted efforts by coalitions" (p. 1029), merely as an effect of group size.

Students of power and organization will be interested in the propriety of such a baseline. Particularly since the publication of Blau's (1970) theoretical discussion of the effects of system size on structural differentiation, researchers have shown special interest in the importance of size in complex organizations. The effects of size on participation and power in societies (Dahl and Tufte 1973) and communities (Clark 1968) have also been the objects of recent attention. We evaluate here the Mayhew-Levinger model and explore some alternative formulations. We argue that an appropriate model depends heavily on what the researchers believe to be the correct underlying assumptions. We intend to demonstrate that significant substantive decisions are implicit in Mayhew and Levinger's model, in particular that they have selected assumptions which heavily bias their "baseline" in favor of "oligarchic" outcomes. We examine variations of their model to determine whether size and oligarchy are necessarily related given plausible alternative assumptions.

The Sequential Decision Model

The Mayhew-Levinger formalization can be briefly summarized. Suppose that power is conceived in terms of influence exerted on discrete decisions in a continuing sequence. Suppose further than every such decision is made by only one group member and that every member has an equal chance of acting as the decision maker at every point in the sequence. This implies that for any given sequence length L (note that setting the sequence length limits the number of decisions which may be allocated among group members), the larger the group size N, the smaller the proportion of group members likely to participate in power. For large groups and short sequences ($N > L$) it is impossible for all members to participate. As the number of members grows relative to sequence length, so does the probability that more members will be excluded from decision. Thus if it is reasonable to limit the maximum sequence length to some fixed number (as Mayhew and Levinger do on the basis of "the limits of human information processing" [p. 1021]), it follows directly that increasing group size in this model implies increasing inequality in decision making.

Such inequality is measured in three ways by Mayhew and Levinger. The simplest measures are R, the proportion of members who make decisions in the given sequence, and n, the number of members who make at least one decision in a sequence. The $E(R)$ declines with group size (table 2, p. 1028), while $E(n)$ increases with group size (p. 1031) as long as the number of decisions to be made remains constant. The third measure, the "coefficient of sequence inequality," K, is used as a measure of the "concentration of power" (p. 1025). While quite complex mathematically, K turns out to be approximately the inverse of R, and $E(K)$ thus increases with group size (table 1, p. 1025).

Interpreting the Sequential Decision Model

In order to evaluate this model, we are concerned first with interpreting its theoretical implications. The implication of declining proportions of members who participate in decision making is interpreted by Mayhew and Levinger as emergence of a "ruling elite" in large groups. Increasing inequality in decision making indicated by $E(K)$ is similarly interpreted in terms of oligarchic power structure. To begin with, it should be noted that these interpretations are based on definitions of ruling elite and oligarchy which conform little to the classic elite theories. In Michels (1962), for instance, the law of oligarchy refers to the ability of minorities to attain stable control of group decision making, to represent their own rather than the majority's interests, and to deflect organizational goals to system maintenance. Generally, what is considered to be the minimal

criterion of elite status is the ability of a politically active minority to maintain control of a series of events (Dahl 1961; Bachrach 1967). In the framework provided by Mayhew and Levinger, this could be operationalized as the probability, P, that any given subgroup of q members would control all events. This is given by the following equation:

$$P = (q/N)^L, \tag{1}$$

where N is group size and L is sequence length. Under these assumptions the probability of "oligarchy" is a decreasing function of group size, the reverse of the relationship derived from the Mayhew-Levinger model. As the authors themselves demonstrate, the larger the group in this model, the more likely that every decision will be made by a different randomly drawn member; the "elites" thus constituted will be highly transient across sequences.

The phenomenon which Mayhew and Levinger have tapped in their model is actually more closely related to the theory of individual alienation and powerlessness in large-scale mass society (Coleman 1973; Mills 1956). If it can be shown that increasing proportions of members are necessarily excluded from power in larger groups, it may be irrelevant to the individual that there is no stable ruling elite. The individual may infer an oligarchical structure from the fact of his own exclusion. But it is exclusion of individuals from decision making which is implied by the sequential decision model, not oligarchy in the classical sense.

Alternative Assumptions

The implication of exclusion of individuals from participation in large groups merits closer examination. It stems from two aspects of the model: (1) the assumption that sequence length can be fixed in the five to nine range, and (2) the specification that every decision is controlled by one and only one group member. The limit to sequence length, as noted above, means that decisions are a scarce commodity; the assignment of every decision to one member means that decisions are also an indivisible commodity. Let us explore the implications of changing one or the other of these assumptions.

Let us suppose first that the number of decisions to be made, or "events" to be "controlled," is not limited to the five to nine range. Many continuing groups have discrete events that are more numerous than this range, for instance, periodic elections of officers, chairing of meetings, daily allocation of resources, and many others. In general, other than for informal face-to-face groups of short duration, there appears to be no need to limit the number of events to nine, since the persistence of a group

structure is not usually dependent on the immediate memory of individuals. Continuing groups typically establish formal procedures, authority relationships, and documentation to assure coherence over numerous discrete events. In these cases it may be shown (see the following section) that the assumption of random interaction can imply either no relationship of size to exclusion, or even an inverse relationship if added numbers increase disproportionately the number of decisions to be made. The latter inference is not unreasonable; it has been argued, for instance, that size increases system complexity (Dahl and Tufte 1973; Blau and Schoenherr 1970), and this complexity may multiply the available number of events that need to be controlled at a faster rate than the increase in the number of members, since the number of system events would be a function of the possible relationships between members (Graicunas 1933). If sequence length advances at the same rate as system size, concentration of power will remain at least approximately equal by the $E(K)$ measure (table 1, p. 1025); and if length advances only somewhat faster than size, concentration of power will remain equal by the $E(R)$ measure (table 2, p. 1028).

Let us now consider the stipulation in the Mayhew-Levinger model that a decision can be made by one person only. It follows that the proportion of members involved in any single decision varies inversely with size; it is simply $1/N$. This is in fact an assumption of a rather pronounced form of concentration of power. There may well be contexts in which it is reasonable to designate a single person as in some sense controlling a single event (e.g., the initiator of a proposal in a small group). But a model based on such an assumption seems inapplicable to a broad range of contexts in which decisions are made by coalitions within groups, by majority rule, etc. These are the cases in which, not having assumed one-person rule on discrete decisions, it is especially interesting to test the effects of group size on the pattern of participation across a series of decisions.

Admitting the possibility of multiple participation in each decision has one automatic consequence regardless of the actual decision rule employed: The expected value of inequality at any given size and sequence length declines according to the likelihood and degree of multiple participation. Even a mild relaxation of the Mayhew-Levinger assumption of one-person decisions has a substantial effect. Suppose, for example, that it requires two persons to make a decision. Then the probability that any given group member will be excluded from every decision declines from $[(N-1)/N]L$ to $[(N-2)/N]L$. For sequence length five and a group size of 21, the probability changes from .784 to .606. With larger sequence lengths and for smaller group sizes the contrasts are even more striking.

Generalizing the Mayhew-Levinger Model

Of course every model involves a certain number of simplifying assumptions and a certain abstraction from reality. In this instance we argue that it is not necessary to be limited to the assumptions used in the Mayhew-Levinger model, and that alternative assumptions produce different relationships between size and oligarchy. It is in fact possible to work from a more general model in which alternative assumptions concerning sequence length and participation in discrete decisions may be explicitly introduced.

Let us suppose that in a group of size N there is a series of L discrete decisions to be made. Let us further suppose that the proportion of members, p, who participate in decisions is the same for each decision in the series. For the purpose of building a baseline model (providing the appropriate null hypothesis for testing the theory that inequality in large groups is due to nonrandom processes such as coalition building, etc.), we also suppose that all group members have an equal chance of participating in each decision. We can then provide equations linking system size to the expected number, $E(n)$, or the expected proportion, $E(R)$, of members who participate in at least one decision. Any member's probability of participation in at least one decision is $1 - (1 - p)^L$. Across a large number of sequences this is also the expression for the average proportion of members who participate in at least one decision. Thus:

$$E(R) = 1 - (1 - p)^L, \tag{2}$$

$$E(n) = N\,[1 - (1 - p)^L]\,. \tag{3}$$

For $0 < p < 1$, it is easily seen that $E(R)$ and $E(n)$ are both increasing functions of sequence length L; this finding reproduces a result reported by Mayhew and Levinger but without assuming one-person rule on each decision. With unlimited L, $E(R)$ approaches its limit of 1 and $E(n)$ approaches N. It is the case, as would be intuitively anticipated, that $E(R)$ and $E(n)$ increase with p, the probability of each member's participation in any single decision. However, while $E(n)$ increases linearly with group size N, $E(R)$ has no necessary relation to N in the general model.

In some instances it may be possible to introduce additional assumptions concerning the relationships among the parameters of the general model. Preferably these assumptions will be based on an assessment of the empirical context in which the model will be applied. It is likely that such modifications will alter the baseline predictions of equations (2) and (3), as we show in the following examples:

1. Suppose, as in the Mayhew-Levinger model, that only one person can be involved in any single decision. Then $p = 1/N$, and p necessarily declines with system size. In this case:

$$E(R)' = 1 - (1 - 1/N)^L, \quad (4)$$

$$E(n)' = N[1 - (1 - 1/N)^L]. \quad (5)$$

Now, with L held constant, $E(R)$ declines with size, as Mayhew and Levinger report, and $E(n)$ increases with a limit of L as N approaches infinity.

2. Suppose alternatively that there is minority rule on all decisions ($p < .5$), but that due to increased complexity there is also a trend toward decentralization in large groups so that the number of persons, q, involved in any group decision increases with N (Blau and Schoenherr 1970; Chandler 1962; Thompson 1967, p. 136). Then $p = q/N$. We found in the general model that, with L and p held constant, $E(R)$ is not related to N. In the present case $E(R)$ increases with N for values of N where $dq/dN > 1$, since p must necessarily increase, forcing an increase in R. When $dq/dN < 1$, $E(R)$ declines since p is then decreasing. However, $E(n)$ always increases with N, with L held constant.

3. Finally, suppose that the number and kinds of decisions in groups increase with size, as suggested earlier. In this case, $L = f(n)$ such that $dL > dN$. If it is assumed that p is held constant, it is easy to see that $E(R)$ will increase with L, since there are no other variables in the model (see eq. [2]). And it should be obvious that $E(n)$ will increase even more as a function of N. The more interesting case where q is held constant and $dL > dN$ is more difficult, but as noted earlier table 2 (p. 1028) indicates that if the rate of change of L is somewhat more than the rate of change of N, $E(R)$ will not decrease with group size.

The general model has been elaborated for only two measures of inequality, $E(R)$ and $E(n)$. It should be possible to investigate also the relationship of $E(K)$ to p, L, and n in the same way, although extending the Mayhew-Levinger results to various values of p would be exceedingly tedious. We would expect to find that $E(K)$ behaves similarly to the inverse of $E(R)$ and would have no necessary relation to N in the absence of additional restrictive assumptions.

Thus the Mayhew-Levinger model has been shown to be a special case of a more general sequential decision model. In the more general model, there is no fixed relationship between group size and concentration of decision making. Additional theoretical assumptions can be made which alter this result, but such assumptions should be chosen only after consideration of the whole range of alternatives: how are single decisions made, how many decisions are there in the sequence, how is the sequence size related to group size, and how does decision making on discrete decisions vary with group size?

Application of the Baseline Model

In their discussion of the model as a baseline predictor (p. 1034), Mayhew and Levinger do not claim that the model applies deterministically, since they admit the possibility that actual power structures may deviate from that predicted by the model. Presumably deviations from the expected values would be caused by such social factors as widespread education, mandated rotation of positions, mass organization, etc. However, what is troublesome to us about their presentation is the repeated statement that oligarchy is the expected outcome merely of size and random interaction. In fact, as we have shown, further assumptions are made (one-person rule, limit of sequence length) that go much beyond that of "random interaction." We have demonstrated here that (*a*) under a more plausible definition of oligarchy—namely, elite domination of a series of decisions—oligarchy is *decreasingly* likely as a function of size, if one makes precisely the same assumptions as in the Mayhew-Levinger model; and (*b*) using their definitions of concentration of power but alternative assumptions, one may find either a positive relation, a negative relation, or no relation between size and expected concentration of power. Oligarchy, or concentration of power, therefore cannot be considered a function of size plus random interaction.

The existence of alternative assumptions does not imply the impossibility of fruitful use of such baseline models in empirical research. It is, however, necessary to articulate and justify the assumptions used.

Any of the variants of the general sequential decision model can in a sense be tested by comparing its expected concentration of decision making $E(R)$ at various size levels with the actual values of R derived from empirical research. It is not correct, however, to presume that deviation from $E(R)$ is a test of the assumption of equal probabilities of decision making by every group member. Deviations might as well result from errors in any other assumption of the model.

With the proper data it is possible to test every element of the general model and thus provide empirical estimates of R, n, p, and L at various values of N and to estimate the relationships among N, p, and L. Once the model is corrected according to these findings, the quantity $R - E(R)$ does represent the degree to which some group members have better than chance probabilities of participation and/or the degree to which participation in one decision affects participation in later ones.

Conclusion

The rate of publication of mathematical formalizations of social theory in recent years suggests a growing concern among sociologists with precision

and clarity in their theoretical statements. The Mayhew-Levinger model is only one of numerous formulations of the expected relationship between system size and structural differentiation, concentration of power, and individual alienation (Hummon 1971; Coleman 1973; Niemi and Weisberg 1972). Their model is not untypical in illustrating some problems in the development of mathematical sociology. Lazarsfeld and Henry, in the introduction to their early collection of readings in mathematical social science, have stated succinctly the utility of mathematical models: "Sometimes they provide a language that tends to clarify the assumptions behind, and the consequences of ideas that are rather vague when expressed in conventional verbal terms. In other cases mathematical models represent real theories of their own that can be tested against empirical data; some of these models provide rationales for organizing and sifting the increasing amount of data that is available from experiments and surveys" (Lazarsfeld and Henry 1968, p. 3). In principle no mathematical sociologist will disagree with this statement. In practice sociologists sometimes commit one or another of the following errors when dealing with formal models:

1. Reification: A mathematical model is a statement of theory, with no necessary empirical validity. But the development of elaborate and complex measures and logical deductions may create an illusion that the conclusions derived from a model are proven valid statements about the social world—the kind of "synthetic a priori" statements that have been so attractive but elusive to philosophers. Mathematical sociologists may slip into this trap even when they explicitly deny empirical validity to their model.

2. Neglect of plausible alternative assumptions: A formal model necessarily relies on its initial definitions and assumptions, and indeed most critical discussion of existing models has focused on the propriety of the particular definitions and assumptions that were employed. Yet sociologists often state these choices briefly or in a mathematical form which deflects attention from their substantive meaning and validity and devote themselves primarily to drawing out their implications. In order to make the model fully accessible and open to evaluation by less mathematically oriented researchers, the mathematical sociologist has a responsibility to evaluate and defend the theory of society embodied in his initial assumptions and to consider the consequences of reasonable alternatives as explicitly and precisely as he manipulates the resulting equations.

3. Displacement: While it is necessary to simplify reality in order to represent it in a formal model, there is a temptation to operationalize important variables in terms most amenable to their uses in the model. A theoretically complex concept (e.g., oligarchy) will be reduced to a sim-

pler or more directly measurable indicator (e.g., inequality in decis making in brief sequences), leading to a misinterpretation of the resu Thus while the intention is to focus on a certain aspect of social real: attention is displaced to an indicator that has limited or even doubt validity. Displacement seems especially to occur in the study of pow a phenomenon which has proven to be particularly intractable to efforts of mathematical sociology (a well-known example of such mis terpretation is the M.O.P. ratio as a measure of concentration of pow in communities [Aiken 1970, p. 503]).

We thus wish to urge caution in the interpretation of mathemati baseline models such as the one presented by Mayhew and Levinger. ' feel unwarranted conclusions can be drawn by readers dazzled by model's complexity and unaware of its limitations. The clear implicat of their model is that size as a structural property has a direct posit effect on concentration of power, an effect that in the absence of ot factors and in the long run is deterministic. We have shown that s is in fact associated with concentration of power only under cert restrictive assumptions concerning social reality. Distinguishing, evalu ing, and testing those and alternative assumptions are the tasks of empirically based sociology; mathematical models will provide no shortc

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COMMENT ON "OLIGARCHY IN HUMAN INTERACTION"

Mayhew and Levinger (*AJS* 81 [March 1976]: 1017–49) claim to show that Michels's "iron law of oligarchy" and Mosca's major proposition on system size and ruling elites can be formally derived from the nature of human interaction. It is my contention that one of their theorems is incorrectly stated, that another (although true) is incorrectly proved, and that the derivation of all five is based on a restriction that excludes most of the sorts of power situations in which Michels, Mosca, and sociologists since have been interested.

Mayhew and Levinger begin by defining focused interaction as situations of face-to-face communication and consider a group composed of N actors in which the communication occurs. (It will be understood throughout this comment that actresses are equally permitted.) The situation is characterized as a series of L communication events, and each event is characterized by the identity of the speaker.[1] If we consider the complete sequence of L communications, each actor i can be characterized by the proportion of events which were his, $0 \leq p_i \leq 1$.

Each particular sequence structure can be described by a coefficient of inequality:

$$K = \frac{\frac{1}{2}\sum_{i=1}^{N}\sum_{j=1}^{N}|p_i - p_j|}{(N-1)}.$$

Mayhew and Levinger note that $K = 1$ when one person speaks all the time, and that $K = 0$ only when all N actors speak in equal proportions.

Mayhew and Levinger then consider "a range of sequence lengths that are operationally relevant to human interaction" (p. 1021). They suggest that the findings of Miller, Simon, and others on the limits of human primary memory are applicable, and that relevant sequence lengths range from five

[1] Mayhew and Levinger permit an actor to follow himself in the sequence structure. This suggests that the description results from some temporal sampling, a process which permits significant sampling error (e.g., a firm no is more likely to be missed than a long-winded equivocation). If the sequence is not a sample, then it is not clear how an actor can follow himself.

to nine events. Mayhew and Levinger fail to show any basis for assuming that human interaction is governed only by primary memory. In footnote 10, pages 1021 and 1022, they state that this short sequence may be a condensed representation of a longer one. This presents a problem, because each event can be characterized only by a dominating actor. It is not possible to represent an event as a tie between two or more actors, although a condensed representation of a longer sequence might require doing so. It is not clear from their presentation whether the coefficient of inequality is proposed as a way in which the actor's "primary memory must operate to visualize power structure" (p. 1022), or a way in which a social scientist can describe the power structure.[2]

Mayhew and Levinger assume that the interaction results from a random process: $P(i_t|j_{t-1}) = 1/N$; $P(i_t) = 1/N$. They consider then the ways in which K behaves under certain conditions and calculate expected values for K by denumeration.

I contend that it is more fruitful to derive a formula for the expected value of K: $E(K) = [\frac{1}{2}N^2E(|p_i - p_j|)]/(N-1) = \{\frac{1}{2}N^2\sqrt{[2 \text{ var }(p)]}\}/(N-1)$. To show that $E(|p_i - p_j|) = \sqrt{[2 \text{ var }(p)]}$, consider $(|p_i - p_j|)^2 = (p_i - p_j)^2 = [(p_i - \mu) - (p_j - \mu)]^2$,
where $\mu = E(p)$. Then

$$\begin{aligned} E[(|p_i - p_j|)^2] &= E[(p_i - \mu)^2 - 2(p_i - \mu)(p_j - \mu) + (p_j - \mu)^2] \\ &= E[(p_i - \mu)^2] - 2E[(p_i - \mu)(p_j - \mu)] + E[(p_j - \mu)^2] \\ &= 2 \text{ var }(p) - 2[E(p_ip_j) - \mu E(p_i) - \mu E(p_j) + E(\mu^2)] \\ &= 2 \text{ var }(p) - 2[E(p_i)E(p_j) - \text{cov }(p_ip_j) - \mu^2] . \end{aligned}$$

Because p_i is independent of p_j, the covariance is zero, and the expression becomes

$$\begin{aligned} &= 2 \text{ var }(p) - 2[\mu^2 - 0 - \mu^2] \\ &= 2 \text{ var }(p) . \end{aligned}$$

To show that this is the square of the desired expression, it is sufficient to show that $[E(|p_i - p_j|)]^2 = E[(|p_i - p_j|)^2]$. As follows: $[E(|p_i - p_j|)]^2 = E[(|p_i - p_j|)^2] - \text{cov }[(|p_i - p_j|)(|p_i - p_j|)]$. Again, because the p_i's and p_j's are independent of each other, the covariance is zero, and the equality is proved.

[2] An anonymous referee of this comment has pointed out the additional problem of variable retention of events. The communication events selected by an observer may not be the same as those retained by each and every participant. As one example suggested by the referee, one participant may be retaining only those events in which he is personally involved, while another is retaining each event in sequences of nine as is assumed by the authors.

This same expression for the expected value of the absolute value of $(p_i - p_j)$ can be derived using Chebyshev's Inequality.

We can use this expression for the expected value of K to determine the behavior of K under varying conditions. Consider any actor i. The probability that he speaks k times in the sequence of L events $(k \leq L)$ is distributed binomially with

$$p = \frac{1}{N}$$

$$\mu = \frac{L}{N}$$

$$\sigma^2 = \frac{L(N-1)}{N^2}.$$

Then the probability that he speaks (k/L) proportion of the time is also distributed binomially with

$$p = \frac{1}{N}$$

$$\mu = \frac{1}{N}$$

$$\sigma^2 = \frac{(N-1)}{LN^2}.$$

This σ^2 is the variance in the proportion of time an actor speaks, or var (p). Then the expected value of K equals

$$E(K) = \frac{\frac{1}{2}N^2\sqrt{[2(N-1)/LN^2]}}{(N-1)}$$

$$= \frac{N}{\sqrt{[2(N-1)L]}}.$$

To prove Mayhew and Levinger's theorem 1, it is necessary only to take the first derivative of the above expression with respect to L: $dE(K)/dL = -N/2L\sqrt{[2(N-1)L]}$, which is negative for all N and L greater than 1. By taking the second derivative, it can be shown that the first derivative is increasing. In the limit, as L becomes exceedingly large, $E(K)$ approaches zero.

Mayhew and Levinger correctly show that, for L less than N, the minimum value for K is an increasing function of N because not everyone can speak. $K_{min} = (N - L)/(N - 1)$. If a group were characterized by a certain K_{min} at some point in time, and if the size of the group grew and the speaking

opportunities did not increase, the group would of necessity become less egalitarian.

$$\lim_{N \to \infty} K_{min} = 1 \, .$$

This is sufficient to prove a revised theorem 3: As group size increases without bound and L remains constant, the expected concentration of power approaches its maximum.

Since this result depends on N being much greater than L, one can question the extent to which it describes situations that are of interest and concern to sociologists. A more interesting question to ask is: if N increases by some proportion, how much must L increase in order to maintain the same expected value of K? It can be shown that L must be increased by slightly more than the same proportion in order to maintain the same expected value.

To prove theorem 2, Mayhew and Levinger reason from the increasing minimum values of K. More properly, we need to consider the derivative of the expected value of K with respect to N: $dE(K)/dN = [\surd(N - 1) - N/2\surd(N - 1)]/\surd(2L)(N - 1)$. The sign of the derivative will be determined by the numerator of the term in the brackets. This expression is positive when $\surd(N - 1) > [N/2\surd(N - 1)]$, $N > 2$. So, for all $N > 2$ the expected concentration of power is an increasing function of group size.

Mayhew and Levinger define the ruling component of a group as that proportion, $R = n/N$, composed of the n actors who control an event. They then support theorem 4 by reasoning from the maximum values of R: $R_{max} = L/N$ for $L \leq N$.

A more proper proof of theorem 4 (Mayhew's restatement of Mosca's proposition), and one that includes a proof of theorem 5, is as follows: The L speaking events will be distributed among the N actors by a Poisson process (see Feller 1968, pp. 160–61). The expected number of events per actor is L/N. In the Poisson distribution, $\lambda t = L/N$. We can divide the N actors into n members of the ruling component and m actors who control no event: $n = N - m$. To find an expression for the expected size of the ruling component, it is sufficient to find a formula for the expected size of the silent majority: $E(n) = N - E(m)$. According to the Poisson distribution,

$$E(m) = Np(0; L/N)$$

$$= Ne^{-L/N} \, ,$$

$$E(n) = N(1 - e^{-L/N}) \, .$$

To determine the effect of increasing group size on the size of the ruling component, we need only take the first derivative of $E(n)$ with respect to N: $dE(n)/dN = (1 - e^{-L/N}) - (L/N)e^{-L/N}$. This is positive if

$$(1 - e^{-L/N}) > \frac{L}{N} e^{-L/N}$$

$$e^{L/N} > 1 + \frac{L}{N}$$

$$\frac{L}{N} > \ln\left(1 + \frac{L}{N}\right).$$

Using the Taylor expansion of the natural logarithm (Feller 1968, p. 51), the right-hand side of the above expression can be shown to be less than the left side for all $L \leq N$.

This result is sufficient to prove theorem 5. As n goes to L, var (p) goes to zero and $E(K)$ goes to zero.

To prove theorem 4 we need to consider the first derivative of R:

$$E(R) = \frac{E(n)}{N}$$

$$\frac{dE(R)}{dN} = \frac{N\frac{dE(n)}{dN} - E(n)}{N^2}$$

$$= \frac{\frac{dE(n)}{dN}}{N} - \frac{(1 - e^{-L/N})}{N}.$$

For the derivative to be negative it is sufficient that

$$(1 - e^{-L/N}) - \frac{L}{N} e^{-L/N} < (1 - e^{-L/N})$$

$$e^{-L/N} > 0,$$

which is true for all L and N greater than 1.

Finally, if we want to know the probabilistic size tipping point for a given L, $L < N$, it can be determined by setting

$$E(R) = \tfrac{1}{2}$$

$$E(R) = \frac{E(n)}{N} = \tfrac{1}{2}$$

$$(1 - e^{-L/N}) = \tfrac{1}{2}$$

$$N = \frac{L}{\ln 2}$$

$$\cong 1.44L.$$

The deterministic size tipping point is $N = 2L + 1$.

The point of this comment is not that the results of Mayhew and Levinger are wrong. They are basically correct. I have tried to show that they can be stated more precisely and proven more generally than the techniques of denumeration and computation permit. This should allow more exacting tests of the predictions of the theoretical process. I leave it to others to take up the question of the appropriateness of the conceptualization.

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COMMENT ON RALPH TURNER'S "THE REAL SELF: FROM INSTITUTION TO IMPULSE"

I am neither a disinterested nor an objective reader of Professor Turner's paper (*AJS* 81 [March 1976]: 989–1016). I read it through the lens of my own experience and the filter of the abstract content of the cognitive structures which seem a comfortable representation of that experience. When new experience is powerful enough to threaten the structural frames of my vision, I tighten or elaborate the frame so as to either exclude or assimilate the experience. Only when I feel particularly strong do I reflect on the adequacy of the information-processing structure itself. That is why I admire Turner's courage in admitting that a basic sociological framework (although he does not identify it as his own) is unable to assimilate a new set of experiences and in suggesting that we need alternative theories or, at least, important revisions of existing ones.

This basic sociological framework is that of moral integration. Social order is possible because individuals share moral commitments which are enacted as conformity to the role prescriptions embedded in social institutions. The new set of experiences which makes this social practice and theory problematic results from the fact that more people, especially younger ones, in the United States seem to locate their "real selves," the core of their self-definitions, in the satisfaction of "noninstitutional impulses." This newer locus of self-definition deflects from the perception of institutionalized values and conformity to them as the criterion of self-validation and adequacy, thereby making social control increasingly problematic.

Turner offers a number of examples in his effort to differentiate between the two loci of self-definition, the "institutional self" and the "impulsive self," and then describes several theories which might be developed in order to explain his perception that there has been a historical shift in the United States during the "past several decades" from an institutional real self to an impulsive real self. However, despite his intellectual openness, I think that Turner's description of the shift and his understanding of its social sources and consequences is limited by his continued use of the very framework which he believes to be inadequate. The use of this framework is evidenced first by the formulation of the problem itself—that a shift in the locus of self-anchorage be posed as a problem of social control. Although concern with social control is not limited to the moral-integration model, it has traditionally been its central practical and theoretical "interest." Second, the moral integration model poses a dichotomy between society and its needs and the individual and his needs. I think that Turner fundamentally recapitulates this dichotomy, even while observing that it is possible to see some social and individual aspects in both types of self-definitions. The important locus of continuity with moral integration theory, however, is in the historical abstraction of two clusters of concrete human commitments, which are then reified as representing "society" and "individual." By moving his analysis from the level of values and norms to that of self-definitions, Turner goes beyond the typical moral integration view but, on the whole, perpetuates the dichotomy. Rather than identifying one set of concrete commitments and self-definitions as "institutional," and seeing another set of commitments as "impulsive" challenges to the social order, we might instead deabstract or further ground the institutional self in historical society. For example, I think that a strong case could be made that the components of institutional self-anchorage, which now seem so absolute that it is easy to slide into identifying them with society, were themselves once part of a broad set of social structural and self-definitional changes which appeared to threaten society (which Turner recognizes but does not develop). The characteristics of the institutional self seem to me remarkably similar to the virtues of the rising bourgeoisie, especially when abstractions like "volition," "achievement," "morality," and "altruism" are seen in terms of their constituent institutional behaviors. Viewed concretely, they represent the self-definitions of the "free-market" individual and the historically developed loyalties to occupational specialization and nuclear family. In short, by moving dangerously close to the reified society-individual dichotomy, called here "institution-impulsive," and by deemphasizing the historical concreteness and social structural connectedness of self-definitions, which he does allude to, Turner increases the difficulty of achieving one of his major purposes: "the articulation of real selves with social structure." The attainment of this aim is made even more

difficult by another direction of analysis, which is also typical of the moral integration model. Although Turner assumes that "self-conception . . . is most usefully viewed as a variable intervening between some aspect of social structure and the working of the same or another aspect" (p. 990), I do not believe that he has specified the structural sources of changed self-definition, or that he has given a fair representation of those aspects of social structure which might be affected by changed self-definitions. Specifically, there is a relative neglect of the social structure of production and an overemphasis on role definitions, sentiments, values and norms, and ritual. This emphasis is reminiscent of the tendency of proponents of the moral-integration model to analyze society primarily in terms of morality, ideas, and conceptions. I think that continuing this tradition makes the articulation of the relationship between self-definition and social structure more difficult and hinders the effort to develop alternative theories of social control.

Similarly, it is difficult to describe a complex and changing set of specific interactions between aspects of social structure and self-definitions when self is reified and seen as an abstraction that stands outside of the organization of interactional networks constituting social structure. A dichotomy which usefully clarifies the distinctiveness of self and social structure may, simultaneously, so far separate the two concepts that the possibilities of seeing their changing and mediating linkages are diminished. Clearly, this is not Turner's intention, for he writes that ". . . few of the instigations and sensations that people experience as impulse are not institutionally conditioned and generated" (p. 1012). Yet, the particular set of self-anchorages to be explained, the impulsive self, is seen as outside of society or, more precisely though metaphorically, as a player, waiting in the wings, who can appear on stage only when the play breaks down (the "lowered inhibitions" of the repressed "impulses," "mad desire") or has been fully enacted. When Turner does discuss social structure in terms of production and exchange relationships it is only after production problems are routinized or "resolved" that the "impulsive self" appears. Social relations can influence the impulsive self as a trigger or catalyst, but its apparently static core consists of "deep, unsocialized, inner impulses." (This characterization seems to belong both to the impulsive actor and the objective analyst.)

I believe that the impulsive self like the institutional self can be regrounded in historical social relations rather than abstracted from them and reified. There is no more reason to see the specific characteristics of the impulsive self—spontaneous action, self-discovery, decreased "self-control," expression of "human frailties," distrust of rules which appear arbitrary—as signifying the breakout of an ahistorical self, than there is to see the characteristics of the institutional self as being society. To some

extent, all these behaviors are rooted in the capacities of the organism, and all are subject to social construction. Then, I think it becomes somewhat easier to see self-anchorage more fully as a constituent of social process.

Turner does, I think, offer a number of stimulating insights into how this can be accomplished. He criticizes a cultural explanation for the shift of self-anchorage because ". . . it accounts for a decline in the institutional locus of self without indicating the basis for increasing impulse locus" (p. 1001). He observes that "if modern civilization had also frustrated mankind's need for interpersonal response, it is easy to understand that people may experience their longings for intimacy as manifestations of their real selves" (pp. 1003–4). He notes that "one key [to understanding changes in self-conception] may be the shift from the organization of group life around production and mutual protection groups to organization around consumption" (p. 1002), and, finally, that ". . . modern society is not so much repressive as it is contradictory, stimulating to an unparalleled degree the impulses whose expression is then inhibited" (p. 1004).

By elaborating these insights, I think we can sketch the outlines of an alternative model. Specifically, I would suggest that the impulsive self can be understood as the expression of a newer set of needs, which are related to changes in the organization of production and class structure. The revolutionary appearance of these needs and self-definitions may, as Turner observes, obscure their ascendance and incorporation into the societal structure of control. However, it is particularly difficult to envision this because, as Turner describes it, the impulsive self seems so much directed toward individual self-expression and so abstracted from, and even antithetical to, any form of social organization at all. However, the impulsive self can be socially reanchored, I think, both by the sociologist and by the social actor. By emphasizing changes in the social structural context of changing self-definition, one could argue not only that the impulsive self might some day be socially tamed, but also that the impulsive self now represents both new needs *and* a form of moral consensus or institutional loyalty at the level of self-definition. In other words, *the impulsive self is not a challenge to social control. It is rather the expression of a different mode of social control.*

I agree that it is very difficult for both the social actor and the social analyst to see the impulsive self as connected to social structure and, even further, as a process of social control. But that is precisely because of the form which social production increasingly takes in advanced capitalism; and the difficulty of seeing highly individualized needs with an enormous stress on self-gratification as an aspect of social control is indeed one of the major socially patterned defenses for the maintenance of the current system.

I think that this apparently contradictory process can be better compre-

hended by an analysis which begins with the tradition of "commodity fetishism" (Marx 1967; Lukacs 1971; Marcuse 1964; Gorz 1964) rather than with the moral integration approach. According to this model, lacking direct control of their means of production and conscious participation in a transparent social regulation of production and distribution, individuals see their own and others' products as alien objects which they must acquire for survival. Losing sight of the process by which their own capacities are in fact appropriated and transformed into commodities, they accept the capitalist market model of individual accumulation. In this way the grounding of social production in the exercise of human capacities through social relationships is obscured, so that finally individuals see themselves, their needs, and self-definitions increasingly in terms of consumption, with the result that they abandon efforts to control the expression of their capacities in the process of production. Thus, social control occurs through a process of the mystification and reification of relations of production. The stability of social organization is insured not by moral consensus or institutional loyalty, but instead by a rechanneling of individual energies and awarenesses toward activities that are less determinant of social structure and in a way which individualizes people's definitions of social organization and fragments their capacities for active, collective control of some of their most central activities. In other words, I am suggesting that social control now occurs increasingly less through moral concentration and commitment to social institutions and more through a process of social *diffusion,* in which extreme "*individual*-ization" rather than "*social*-ization" creates social stability. From this point of view, the emphasis on self-gratification is precisely that which does not threaten the social order, as long as it is directed away from the social organization of production.

Of course, this is a very general formulation, and one can imagine, for example, concrete situations in which a lack of synchronization between consumption demands and production resources might become socially disruptive. Also, in its early stages, the expression of the "revolutionary" demands of the impulsive self may stir things up so much and lead to such a questioning of existing legitimations that the social relations which support and encourage the impulsive self may themselves enter the realm of the problematic. For the present, however, I think that what Turner calls the impulsive self is not revolutionary but fits into the existing social totality rather well. Let me offer some nonsystematic and brief examples of how the impulsive self fits into the overall social organization of production both as a set of new needs and as a process of social control.

It might be argued that the desires for "self-discovery," "spontaneity," and "lowered inhibitions" represent changed needs of production, both for the ruling class and the increasingly educated, technologically employed

working class. As the earlier stages of broad capital expansion and empire building come to be replaced by the greater elaboration and intensification of capital and the use of more limited resources, the importance of innovation increases, and the ruling class takes on more characteristics of "foxes" than of "lions." In modern terms, I think that the requirements of innovation under these circumstances support a different kind of psychological mobilization, a digging deep into different aspects of the self for competitive solutions and discoveries. Similarly, spontaneity might be related to the production need for quicker turnaround times and immediate feedback. Lowered inhibitions might be thought of as a "loosening up," a generalized preface to a more total, more intensive use of the self for capital accumulation. These needs and self-definitions, it might be argued, become more widespread not simply as the legitimation of ruling class needs, but also because of the changing job requirements of workers. This generalization of needs and, ultimately, self-definitions, like the increasing material consumption standards of workers, is permitted and even encouraged, because it, too, ultimately increases production and the accumulation of capital. Also, as Turner notes, the impulsive self-definition may be related to consumption—as a way of intensifying the interest and ability of individuals to consume, an intensification of individual consumption which serves as a complement and, when necessary, as an alternative to continuously expanding external markets.

While these newer needs and definitions of the impulsive self might be utilized in production as well as in consumption, its social control aspect lies in commodity fetishism. I mean by this that social actors come to focus on these aspects of the self in terms of consumption, making even their partial utilization in production less apparent, and that their mobilization in consumption occurs in a way that saps their revolutionary potential. The impulsive self represents a standardized commodity form of those very same new capacities used in production, a form which renders their creative potential invisible to the individual by representing social relational capacities of production as individual characteristics, which can only be realized through increased *individual* consumption and not through the social relations of production themselves. For example, I see the need for "making intimate self-revelations" as a distorted, stylized form of the capacity for human communication and attachment, a capacity which is highly controlled and frustrated in production, but is now realizable as the expression of a socially abstracted individual. Similarly, I understand the desire for spontaneity as a distorted, individualized commodification of the human capacity for exercising freedom in social relationships. In this way, the impulsive self does not express noninstitutionalized human capacities or even "impulses" rooted in the potential of the organism, but signifies instead the appropriation of these capacities to serve socially

manageable needs and self-definitions. It is not, therefore, the expression of these appropriated, individualized needs and self-definitions which most threatens the preservation of the status quo. Rather, it is the realization in social relationships of the capacities on which they are based which would most unsettle not society in general but the organization of this particular society. This commodification and individualization of needs does not provide for eternal social control, for I do not think that the contradictions which I have mentioned can be simply resolved by "logical" argument. For example, while production may increasingly require the utilization of different kinds of human capacities, these very same capacities are frustrated by the social organization of production and are instead realized primarily through fetishized individual consumption outside the productive sphere. While the impulsive self may be disruptive of existing institutional legitimations and generate insatiable and disruptive consumption-oriented self-identities, it does provide a mechanism for social control which gives the appearance of individual freedom from social constraint.

Thus, I agree with Turner that ". . . if people recognize their real selves in impulse, they become susceptible to social control from that quarter" (p. 1011). I have tried to indicate very briefly how a major historical shift in self-anchorage might be related to changes in the broader social context and how that shift can be understood by emphasizing an alternative mode of social control. This model, which relates capacities and needs both to changing forms of production and to changing loci of self-definition, obviously requires a great deal more elaboration and specification. Eventually, I hope to be able to specify the linkages and transformations of historically specific expressions of human capacities, their appropriation into the dominant, changing forms of production, their early self-representation (perhaps as impulses) and their later institutionalization and social legitimation. I agree with Turner both that ". . . people are not just miniature reproductions of their societies" (p. 989), and that the relationship of particular aspects of the self to ". . . recognized institutional structures will be complex and indirect" (p. 1011). But I do think that the "self" is an integral part of the historically changing forms of social production.

Finally, I want to emphasize the fact that while we might understand social control from this point of view—one which emphasizes that the production of commodities and the extensive commodification of the producers themselves rather than the maintenance of moral conformity form the central social dynamic of contemporary American society—"social control" is neither the theoretical nor the practical interest of this approach. On the contrary, I think that we are now doing all too well in the area of social control by utilizing a combination of the internalization of older institutional loyalties and an individualized commodification of newer needs and self-definitions. What "we need," I believe, is not so much more effective

social control, and a theoretical guide for achieving it, as a satisfaction and realization of human capacities. The theoretical and empirical task of a critical social psychology is to describe better how our current forms of social organization prevent the satisfaction and realization of human capacities and to show how society can be organized so as to maximize the realization of the human capacities of all its members. What we need then, most of all, is not a new theory of social control, but a theory of social liberation; a theory which appreciates, both critically and practically, the centrality of psychological bondage in this society and which acknowledges that liberation requires the recognition of human relational capacities as a constituent in the social process and as a social goal.

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REPLY TO PHILIP WEXLER

Philip Wexler's thoughtful exposition is a welcome addition to discussion of the changing anchorage of self-conceptions. It is altogether consistent with the inductive and tentative spirit of the original article that additional explanations should be advanced. Without denigrating Wexler's contribution I should like to highlight what I think are the major issues on which our viewpoints diverge.

1. Wexler's assumption that I suppose the impulse self actually to be based on deep, unsocialized, inner impulses is a serious misreading. I had hoped to make clear the view that the impulse self is just as fully a social product as the institutional self and that impulsives are engaged in self-deception when they see themselves as freer and more spontaneous than institutionals. It is in this connection that sociology's most widely heralded theories of social control show clearly how an institutional self-conception subjects one to control but are less helpful in showing how an impulsive self-conception can do similarly.

2. Wexler proceeds on the unwarranted assumption that my interest in the social control implications of self-conception serves the goal of developing a prescription for more effective social control. In the liberal tra-

dition of Karl Mannheim, the Chicago School, and mainstream Anglo-American sociology, the desire to expose the dynamics of social control reveals an ambivalence toward, or even abhorrence of, social control. I am of the conviction that one of sociology's most important contributions can be the exposure of the hidden constraints that compromise our freedom.

3. In the repeated references to "moral-integration" theory and ahistoricity I sense a determined effort to reshape my views until they fit the Procrustean bed that Wexler has prepared for the occasion. I do not find these polarities useful in distinguishing my approach from others. Part of the difficulty seems to arise from failure to appreciate an analytic strategy that identifies both immediate and removed causes for the configuration of the self.

4. I believe there is a significant contrast between the naturalistic perspective in the original paper and the teleological perspective in some portions of Wexler's analysis. The conspiratorial functionalism that Wexler employs is more credible than the unconscious-system-wisdom brand of functionalism. But it still requires too monolithic a view of societal power structure and too godlike an omniscience on the part of the ruling classes about what psychological dispositions will serve their ends and how these dispositions are instilled.

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Review Essay: The Once and Future Marx

The Twilight of Capitalism. By Michael Harrington. New York: Simon & Schuster, 1976. Pp. 446. $10.95.

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Michael Harrington begins *The Twilight of Capitalism* with the startling premise that Paul Samuelson, Joan Robinson, Louis Althusser, Erich Fromm, Hannah Arendt, Raymond Aron, and I, in one way or another, have misinterpreted, misunderstood, and even misquoted Marx and that he will present not just a possibly better or more comprehensive reading but, to quote him, the "authentic Marx," the "real Marx" (and presumably, then, the only rational Marx), known hitherto only to a gnostic "underground" (his word again) but whose second coming is at hand, since the resurrection of the old scrolls is now complete.

This is surely an extraordinary claim. How could so many well-known and even distinguished scholars mislead themselves and thus their readers? It turns out on closer examination that Harrington believes the real culprits were Marx and Engels themselves:

a) In *The German Ideology,* Marx and Engels committed a "youthful indiscretion" (Harrington's phrase) in seeking a universal key to human history, which "they never formally retracted," though in later writings they sought to relate sociological generalizations only to the stage of development of the society. "Youthful indiscretion" it may have been, but Engels maintained the idea of a universal key even in his speech at the grave of Marx, comparing Marx's findings with those of Darwin.

b) Marx himself contributed to the misunderstandings by his *Forward* [sic] *to the Critique of Political Economy,* "perhaps the best known, and certainly the most unfortunate, statement of what Marxism is [which] even a sophisticated scholar like C. Wright Mills [put] first in his anthology of Marxist writings." The *Forward,* of course, set forth the famous statement that "the mode of production determines the social, political and spiritual life processes in general." In this form, it is the foundation of a view of society as consisting of a substructure and a superstructure. It is "the very essence of vulgar Marxism; it is also the ideological foundation of Stalinism," writes Harrington (p. 37).

Then "why did Marx do such a disservice to his thought?" This was the period when Marx had been wrestling with the problem of finding the key to the capitalist system; in the preceding two years he had written a thousand pages of notes and commentaries (recently published as the *Grundrisse* [Marx 1973]. "One explanation, then, is that the *Forward*

is the kind of oversimplification even a genius might write when confronted with the problem of summarizing extremely complicated material." In this context, concludes Harrington (faith moves mountains), "The 1859 statement would be subsumed under the famous rule, 'Even Homer nods' " (p. 41). But then why publish the *Forward,* when he had not published the *Economic-Philosophical Manuscripts, The German Ideology,* and the *Grundrisse,* more than 2,000 pages, before?

c) But the real culprit is Friedrich Engels, "the lifelong friend and colleague of Marx, who shared in his intellectual development." He is "the second great figure in the Marxist misunderstanding of Marxism." Marx "was unjust to his ideas in a few passages; Engels did much more consistent harm to his mentor's theory although he sometimes was its shrewdest interpreter. He was the inventor of an omniscient theory of society and nature, called *dialectical materialism,* which is not to be found, even as a momentary indiscretion, in the writings of Marx" (p. 42).

Engels's presentation of Marx's views—the first comprehensive codification to be published—appeared in *Anti-Dühring* in 1876–78. But now the mystery deepens. Not only did Marx read the entire manuscript; he also contributed to it: the tenth chapter of part 3, entitled "From the Critical History," was written entirely by Marx. And Engels noted specifically, "The mode of outlook expounded in this book was founded and developed in far greater measure by Marx, and only in an insignificant degree by myself."

But if this book was a travesty—and the quixotic fact is that the entire first generation of Marxist writers, Plekhanov, Lenin, Bernstein, and Kautsky, were instructed by it; and the extract taken from it and published as a pamphlet, "Socialism, Utopian and Scientific," was circulated as widely as the *Communist Manifesto* and became the textbook for all Marxist schools—why did Marx allow it?

Anti-Dühring began as a polemic against a rival of Marx, a popular academic figure, Eugen Dühring, so Harrington concludes that "the result, if I am right, was that Marx tolerated a kind of intellectual double standard, allowing his factional partner the rhetorical luxury of imprecisions and sweeping generalities, which he himself would never tolerate in his own scientific work." Besides, "Marx allowed Engels to exaggerate because he felt that was necessary in a factional struggle which involved many uneducated people" (p. 42).

How remarkable! What is one to say of Marx's preface to *Capital,* of the "laws of motion," the "natural laws of capitalist production," of "these laws themselves, of these tendencies working with iron necessity towards inevitable results" (Marx 1906, p. 13)? Or of Marx's characterization of Kant in *The German Ideology* as the "whitewashing spokesman" of the German burghers (Marx [1867, 1885–94] 1965, p. 209), or any dozen other sweeping, imprecise, and "factional" statements in *his* scientific work?

Actually, in his crude attempt to "whitewash" Marx, Harrington is unfair to Marx and to the genuine intellectual questions he wrestled with all his life, which led him often, even if understandably, to vulgar statements as well as to different and more complex formulations. Like all of us to this day, Marx was seeking to resolve a number of inherently irreconcilable dilemmas in the epistemology and sociology of the social sciences. Schematically, the contradictions are

1. an activity theory of knowledge versus a copy theory;
2. voluntarism, according to which men make their own history, versus structural constraints or mechanistic determinism;
3. human nature seen as an essence (*wesen*) versus human nature seen as recreated by history;
4. class role and persona of persons as against diverse individual motivations, and the mechanisms that mediate between the two concepts;
5. the "logic of history" versus moral condemnation of inhumanities;
6. scientific inquiry as either theoretical or historical, for it cannot be both simultaneously; thus one has either a logical explanation through a conceptual prism or an empirical explanation seeking to identify actual sequences;
7. a general theory of "society" and its determining mode (or even "functional requisites") versus a historicist theory of specific, qualitatively different social formations.

Clearly I do not have the space to elaborate upon these, but in reading Marx (not just Engels) one can find him, at one time or another, espousing (at different times) *both* sides of nearly all the polar opposites listed above, and one cannot explain that by using the word "dialectical" since that word explains everything. An activity theory of knowledge, which we find in the *Theses on Feuerbach,* sees man as an active agent in history, but this view risks accusations of idealism, as Lukács found out when he was forced in Moscow to recant the *History and Class Consciousness*. A copy theory of knowledge, which we find in *Anti-Dühring* (and later in Lenin's *Materialism and Empirio-Criticism*), is more positivist and scientistic, but to introduce a theory of change, one has to posit the absurd argument that matter moves dialectically. Given his early Hegelianism, why should Marx have endorsed *Anti-Dühring?* In the *Economic-Philosophical Manuscripts,* Marx talks of man as having an essence. But in *The German Ideology,* he defines man by his history. Yet if, as Marx states in *Capital,* in achieving new powers man changes his nature, then human nature in ancient Greece must have been significantly different from human nature under modern capitalism, in which man's powers are so much greater. And if this is so, how is it possible, as Sidney Hook asked long ago in his article on materialism in the original *Encyclopedia of Social Sciences,* to understand past historical experience in the same way we understand our own, since understanding presupposes some invariant categories? Marx scorned the idea of "timeless truths" (see the vicious discussions of Stirner in *The German Ideology*); yet if we accept, with Kojeve, Marcuse, and

Lukács, the "logic of history," where is the right to pronounce absolute moral judgments, as on Stalin?

On almost all these issues, Marx was "inconsistent," and it is this inconsistency which allows so many individuals to construct their "own" Marx. Moreover, Marx "finished" only one major scientific work in his lifetime, volume 1 of *Capital*. The works before 1848 were slashing, vitriolic attacks on Bauer, Stirner (occupying 374 of 632 pages of *The German Ideology*), Proudhon, Ruge, et al. Of *Capital*, volume 1 appeared in a German edition in 1867, but Marx was still unhappy with the work; when a French edition appeared in several parts from 1872 to 1875, it bore the note "entirely revised by the author." As late as 1881, two years before his death, Marx told Kautsky that little of the remaining work was ready for publication, because it lacked internal cohesion; the task of sorting and arranging the order of the remaining inchoate manuscripts fell to Engels (for *Capital*) and Kautsky (for the *Theories of Surplus Value*).

The point is that on no single theme associated with Marx's name —historical materialism, class, the crises of capitalism—is there a single unambiguous definition of a concept. Marx never used the phrase "historical materialism" (it was coined by Engels; Engels never used the phrase "dialectical materialism," which was invented by Plekhanov); and the famous statement that it is not the consciousness of men that determines their existence but their existence that determines their consciousness is vague, mechanistic, and even contradictory. "Class" is defined variously: in relation to property (the proletariat being defined as the propertyless in the *Critique of Hegel's Philosophy of Right* and in *The German Ideology*); in terms of political consciousness (the *an sich* passages in the *Poverty of Philosophy*); in terms of political interests (in *The 18th Brumaire*); according to positions in the mode of production (*The Communist Manifesto* and *Capital*); and in relation to the source of income, in the incomplete fragment which ends volume 3 of *Capital* (and which Dahrendorf sought to complete by piecing together other sections in his *Class and Class Conflict in Industrial Society*). There are three different theories of crises of capitalism: an underconsumption theory; a theory of disproportions between the growth of producer-goods and consumer-goods sectors; and a theory of the tendency of the falling rate of profit, as a result of the change in the organic composition of capital. It is no accident that, as Charles Frankel has remarked, it is not Marxism that creates radicalism; each new radical generation creates its own Marx.

Harrington wants to correct the "vulgar Marxists" who see society in terms of a substructure and superstructure and see the politics and culture of a society as always "determined" by the economic elements or even the mode of production itself. Society is an "organic whole," "in which the economic, political and social interact reciprocally upon one another," but this "leaves room for relative autonomies. Art, science and

politics all have their own rhythms," though "production predominates within the organic whole"; and the "idea of a reciprocally interacting causation, which is so central to the Marxist method," is "thus pertinent to computerized sociology as well as to Hegelian philosophy." This is the "first step toward methodology that can help in the understanding of the late twentieth century. In short, the new Karl Marx, announced in the first chapter and contradicted by the familiar Karl Marx in the second, now begins to emerge in his own right." Thus, "When one conceptualizes society as an organic whole in which the economic, the political, the sociological and the cultural so interpenetrate one another they cannot be explained in and of themselves, then there is no room for a completely independent discipline of economics or political science or sociology or aesthetics."

One rubs one's eyes in astonishment. This is like saying that, if one sees "nature whole," there is no possibility of independent disciplines such as physics, chemistry, geology, astronomy, or the like. But the real confusion is compounded because Harrington nowhere defines what he means by "society" or what are its boundaries in space and time. If one talks, as Harrington does, of "capitalist society," are prewar and postwar Japan; the Weimar, Nazi, and Federal Republics of Germany; and the United States all part of an "organic whole"? One can say that a "socio-economic formation" such as capitalism has a coherent conceptual consistency, but if the political and cultural are "relatively autonomous" (as Harrington also says), what is the "organic whole"?

Harrington is confusing a "system" with a "society." Any system has mutually interacting elements, and capitalism as a socioeconomic system (e.g., commodity production) is an aspect of these different societies; but the political systems are largely at variance because they do not derive from the socioeconomic. And the different components such as the technological and the cultural have completely different historical rhythms; so again, what is "organic"?

One can say that the idea of an "organic whole" is a conceptual, not a historical or empirical, construct. But if it is conceptual, does it exhaust the totality of social reality? The ideas of the "mode of production" and of "socio-economic formations" are very powerful constructs. But so are Hegel's "moments" of cultural consciousness or Weber's "modes of domination," and if one uses these different conceptual prisms, there is no exact overlay that makes them coterminous within historical time.

The central dilemma for Marx was that he thought the "mode of production" (a conceptual abstraction) *constitutive* of society, as Darwin's theory of evolution was constitutive of biological development or Newton's laws of motion constitutive of the universe. Harrington writes that for Marx "economics is, by its very definition, a bourgeois discipline." This is not so. For Marx—and this was the rock of his belief—economics was the *material embodiment* of philosophy, which is why he could stand

Hegel on his feet. The "realization of philosophy"—the overcoming of the ontological dualities of subject and object, spirit and matter, etc.—was naturalized by Marx into the overcoming of the social dualities—the division of labor into mental and physical, town and country. That is why communism was for Marx the "realization of economics," meaning its abolition, by the overcoming of *necessity* (i.e., scarcity) and the entry into the "kingdom of freedom." Marx, like Hegel, did believe in a "logic of history" (and the *Begriff* became the "modes of production"), and this remains the permanent utopianism in Marxism.

Harrington's second effort to provide a "new Marx" is to rehabilitate the "law of value" against its economic despisers such as Paul Samuelson. But if the first effort is highly focused, the second has no focus at all. It is quite evident that Marx's idea of value is independent of price, because he sought a system of constants in which, to use the technical jargon, microeconomics (the individual decisions of buyers and sellers) could be aggregated into a macroeconomic, or system, model. Harrington seems to be completely unaware of that problem. His discussion of the law of value repeats the motif of soapbox oratory that, when a worker works an eight-hour day, some hours are "gratis" or surplus value; his central point is that, since the system is unplanned, there is bound to be a cycle of boom and bust. (How planners would know what the people want, without markets, remains undiscussed.)

Harrington spends much time on the so-called transformation problem—how values become converted into market prices—yet seems totally unaware of the question of aggregation. And the crux of that issue is whether capitalism necessarily has to break down. About the one theory of Marx that does lead to the idea of breakdown—that of the tendency of the rate of profit to fall—Harrington agrees with Samuelson that it is not central to Marx. (Parenthetically, an entire new school of young Marxist economists, English and American, argue that it *is* central; and one of them, David Yaffe—whose work, according to Andrew Gamble and Paul Walton, is "the most authoritative reading of Marx at present available"—argues that "abandoning the organic composition of capital argument is to reject Marx's whole value analysis, which leaves Marxism reduced to Ricardian economics plus crude facile empiricism" [Gamble and Walton 1976, p. 142].)[1]

[1] This quotation is a characterization of Yaffe's argument by Gamble and Walton, not directly from Yaffe himself. Earlier two other Marxian economists, Andrew Glyn and Bob Sutcliffe, had written a book (1972) which argued that the decline in British capitalism is due to the falling rate of profit. But they were disputed in part by Gamble and Walton, who, agreeing with their conclusion, claimed that it had been derived from an inadequate premise, namely, a simplified Ricardian model of value. Yet Braun (1976) claims "that a certain confusion reigns between the meaning of the word 'value,' as used by Marx in different parts of *Capital,* and as used by Ricardo in his *Principles.*" He claims that the definition of the word "value" is "not at all important in the general theory of Marx about the capitalist mode of production" or in a theory of prices of production where "Marx tries incorrectly to derive prices of production from val-

On the other hand, Michio Morishima, one of the most respected economic theorists in the field, shows that Marx did solve the "transformation problem" but dealt inadequately with the aggregation question because his algebra and mathematics were inadequate. Building on Marx, however, Morishima grafts the labor theory of value onto a von Neumann general equilibrium model in order to construct an aggregation or macroeconomic model. This model could then be used as the basis for a new growth theory that can accommodate substitution and choice of alternative techniques, which had been stumbling blocks in Marxist theory (Morishima 1973, esp. introduction and chap. 14).

I cannot mediate the argument. The point I want to make is that Harrington's exposition is a cheat. It pretends to discuss the "law of value" but ignores the entire technical literature on the problem, from its comprehensive exposition in Paul Sweezey's *Theory of Capitalist Development* (reformulating Bortkewicz) down to the profuse literature of the present day.

The same cheat is repeated on a more elaborate scale in part 2 of the book. In a chapter entitled "Introduction to a Secret History," Harrington claims, "It is the argument of Part II of this book that it was and is the structure of capitalist society that turned the historical accidents of the 1970s into calamitous necessities." But nowhere does Harrington employ, in coherent or more than offhand remarks, any of the Marxian tools or any of the specific theories of crises in order to explain the situation of the 1970s; he merely states repeatedly that the unplanned nature of capitalism leads to crises. Most of the chapters are taken up with polemics seeking to show that the United States government has intervened more directly to help corporations than other social groups, that inequality has not been substantially reduced, that the neo-conservatives are wrong in their judgments about the welfare state, and so on. All of these points are debatable, but I do not want to be deflected from the central question, which is, What specifically does this "new Marx" tell us about contemporary society that is genuinely new? The answer is nothing.

In the one effort to deal theoretically with the question of the 1970s, Harrington relies briefly on James O'Connor's *Fiscal Crisis of the State*, whose argument is not at all congruent with the "law of value." The heart of the "law of value" argument is that *competition* between capitalists would lead to the elimination of the inefficient, that the increasing substitution of machinery for labor would lead to more intense exploitation to overcome the shrinking base of labor, and that such competition was the

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ues"; but it is "important in any model of accumulation, and in this context, Marx uses the concept adequately" (pp. 116–17). If "authentic" Marxists fall out so among themselves, what are we "inauthentic" Marxists to say? Curiously Harrington, who in other respects is so ravenous about recent Marxian literature, ignores this easily available English debate and devotes himself to some recondite German arguments which deal metaphysically with the "law of Value."

motor of destruction. O'Connor's argument, however, is that the capitalist state faces the contradictory problems of *accumulation* and *legitimization*, of providing for capital expansion yet meeting social demands. And he is right. But this is true for all societies committed to growth, because —as the present Polish government sees very well—they have to balance the need to increase capital against workers' demands for more food and social services.

Curiously, Harrington misses a neat opportunity to apply Marx's idea of competition as the source of destruction (which is nicely stated in *The Poverty of Philosophy*), as he could have done if he had taken the international economy as his canvas. Thus the strength of the Japanese and German capitalist economies is, in one sense, at the "expense of" the British economy; but this is then not "the twilight of capitalism" but the twilight of *some* capitalist societies—a point which proves Veblen right more than Marx.

If one seeks for some root source of the contemporary economic crisis, it is the fact that in the modern world demand rules the society, as against the traditional societies, where supply ruled. Within the international economy, we have seen in the past 20 years the gathering swell of an international demand which, by its synchronization through interdependence, led to a worldwide inflation. And within societies, the demand for services and entitlements has led to the expansion of the public sector —here the neo-neo-Marxist James O'Connor and the vulgar Marxist Milton Friedman (who believes that economics determines other realms of society) are in agreement—and again to a persistent inflationary pressure.

In a very different sense from what Harrington understands, Marx was right about the present. For what Marx said was that, when the "political revolution" was won, the "social revolution" would follow. The political revolution—the heart of 19th-century struggles—was the effort to gain the political franchise and similar rights. (In most European countries workers did not obtain the right to vote until the period between 1890 and 1910.) What we see now—and what has existed for the past 40 years in the United States and somewhat longer in western Europe—is the effort to extend social claims in all dimensions. This is the fruit of democracy and therefore one of the sources of crises—what Schumpeter called the "fiscal sociology" of capitalism. The one area where few such tensions exist (openly at least) is the Communist world, where the workers are suppressed.

I have said Harrington's book is a cheat. That is a serious charge. Yet it derives from his method. On a theoretical level, it derives from the most serious violation of Marx's own method, which is to treat ideas historically. In constructing his "authentic Marx," Harrington makes a pastiche in which passages from the *Economic-Philosophical Manuscripts* are joined with passages from *Capital*, etc. This is a lawyer's brief or a theological

mode, but not true to the way a man's ideas develop. He compresses passages in order to make Marx seem more foresighted than he was. For example, on pages 128–29 he quotes from Marx's *Grundrisse* (without indicating whose translation he is using) a section that shows how Marx anticipated the application of science to production. But if one compares Harrington's literary rendering with Martin Nicolaus's literal translation (Marx 1973, pp. 704–5) one can see how much more clumsy and inexact is Marx's own formulation. What is more, Harrington is quoting Marx in order to argue that I, in my book *The Coming of Post-industrial Society,* "failed to understand that Marx had anticipated [Bell's] own point on the growing importance of productivity in the domain of capitalist labor"; yet after his compressed quotation, Harrington fails to point out that, four pages further on, Marx argues that such productivity is impossible for capitalist labor: ". . . Real wealth is the developed productive power of all individuals. The measure of wealth is then not any longer, in any way, labour time, but rather disposable time. . . . *The most developed machinery thus forces the worker to work longer than the savage does, or than he himself with the simplest, crudest tools*" (Marx 1973, pp. 708–9; italics in the original).

I must add one more personal point. Harrington writes (p. 162) that Erich Fromm has charged me with a misquotation of Marx. This is so. But it reflects more on Fromm than on myself. Fromm was analyzing an essay of mine, "The Meaning of Alienation," which he had read in an Indian journal named *Thought*. Why he quoted from that esoteric source rather than the original place of publication, the *Journal of Philosophy* (November 1959), I do not know. What did not seem to occur to Fromm is that Indian typesetters often think they know the English language better than those whose native language is English; where I had written "persona," it appeared in the Indian journal as "person." That was the basis of the charge. In reviewing Fromm, Richard Bernstein was struck by the fact that in the context the statement made no sense. He wrote me about it, and I thus discovered that Fromm had misquoted *me* and therefore charged me with misquoting Marx. But Harrington never seems to have been struck by the obvious incongruity and repeats the charge.[2] Old factional habits never change.

The notion of an "authentic Marx" is inherently absurd. No protean

[2] Harrington makes some other statements that are of equally grave import. In *The Coming of Post-industrial Society,* I stated that Marx's *Capital* could be looked at as two different schema. One, a logical abstraction, which is in vol. 1, eliminated the *dritte personen* (the complicating elements such as farmers, shopkeepers, lawyers, etc.) to provide a "pure" theory of capitalism. In vol. 3, there was an empirical model that provided some brilliant statements about the actual transformation of capitalist society, in particular the separation of ownership and management, which modified the schema in vol. 1. I said that I found Schema *2* more fruitful than Schema 1. Harrington points out, as Engels did in his prefaces to *Capital,* that most of the materials had been written at the same time and that the task of sorting out the order had fallen to Engels

thinker can ever be given a single, unambiguous reading. We have seen arguments about whether there is one John Stuart Mill or two and whether Keynes belonged to Cambridge, England, or Cambridge, Massachusetts; and I have at hand an article from the *British Journal of Sociology* entitled "Émile Durkheim: Was He a Nominalist or a Realist?" At one point, Harrington says smugly, "All that serious Marxism demands of you is your lifetime." I have devoted half my life to the study of Marx, and that may be insufficient. But Eugene Kamenka, the Australian political philosopher, has devoted his entire life to the study of Marx. In a recent issue of the *Times Literary Supplement*, reviewing a book (*On Materialism*, by Sebastian Timpanaro) that seeks to "rehabilitate" Engels as a "true" Marxist, against his cultured despisers, Kamenka wrote, "The past history, present character and likely future development of Marxism show Marxism to be as complex and as much subject to historical change and tension as Christianity. . . . The only serious way to analyze Marxist or socialist thinking may well be to give up the notion that there is a coherent doctrine called Marxism or socialism, that there is any such thing as *the* Marxist or socialist idea, or even *the* Marxist or socialist view of the world" (Kamenka 1976, p. 1442).

Otherwise, one is left with the situation of Harold Laski, who said, bitingly, in replying to a critic, "You can interpret Marx in your way, and I will interpret him in *His*."

(which makes Harrington's remark [p. 111] that Marx had written the "fourth and last volume of *Das Kapital* first and then worked backward to the beginning" quite silly, considering, too, Marx's remark to Kautsky in 1881 that he still felt the work lacked coherence). But the crucial point is that when Engels edited the volumes he made many interpolations, and we do not actually know what he did add. Harrington writes, "Then in the process of editing Volume III, which appeared in 1894, Engels made even more sweeping revisions of Marx's earlier assessments." And he goes on for two pages to indicate that these were the additions which made vol. 3 so much more relevant to the actual institutional changes in capitalism: cartels, the stock market, the corporation as an international instrument, etc. Since these were the elements I had included in my second schema, what then is the meaning of the appendix charging me with misreading Marx, when in his text Harrington makes the exact same point? Or the meaning of the offhand earlier assertion (p. 380), "A carelessness on the same count is also at work in Daniel Bell's confused statement of the Marxist view of social class [to] be taken up in Note 4 of this Appendix"—since note 4 deals with the two schema, what has it to do with social class? And since when is there "the" Marxist view of social class? In this instance, as in many other sweeping accusations against Samuelson, Aron, and others, Harrington is unfortunately imitating the habits of Marx, who rarely played fair with an opponent. If Harrington is interested in the sources of this "repetition compulsion," he should read Leopold Schwarzschild's brilliant book, *The Red Prussian*, the only book which goes into detail on Marx's vitriolic invective against opponents ("perfidious boor," "toads," "the emigrant scum," and the disgusting anti-Semitic characterizations of Lassalle); as well as two works by Marx that almost no Marxists have ever read, *The Knight of the Noble Conscience*, a vile attack on his factional opponent August Willich, and *The Great Men of Exile*, an attack on the Germans who emigrated to America which was not printed because his Hungarian publisher embezzled the publication funds. If a complete *oeuvre* of Marx is ever to appear, I commend these books to those who seek the "authentic Marx."

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Review Essay: Habermas's Social and Political Theory

Toward a Rational Society: Student Protest, Science, and Politics. By Jürgen Habermas. Translated by Jeremy J. Shapiro. London: Heinemann Educational Books, 1971. Pp. x+132. £1.50 (cloth); £0.75 (paper). Boston: Beacon Press, 1971. Pp. ix+132. $2.95 (paper).

Knowledge and Human Interests. By Jürgen Habermas. Translated by Jeremy J. Shapiro. London: Heinemann Educational Books, 1972. Pp. viii+356. £3.50. Boston: Beacon Press, 1971. Pp. viii+356. $7.50 (cloth); $3.95 (paper).

Theory and Practice. By Jürgen Habermas. Translated by John Viertel. London: Heinemann Educational Books, 1974. Pp. ix+310. £4.00 (cloth); £1.80 (paper). Boston: Beacon Press, 1973. Pp. ix+310. $10.00 (cloth); $4.95 (paper, 1974).

Legitimation Crisis. By Jürgen Habermas. Translated by Thomas McCarthy. Boston: Beacon Press, 1975. Pp. xxiv+166. $8.95 (cloth); $3.95 (paper).

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Jürgen Habermas is the most distinguished, and by that token also the most controversial, social theorist and political philosopher writing in German today. In the English-speaking world, to adopt a well-worn phrase, Habermas's works are well known, but they are not yet known well. In some part this is because of vagaries of translation. Four of Habermas's major writings have been translated into English under the titles of *Toward a Rational Society,*[1] *Knowledge and Human Interests,*[2] *Theory and Practice,*[3] and *Legitimation Crisis.* These, however, represent only part of a vast output, and they have not been published in a chronology which conforms directly to the development of Habermas's ideas. The original version of *Theorie und Praxis,* for example, was published in 1962, some years before *Erkenntnis und Interesse* (*Knowledge and Human Interests*), but these have appeared in reverse order in English. A more important

[1] A collection of essays taken from *Protestbewegung und Hochschulreform* and *Technik und Wissenschaft als "Ideologie."*

[2] Includes as an appendix Habermas's inaugural lecture at Frankfurt, originally in *Technik und Wissenschaft als "Ideologie."*

[3] This abridgment of the fourth German edition of *Theorie und Praxis* includes the essay "Arbeit und Interaktion" from *Technik und Wissenschaft als "Ideologie."*

reason for the relative lack of impact that Habermas's work has had among English-speaking social scientists is that he writes from the context of unfamiliar intellectual traditions: those of Frankfurt critical theory, hermeneutics, and Hegelian philosophy, as well as Marxism. To attempt a mix of all these sounds formidable enough, but Habermas's compass in fact extends much more widely. He is very familiar with the dominant trends in Anglo-Saxon social theory and philosophy and is considerably indebted to the latter in two particular respects. In the earlier part of his career, in conjunction with K-O. Apel, (see Apel 1971) he drew extensively upon the writings of Peirce; in his current work he makes a good deal of use of the theory of speech-acts, as developed by Austin, Searle, and others. The extraordinary range of Habermas's writings defies easy analysis. In this review essay I shall concentrate my attention upon the works available in English but shall attempt to connect them to the overall development of Harbermas's thought.

Two leading, and massive, themes recur throughout Habermas's writings and give his works their continuity. One is a concern with metatheoretical problems in social theory, especially in respect of the relation between theory and critique. The other is the objective of placing such a critique in the context of an interpretation of the main trends of development in Western capitalism since the rise of bourgeois society in 18th-century Europe. These were of course also the classic preoccupations of the "older generation" of Frankfurt social philosophers, and Habermas's work preserves certain of their emphases. His reading of Marx, like theirs, strongly accentuates the Hegelian legacy, and he is highly critical of the more orthodox forms of Marxism; he tries to complement ideas drawn from Marx with others drawn from Freud; he accepts the theorem that "organized capitalism" differs so profoundly from 19th-century competitive capitalism that some of Marx's central theories have to be radically reworked if they are to retain any relevance today; and he continues their concern with revealing the origins of the dominance of technical rationality in modern culture and politics. But these shared emphases at the same time serve to distinguish his views from the various positions taken by Horkheimer, Adorno, and Marcuse, because the modes in which he develops this inheritance differ substantially from those of his legators.

A useful way of achieving an entrée into Habermas's metatheoretical writings is via his relation to the hermeneutic tradition or the *Geisteswissenschaften*.[4] Some of the ideas central to this tradition, especially the notion of *verstehen*, have become well known to English-speaking social scientists—principally through the methodological writings of Max Weber, who drew extensively although critically from this intellectual source. In Weber's writings, as in those of the early Dilthey, *verstehen* is linked to the reconstruction of the subjective experience of others, the grasping of the "subjective meaning" of action, as Weber put it. Expressed in this way, the notion of *versteken* gives rise to numerous objections, espe-

[4] For a further discussion, see Giddens (1977).

cially since both Dilthey and Weber wished to claim that the method of interpretative understanding is compatible with the achievement of objectively verifiable data.[5] The "psychological" version of *verstehen* has been effectively criticized and transcended by Gadamer in his *Truth and Method* (1975).[6] Drawing extensively upon Heidegger, Gadamer develops a notion of *verstehen* in which understanding is seen not as a "method" which the historian or social scientist uses to approach his distinctive subject matter, but as the characteristic property of human intersubjectivity as such—and as expressed above all in language. Language is the medium of social being and of self-understanding. *Verstehen* depends upon common membership in a cultural frame of meaning or what Gadamer calls a "tradition." The understanding of distant historical periods, or alien cultures, can be treated as involving the establishing of dialogue between discrepant traditions. Hermeneutics, Gadamer concludes, is the universal principle of philosophy, since understanding is the condition of all knowledge. Habermas accepts some of Gadamer's main ideas but sees them as partial, rejecting the latter's claim of the "universality of hermeneutics." Habermas opens his systematic discussion of logical issues in the social sciences, *Zur Logik der Sozialwissenschaften* (1967), with the suggestion that the major problem which has to be tackled is that of connecting two disparate philosophies which have largely developed in separation from one another: hermeneutics on the one hand, and empiricist philosophies of science on the other. Certain elements of each have to be accepted, but in order to supersede both.

The arguments underlying this view are fully developed and clarified in *Knowledge and Human Interests,* which culminates the first phase of Habermas's career and remains perhaps the most hotly debated of his works. *Knowledge and Human Interests* covers a sweep of intellectual history from the late 18th century to the early 19th. In the beginning sections of the book, Habermas discusses Hegel's critique of Kant's philosophy of knowledge and Marx's critique of Hegel, proceeding thence to discuss hermeneutics and positivism as two contrasting types of philosophy, exemplified by Comte and Mach, and by Dilthey, respectively. The line of development from Kant to Hegel to Marx forms a key period in the elaboration of epistemology or the "theory of knowledge." Hegel's writings mark a high point in the development of epistemology insofar as they make central the self-reflection of the knowing subject. Marx was correct, Habermas proposes, in holding that Hegel's philosophy was inherently limited by its idealism; but in attempting to formulate a materi-

[5] This has been fastened upon by critics influenced by logical empiricism and behaviorism. How, they ask, can we reliably grasp the content of the subjectivity of others? If *versteken* is of any use at all in the social sciences, they conclude, it is as a source of intuitively plausible hypotheses, which then have to be tested against observations of observable behavior. (See, e.g., Abel [1948]; Nagel [1953]. A similar view is expressed in the writings of Carnap and Hempel.)

[6] Originally published in German in 1961 as *Wahrheit und Methode.*

alist critique of Hegel, Marx himself slips into a basic error. What Marx does is to tend to assimilate two strands of Hegel's philosophy in his own conception of historical materialism: the self-reflection of the subject and the material transformation of the world through labor. The thesis that history is the expression of the expanding consciousness of human beings of the circumstances of their action is thus merged with and reduced to the theme that human beings transform the material world and themselves through labor. Habermas also sets out this argument at various points in *Theory and Practice,* especially in the context of a lengthy discussion of Marxism in the chapter entitled "Between Philosophy and Science: Marxism as Critique." To the degree to which Marx collapses the idea of the self-formation of the human species through reflection into that of its self-formation through productive activity, Marxism merges with the rising tide of positivistic philosophy which dominates the latter part of the 19th century. Positivism is a philosophy which eclipses the philosopher, that is, the subject reflexively investigating the grounds of his claims to knowledge. Epistemology, as Habermas puts it, is replaced by the philosophy of science, based on "the conviction that we can no longer understand science as *one* form of possible knowledge, but must rather identify knowledge with science" (*Knowledge and Human Interests,* p. 4; all citations are to British editions except where indicated).

The tradition of the *Geisteswissenschaften,* Habermas goes on to say, offers a contrasting viewpoint to positivism, but dissociates itself from the latter rather than developing a direct critique of it. Hermeneutics becomes concerned with the primacy of meaningful understanding, but only in spheres of activity disconnected from science. Hermeneutics and positivism are thus both partial philosophies, flawed by their incomplete character. A viewpoint that attempts to transcend both, which it is Habermas's aim to set out, has to recognize that they are (unknowingly) directed to different types of "knowledge-constitutive interest": different logical forms which the disclosure of reality can take, whether this be natural or social reality. Hermeneutics is oriented toward an interest in understanding, at the level of ordinary language communication, in the context of the practicalities of day-to-day social life. Positivism, on the other hand, implicitly assumes an orientation to an interest in prediction and control, in the production of instrumental or "technically exploitable" knowledge—mistakenly claiming that all knowledge which is to *count* as knowledge is of this type. The knowledge-constitutive interests in understanding and in technical control are not, for Habermas, transcendental categories of a quasi-Kantian type; rather, they are presupposed as aspects of the human self-formative process. They thus connect to two major concepts that Habermas introduces, which occupy a central place in his thought: "interaction" and "labor" (*Arbeit*). Labor refers to man's instrumental relation to the world around him. Labor or instrumental action is based upon empirical knowledge, yielding predictions that can

be tested in accordance with technical rules (*Toward a Rational Society*, pp. 91–92). It is the object of what Habermas calls the "empirical-analytical sciences" to develop knowledge which can be used instrumentally, to realize an interest in the prediction and control of events. The empirical-analytical sciences include not only the natural sciences but also the generalizing social sciences, such as sociology. (Habermas also sometimes refers to these as the "nomological sciences.") Interaction refers to the saturated context of everyday life, involving ordinary language communication and governed by social norms. This is the concern of the "historical-hermeneutic sciences." Hermeneutic inquiry, in Habermas's words, "discloses reality subject to a constitutive interest in the preservation and expansion of the intersubjectivity of possible action-orienting mutual understanding" (*Knowledge and Human Interests*, p. 310).

In *Knowledge and Human Interests*, Habermas identifies Dilthey and Peirce as the two figures who came closest to uncovering the roots of knowledge in interest, in regard of hermeneutics and natural science, respectively. But, as a result of the decay of epistemology in the 19th century, neither was able to connect his insights to a conception of the self-formative process of human history, such as had earlier been attempted by Hegel. In the context of the spreading influence of positivism, a return to this type of conception would have appeared as a regression to metaphysics. But such a reappropriation is just what is necessary for us to be able to grasp the significance of knowledge-constitutive interests: that is to say, a reappropriation of the notion of the reflexive capability of human knowledge. What has become lost to philosophy since Hegel is "the emancipatory power of reflection, which the subject experiences in itself to the extent that it becomes transparent to itself in the history of its genesis" (*Knowledge and Human Interests*, p. 197). Recognition of an interest in reflexively comprehending the conditions of our own action is thus shown to be the condition of acknowledging the interest-bound character of knowledge in general. Self-reflection is connected to the interest in achieving autonomy of action through self-understanding, and hence is an emancipatory interest. The emancipatory interest provides the guiding inspiration for critical philosophy, or critical theory, which has as its aim the liberation of human beings from their domination by forces constraining their rational autonomy of action. In Habermas's metatheoretical scheme, we thus have three elements of the self-formative process (labor, interaction, domination), connected to three knowledge-constitutive interests (technical control, understanding, emancipation), relating in turn to three types of disciplines (the empirical-analytic sciences, historical-hermeneutic sciences, and critical theory).

At the turn of the 20th century, there emerged a program of research which mobilizes each of the knowledge-constitutive interests and which, therefore, in Habermas's view provides something of a model for critical theory: psychoanalysis. Freud himself regarded psychoanalysis in a posi-

tivistic way, as a form of natural science. Looked at in terms of the theory of knowledge, however, psychoanalysis can be seen to incorporate an emancipatory aim within a framework which relates the hermeneutic and the nomological (for relevant discussions see Lorenzer [1974]; Ricoeur [1970]). Analytic theory is a dialogue between therapist and patient, and in that sense proceeds on the level of ordinary language communication. It is a hermeneutic endeavor in the sense that it investigates the character of such communication as an expression of an unconscious symbol-system. But this "depth hermeneutics" is complemented by an interest in the causal conditions influencing the behavior of the analysand, via mechanisms of repression. Hence we find in Freud a mixture of terms that are interpretative in character with others that borrow the terminology of natural science, such as "force," "energy," etc. These are related to one another through the emancipatory project of analytic therapy: the elimination of distorted communication through the enhanced self-understanding of the analysand, whereby he is able to expand his autonomy of action. The dialogue between analyst and patient simultaneously furthers the progress of emancipation and expresses it, since it makes possible the growth of mutual understanding between the two parties.

Habermas has pursued the theme of distorted communication in a series of writings, only some of which are available in English.[7] He has continued to defend, in the face of some considerable attack, the thesis that psychoanalysis provides an exemplar for critical theory. But a concern with distorted communication obviously presupposes an idea of what a situation of "undistorted communication" might be like—a notion of what Habermas calls an "ideal speech situation." In some part this is already latent in *Knowledge and Human Interests*, and it seems that Habermas has drawn upon Peirce's account of science in formulating the conception of an ideal speech situation. Peirce argued that scientific truth concerns not the relation between an isolated observer and his subject matter, but a consensus arrived at through the discourse of many observers, where that discourse is not constrained by anything other than the canons of logical procedure or rational argumentation. Habermas in effect generalizes this, since Peirce's argument refers only to one aspect (instrumental rationality or the technical interest) of the three dimensions of the human self-formative process. An ideal speech situation, the "baseline" against which distortions in empirical circumstances of social conduct can in principle be determined, involves not only the rational attainment of consensus but also complete mutual understanding by participants and recognition of the authentic right of each to participate in the dialogue as an autonomous and equal partner.

The ideal speech situation is an analytical construct but, Habermas argues, any actual circumstance of communication anticipates it implicitly.

[7] For example, "Toward a Theory of Communicative Competence" (1970); "Some Distinctions in Universal Pragmatics" (1976).

Communication in interaction, he has tried to show in his most recent discussions of "universal pragmatics," raises four types of "validity-claims": those of *Verständlichkeit* (intelligibility), *Wahrheit* (truth), *Richtigkeit* (adequacy or correctness), and *Wahrhaftigkeit* (veracity or "truthfulness"). Consensual interaction can be carried on only to the degree that participants credibly sustain validity claims in each of these respects (which are usually taken for granted but can always be "problematized," either by those involved or by the sociological observer): that what each speaker says is intelligible or meaningful; that the propositional content of what he says is true; that what he says is normatively legitimate; and that he is speaking honestly and without guile. *Verständlichkeit,* Habermas proposes, is the condition of all symbolic communication whatsoever; it has a different status from the other validity-claims, since communication must be intelligible before the others can be problematized at all. *Wahrhaftigkeit* also has to be separated out in a certain sense, because genuineness of intention can only be really demonstrated in how a person actually behaves. The other two are open to discursive justification, yielding two major types or parameters of discourse: "theoretical-empirical discourse," concerned with the sustaining of truth claims, involving appeal to empirical observation, and couched in terms of law-like generalization; and "practical discourse," concerned with justifying normative claims, involving appeal to interpretations of values, and couched in terms of appeal to moral principles ("Some Distinctions in Universal Pragmatics").

In his ongoing writings, Habermas is attempting to relate the mastery of these two forms of discourse to the psychological development of the child (using Piaget and Kohlberg) and to the major stages of human social evolution (Habermas 1976). What will emerge from this work still remains to be seen. It is clear enough, however, that Habermas's current concern with these issues links directly not just to the metatheoretical problems I have sketched so far, but also to more substantive themes which have preoccupied him from the earliest part of his career onwards. In his first major work, *Strukturwandel der Öffentlichkeit* (1962) Habermas undertook a historical study of the rise (and subsequent decline) of discursive politics in bourgeois society from the 18th century to the present day.[8] The book was much debated in Germany and had an important influence upon the student movement, although Habermas himself eventually came to a position of sharp dissension from the views of some of the student leaders.[9]

A "public sphere," in which political life can be discussed openly in accordance with standards of critical reason Habermas tried to show in the

[8] The book has been the subject of much commentary; see, e.g., Jäger (1973).

[9] Two of Habermas's essays on students and politics are included in *Toward a Rational Society:* "Student Protest in the Federal Republic of Germany" and "The Movement in Germany: A Critical Analysis."

study, emerges for the first time in the 18th century. "Public opinion" becomes differentiated from mere "opinion," prejudice, or habit: the former presupposes a reasoning public. The bourgeoisie promoted the development of the public sphere in opposition to the traditionalist and hierocratic forms of authority of feudalism. This development reflects the division between civil society and the state characteristic of the emerging bourgeois order: the formation of rational public opinion mediates between society and the state. The spread of newspapers and journals, which both express and help fashion public opinion, played a major role in this process. The bourgeois or liberal idea of the public sphere was always at some distance from the reality but has been increasingly undermined as a consequence of the social changes which have occurred from the early part of the 19th century to the present day. Today state and society increasingly interpenetrate: the public sphere is squeezed or "refeudalized" by the growth of large-scale organizations coordinated with government, and by the commercialization of the media. The expanding influence of science and more generally the technical rationalization of social life accentuate this process.

The impact of technocratic consciousness in advanced capitalism is discussed by Habermas in several of the essays included in the English edition of *Theory and Practice* and in *Toward a Rational Society*. Since the latter part of the 19th century, he says, two major trends have become marked in Western capitalism: the burgeoning of state interventionism, directed toward stabilizing economic growth, and the increasing mutual dependence of research and technology, as a consequence of which science has become a leading force of production (*Toward a Rational Society*, pp. 50 ff.). These changes mean that the relations between "infrastructure" and "superstructure" which Marx specified in relation to 19th-century competitive capitalism no longer apply. The idea that politics can be treated as part of the superstructure is adequate only so long as polity and civil society are separated, with the latter being "autonomously" regulated through the operations of the market; and this is no more the case. The "political" and the "economic" are no longer easily separable. Hence the old form of legitimation, based upon the ideology of fair exchange, and expressed in its most sophisticated form in classical political economy, becomes increasingly obsolete, and is replaced by new modes of legitimation. There can be no return, Habermas argues, to the type of direct legitimation of power characteristic of the precapitalist order, and the legitimation system of advanced capitalism thus tends to become a technocratic one, based upon the capability of elites to "manage" the economy successfully and sustain economic growth.

Because of the premium placed upon "controlled economic development," science is more and more directly harnessed to the process of technological innovation. This situation, Habermas suggests, undermines the relevance of Marx's theory of surplus value; the labor power of the

immediate producers now plays a relatively small role compared to the value generated by scientific-technical innovation. Critical theory can no longer be limited to the critique of political economy but must extend the critique of ideology to encompass the ramifications of technocratic power. Just as the traditional formulation of the theory of surplus value has to be abandoned, so do other basic formulae of Marxian theory, including that of class conflict. "State-regulated" capitalism emerged in substantial part in response to the massive strains created by direct class antagonism. Class division, based in private property, still remains integral in advanced capitalist society. But it has become the area where the society can *least* admit the occurrence of open confrontation or struggle; hence direct conflicts tend to break out most frequently in those sectors which have less transformative consequence for the system. Conflicts which reflect divisions at the center become manifest at the periphery.[10]

At the core of the technocratic ideology of advanced capitalism, Habermas says, is the collapse of the distinction between the categories of labor and interaction, or the technical and practical: positivism, which also assimilates these categories, reducing the latter to the former, is hence a philosophical expression of technocratic domination. The growth of technical powers of control in contemporary society, he makes clear in *Theory and Practice,* can be analyzed in terms of four levels of rationalization.[11] On the first two of these levels, the growth of technology is associated with the exclusion of normative elements from scientific discourse; on the other two levels, technical procedures become established as values in themselves, the preexisting value elements having been defined as "irrational." The first level of rationalization involves simply the use of nomological knowledge for the realization of independently affirmed values. Rationalization proceeds on the second level when choice is demanded between two or more procedures of equal technical effectiveness. One of the expressions of this is the development of formal "decision theory," which attempts to rationalize the relation between goals and values, rather than being limited to providing technical means of reaching given ends. On each of these levels, values are treated as removed from the possibility of rational discussion, save insofar as some values are in part means to the realization of others. When technical rationality is extended to values themselves, we have moved to the third level of rationalization. The clearest example of this level, Habermas suggests, is game theory, where actors are treated as evaluating the consequences of their actions in terms of a calculus of preferences intrinsic to the postulated form of the game, without reference to any wider value-systems, which then disappear from view. The final, and most embracing, level of rationalization is reached when the strategic decision-making framework, such as is characteristic of game theory, is generalized to cover all types of decision: where "sta-

[10] This idea has been developed in various publications by Claus Offe.

[11] See especially "On Theory and Praxis in Our Scientific Civilization."

bility" or "adaptability" becomes the basis of analysis of decision making within self-programming feedback systems. On this level of rationalization, decision making can in principle be carried out through the computer: and it is this stage of rationalization toward which modern technocratic politics is moving on a large scale. This, in Habermas's words, is a "negative Utopia of technical control over history." It is the most complete form of the reduction of the practical to the technical: hence Habermas's critical attitude toward systems theory, which he sees as the political economy of the contemporary era (Habermas and Luhmann 1971).[12]

In *Legitimation Crisis*, which first appeared in 1973 in German as *Legitimationsprobleme im Spätkapitalismus*, Habermas provides an extensive discussion of these and other issues. The language of systems theory is drawn upon and turned back against itself, and the theme of the displacement of strains from the "center," the class structure, is developed. Although the central conflict has been displaced, advanced capitalism is still riven by contradictions and open to persistent crises. The term "crisis," Habermas argues, itself needs elucidation. The word originally comes from medical usage, and was from there imported into the social sciences. In its medical sense, it refers to the phase of an illness which determines whether or not the healing processes of the organism produce the recovery of the patient. A crisis in the context of illness is not just an "objective" sequence of events; it is something experienced by the patient, having the form of powerlessness dramatically contrasting with his normal capabilities as an autonomous being. To speak of a "crisis" is therefore to give a series of events a normative meaning, counterposing determinism to liberation. This is basic to the viewpoint which Habermas tries to develop in the book. There cannot be an adequate systems-theoretic (or functional) concept of crisis, simply treating "crisis" as equivalent to "threat to the stability of the social system"; the concept of crisis, in the context of critical theory at least, has to be understood as conjoining the "subjective" and "objective," as involving the *experience* of impairment of autonomy of social action in certain definite social conditions. We can grasp the relation between the subjective and objective components of crisis tendencies, Habermas claims, by distinguishing between social integration and system integration. Social integration refers to the "life-world," "in which speaking and acting subjects are socially related," system integration to abstractly conceived social systems, which however are to be seen as rooted in a normative order of symbols. Crises can be regarded as unresolved "steering problems," of which actors in the life-world are often not conscious but which express themselves on that level in normative strain and a diminishing autonomy of action.

The "steering problem" of advanced capitalism, Habermas tries to show, can be illuminated against the background of a general theory of evo-

[12] The debate in this book has ramified into several further volumes by other protagonists.

lution.[13] What Habermas seeks to achieve here is nothing less than a wholesale overhauling of Marx's historical materialism, based on the differentiation of labor and interaction. The evolution of human society proceeds in two separable but connected dimensions: the development of the forces of production (which can be thematized as instrumental knowledge, oriented to technical control), and the development of normative orders (which can be thematized as symbolic norms ordering communication). Each involves knowledge-claims which, as Habermas has indicated previously, are in principle open to discursive redemption. In respect of the first, the theory of evolution aims to reconstruct the formation of scientific and technical knowledge, and in respect of the latter, the opening of normative structures to discourse via the movement through "increased reflexivity of the mode of belief, which can be seen in the sequence: myth as immediately lived system of orientation; teachings; revealed religion; rational religion; ideology" (*Legitimation Crisis* [American ed.], p. 12). The relation between the development of the forces of production and normative structures, Habermas claims, is a problematic one. The changes which occur in one sphere do not necessarily produce corresponding alterations in the other, and this is the source of steering problems. In every type of social formation, what Habermas calls a "principle of social organisation" connects forces of production and normative structures; steering problems lead to crisis effects if the organizational principle of the society in which they occur is unable to accommodate to them.

Habermas distinguishes several major types of society based on different principles of organization. "Primitive social formations" are organized around age and sex roles, coordinated through the kinship system. "Traditional social formations" are organized in terms of political class domination: a bureaucratic state apparatus serves as the coordinating focus of the hegemony of a landowning class. In more modern times the two most important types of society are liberal capitalism and advanced or "organized" capitalism. As regards the former, as he made clear in his earlier writings, Habermas accepts the main lines of Marx's analysis, although he attempts to reconceptualize it in his own terminology. In liberal or competitive capitalism, the organizational principle is centered upon the relation of capital and wage-labor, and involves the duality of state and society. In contrast with the previous type of social formation, in liberal capitalism class relationships are depoliticized and made anonymous: class relations express the asymmetries of economic exchange, and the latter becomes the principal steering medium. This system greatly expands the range of the productive forces and furthers the secularization of belief on the level of normative structures. The formation of the "public sphere" corresponds to the drive of bourgeois ideology toward universal principles defended by reason rather than by tradition and by the "anony-

[13] Discussed in *Legitimation Crisis*, pp. 7 ff. (American ed.), and in *Zur Rekonstruktion des Historischen Materialismus*.

mization" of class rule. "The socially dominant class," Habermas says, "must convince itself that it no longer rules" (*Legitimation Crisis* [American ed.], p. 22). Since the organizational principle of competitive capitalism involves an "unregulated" market, steering problems express themselves as economic crises of a periodic character. Liberal capitalism produces "a crisis-ridden course of economic growth."

The main differences Habermas acknowledges between liberal and advanced capitalism are familiar from his writings prior to *Legitimation Crisis*. The development of strong tendencies toward monopoly or oligopoly within the economy, coupled with the expanding interventionism of the state, coordinated by the spreading rationalization of technical control, transforms the organizational form of capitalism. Advanced capitalism is still capitalism, but the central economic contradictions express themselves in novel and more complex form than previously. While class relations are "repoliticized" in a certain sense, the directly political form of class domination characteristic of precapitalist societies is not thereby restored: because, as Habermas has reiterated throughout his writings, capitalism cannot retreat to the traditional forms of legitimacy based on religion or custom. Advanced capitalism really needs such a mode of direct legitimation but does not itself generate it; rather it corrodes these older forms of legitimacy. In their place it is able to put only the administrative capability to control the economic system: to be able to overcome the economic fluctuations of competitive capitalism. To achieve this, Habermas reaffirms, the center has to be controlled most firmly: overt, politicized class conflict is not the most typical form of social conflict in advanced capitalism.

Advanced capitalism is a crisis-ridden society but one in which, as a result of the partial success in stabilizing economic fluctuations, crisis tendencies express themselves in various guises. Habermas finds four such types of crisis tendency, interwoven with one another. One type is economic crisis, but like class conflicts today, and for the same reasons, economic crises rarely appear in pure form. Economic steering problems have come to be treated largely as problems of rational administration: difficulties in resolving dilemmas of economic growth thus tend to become "rationality crises." A rationality crisis is a hiatus in the administrative competence of the state and its affiliated agencies, an inability to cope. Habermas has already argued that the ability to cope, to coordinate economic growth successfully, is the main element in the technocratic legitimation system of late capitalism. Hence it follows that rationality crises, if prolonged or pronounced, tend to devolve into legitimation crises, the potential of mass withdrawal of support or loyalty. Finally, crises of legitimacy can in turn become "motivational crises": the motivational commitment of the mass of the population to the normative order of advanced capitalism is tenuous anyway, as the old moral values are stripped away. Technocratic legitimation provides little in the way of meaningful moral

commitment, but only the provisional acceptance of a materially successful economic system; the threat of widespread anomie, Habermas says, is endemic in late capitalism.

Since organized capitalism remains a contradictory order, whose principle of organization cannot satisfactorily reconcile the forces of production (more and more dominated by scientific-technological imperatives, but still resting on private property) with the normative structure (technocratic legitimation), its crisis tendencies cannot be overcome without major social transformation. But what chances are there of this occurring, and what form might such a transformation take? We find very little in *Legitimation Crisis* in the way of answers to either of these queries. Habermas mentions the possibility of the emergence of a "postmodern" era at one point, referring to Bell's concept of the postindustrial society, but does not develop the comment. The other relevant remarks which he occasionally makes are cursory and tentative, and the concluding part of the work reverts to a high level of abstraction, raising again the general question of the redemption of validity claims in "practical discourse."

As a consequence, whatever illumination the reader may derive from *Legitimation Crisis,* he is likely to feel dissatisfied with its outcome. The book condenses and connects many of the ideas developed in Habermas's previous writings, but it raises more dilemmas than it claims to resolve. It would perhaps not be too harsh a judgment to say that this is true of all Habermas's major writings. Certainly his work, since its beginnings, has stimulated a continuing chorus of criticism—"critical theory criticised" as one author surveying the response to *Erkenntnis und Interesse* put it (Dallmyr 1972). Habermas has been involved in a series of public controversies—with Albert, in relation to the writings of Popper, and with Gadamer, Luhmann, and others—which have helped shape the direction of development of his thought as well as led him to modify some of his original ideas. I shall not attempt to survey these debates here and will offer only a brief personal assessment of the strengths, and what seem to be some of the main shortcomings, of his work.

As for the former, perhaps it is sufficient to say that no one can approach basic problems in the social sciences today, in respect of either the philosophy of method or the more substantive theory of the development of the advanced societies, without confronting Habermas's writings in a serious and systematic way. Habermas has helped to forge closer contacts between Continental and Anglo-American philosophy and social theory; he has made it clear that traditional versions of the epistemology of the social sciences have to be radically overhauled; he has developed the main threads of a powerful analytical model for interpreting the major processes of development that have transformed, and are transforming, Western culture; and he has elaborated a scheme for grounding critical theory that departs significantly from those formulated by the

earlier generation of Frankfurt philosophers. And yet I would be prepared to say that there is barely one of the major claims in Habermas's writings that I should be willing to accept without reservation.

On the level of metatheory, I think the differentiations which Habermas makes between the nomological, hermeneutic, and critical, and between labor, interaction, and power, which are tied into a whole series of other distinctions, are unsatisfactory. The logical status of the knowledge-constitutive interests, as many critics have pointed out, is obscure. Moreover, the contrasts drawn between the first two terms in each of these triads seem to me to preserve too much of the old *erklären/verstehen* distinction that has dogged the tradition of the *Geisteswissenschaften*, even if Habermas does rework these notions very considerably.[14] This polarity of "explanation" and "interpretation" or "understanding," although not applied by Habermas to separate the natural and the social sciences, has two consequences. On the one hand, it obscures the degree to which knowledge aimed at technical control is interpretative in character: an "explanation" in science often consists in locating an observation within a theoretical system so as to render it "understandable." In spite of his strictures against positivism, Habermas seems to preserve something of the logical empiricist view of scientific theory (as a deductive system of causal laws) in his own scheme. Conversely, "predictability" and "control" are integral features of interaction without being linked to nomology: the "predictability" of human interaction is in some part a contingent accomplishment of lay actors. One result in regard of the distinction between labor and interaction is that the latter comes to be equated, implicitly or otherwise, with symbolic communication or "communicative action," and it is difficult to recover conceptually the material interests (in the usual sense of that term) involved in interaction. Critique then tends to become identified with the critique of ideology; or at least, the abstract provisions of the latter exist at some distance from the practicalities of achieving real social change. Similar difficulties seem to exist in the appeal to psychoanalysis as an exemplar for critical theory. The emancipatory goal of psychoanalysis, the expanded autonomy of the patient, is achieved through a process of self-understanding developed through the analyst-patient dialogue. Here there is a preexisting consensual system, since analysis is entered into voluntarily by both parties; the participants share a mutual interest in the outcome, the betterment of the patient; the process of therapy is organized purely through symbolic communication; the achieving of reflexive understanding is the very medium of the extension of the analysand's autonomy of action; and the "domination" which the patient overcomes as a result of successful therapy is that of his own inner makeup, not the domination of others over him. None of these conditions seem to apply

[14] For a longer discussion of some issues, see Giddens (1977).

in the circumstances of actual social life, for example in situations class domination.[15]

Similarly, there are various critical observations which one could ma on Habermas's discussion of the rise and decline of the public sphere a the transition from liberal to organized capitalism. His reappraisal Marx, in respect of the latter's analysis of liberal capitalism at lea seems to me to be both too revisionist and not revisionist enough. N revisionist enough, because he accepts too readily that Marx's account w valid in the 19th century; Habermas's portrayal of competitive capitalis is a rather orthodox one in this regard. Too revisionist, because he writ off too completely the relevance of some central Marxian ideas today. O must surely accept that, in classical Marxian theory, the themes of t rationalization of social and economic life, and its consequences for hum freedom, are broached only very inadequately. But I am not convinced th technocratic consciousness has submerged preexisting economic divisio and conflicts as pervasively as Habermas seems to believe.

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[15] Habermas comments on criticisms of this sort in "A Postscript to *Knowledge a Human Interests*" (1973) and in the "Introduction" to the fourth edition of *Theo and Practice* (which appears in the English translation).

Book Reviews

The Transition from Feudalism to Capitalism. Edited by Rodney Hilton. London: New Left Books, 1976. Pp. 195. $8.50.

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In the early 1950s, following the publication of Maurice Dobb's *Studies in the Development of Capitalism* (1946), an exciting debate developed among Marxist historians and social scientists concerning the nature, origins, and dynamics of the transition from feudalism to capitalism. Many of the original contributions to this debate were published in *Science and Society*, where they appeared as a lively interchange of comments and rejoinders for almost two years.

In the quarter century since the first set of articles in the debate appeared, a number of original and thoughtful papers and books have been written on the transition. However, the entire corpus of original articles has been unavailable except in a long out-of-print English collection edited by Paul Sweezy (Sweezy et al., *The Transition from Feudalism to Capitalism* [London: Fore Publications, n.d.]), whose review of Dobb's book touched off the entire debate.

Now, however, a new collection has been published, edited by Rodney Hilton, professor of history at the University of Birmingham. This edition contains all the original materials from *Science and Society*, plus significant contemporaneous articles written for other journals, together with a set of four more recent contributions to our understanding of the transition.

Essentially, the debate centered on three fundamental questions, each of which was important not only for historically specific empirical research, but also for assessing the accuracy of Marxist theory in general. First, the debators asked, what was the nature of feudalism as a mode of production? In particular, they were concerned with the social relations that would present an adequate definition of feudalism and the productive base of feudal social structure. Second, they asked what the role of the feudal state was; its nature, class bases, and process of centralization became specific concerns. Third, and probably most important, were the questions aimed at delineating the "prime mover" in the transition, asking why capitalism emerged from feudalism where and when it did in the first place.

Dobb's original position was that feudalism can be defined by understanding the relationship between the serf and the lord in a "petty" mode of production, that is, the relationship between "the direct producer and his overlord" in which "the producer is in possession of his means of production as an individual producing unit" (p. 58). More important, however, Dobb conceived of the crisis of feudalism, and its ultimate decline, as developing within the boundaries of the feudal mode of production. Feudalism, Dobb argued, collapsed as a result of the

intensification of its own internal contradictions: "The inefficiency of feudalism as a system of production, coupled with the growing needs of the ruling class for revenue, that was primarily responsible for its decline" (*Studies*, p. 42).

Paul Sweezy, on the other hand, argues that feudalism is "an economic system in which serfdom is the predominant relation of production and in which production is organized in and around the manorial estate of the lord" (p. 35). While he cautions that his definition does not imply a necessarily stable or static system, he does note that it is free of "the pressure which exists under capitalism for continual improvements in methods of production" (ibid.).

Sweezy further argues that, not the articulation of internal contradictions, but "the growth of trade was the decisive factor in bringing about the decline of western European feudalism" (p. 41). Here Sweezy supports Henri Pirenne's classic formulation that the reemergence of Mediterranean trade routes in the 11th century (after a four-century hiatus), coupled with the introduction of the Baltic and North Seas as trading areas, was the main impetus in the development of capitalism.

The subtleties of both Sweezy's and Dobb's positions are worked out in a meticulous fashion throughout the remainder of the volume by the other authors. It is interesting to note, however, that nearly every contribution agrees fundamentally with Dobb. Christopher Hill, for example, notes that Sweezy equates "a feudal state with a state in which serfdom predominates" (p. 121) and that Dobb's great merit is that he refutes this equation by showing that "the partial emancipation of the petty mode of production does not in itself change the economic base of society . . . although it does prepare the conditions for the development of capitalism" (ibid.).

Hill's major contribution, in his short "Comment," is in showing that throughout the general crisis of feudalism, and precisely by unshackling the feudal mode of production by the introduction of trade (still within feudalism), the state increased its central power in order to "repress peasant revolt," "use taxation to pump out the surplus retained by the richer peasantry," and "control the movements of the labor force by *national* regulation" (p. 121).

Georges Lefebvre, the great historian of the French Revolution, differs in his account from most others in this volume, arguing that the relations of production (lord and serf) are not the defining characteristics of feudalism at all, but that "the specific characteristic of a feudal regime was the hierarchical relationship between a lord and his vassals . . ." (p. 122). However, Lefebvre alludes only to the political relationship based on military obligation in defining feudalism, and it is clear that while feudalism did maintain a unique political structure, this structure does not define the entire society.

It is Giuliano Procacci, in his "Survey of the Debate," who provides the most convincing refutation of Pirenne and Sweezy when he argues: "To assert that feudalism was an immobile historical formation, not itself capable of internal development but merely susceptible to external

influence, is precisely to pose the problem in terms of random contingency and not in terms of dialectical interaction" (p. 129). Procacci sides here, of course, with Dobb as he argues that capitalism develops within the feudal mode of production: "The 15th and 16th centuries do not appear as a phase in their own right, as a distinct intermediate era between feudalism and capitalism, but as a historical period characterized by the emergence and development of capitalist forms within the framework of a surviving feudal mode of production" (p. 136).

Following the presentation of the "classic" contributions to the debate, Hilton has included four additional essays. The first, which he wrote, "Capitalism—What's in a Name?", reasserts the anti-Sweezy position of the external introduction of capitalism, while further urging a closer look at political relations: "It is not possible to talk of a capitalist *society* when political power is still in the hands of a feudal aristocracy" (p. 154).

Two short pieces, one by Eric Hobsbawm, the other by Dobb, were first published in *Marxism Today* (1962). Hobsbawm regards the 16th-century transition as a key to understanding uneven development and the origins of contemporary underdevelopment. (His position is therefore similar to that of the recent work by Immanuel Wallerstein.) Dobb restates his earlier argument in the form of simplified axioms. The final article, recently published in *New Left Review,* is John Merrington's "Town and Country in the Transition to Capitalism," which provides perhaps the most exciting and original thought on the growth of towns and the concomitant "ruralization" of the countryside since the debate began.

Rodney Hilton also provides a fairly long introduction to the book in which he describes both the chronological and conceptual histories of the debate, from its classic origins to the most recent research. But, of course, the real "classic" formulation is that of Marx, to whom all contributors to the debate look for justification. But this volume is not a simple homage of the true believers to the great master; it is a book of careful and insightful (often brilliant) exposition by some of the foremost minds in Marxist social science in the past 30 years.

It is also exciting to be allowed to experience the chronology of the debate as a process in itself. It is captivating to experience the lively exchange of comment and rejoinder; in this sense, the collection of essays presents more of a dialectical process than a comprehensive historical analysis. No synthesis of the various positions is offered, although Merrington comes perhaps the closest to realizing one.

This book is already an important document of Marxist scholarship and will certainly become a valuable reference for researchers into the transition from feudalism to capitalism. It also provides a fine (even if partially obscured by its overt intention) presentation of historical methodology, informing us where to look if we wish to understand the transition. Finally, the book is important because it is about an era of transition from one mode of production to another, a firm historical understanding of which is particularly relevant to those of us who find ourselves involved in another modal transition.

The Formation of National States in Western Europe. Edited by Charles Tilly. Princeton, N.J.: Princeton University Press, 1975. Pp. xiv+711. $22.50.

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This book is the latest addition—probably the penultimate one—to a series of studies on modernization sponsored by the Committee on Comparative Politics of the Social Science Research Council. As the title indicates, it focuses on the processes of state formation in early modern and modern Europe (i.e., 16th century through the 18th century and to some extent through the 19th century), with an emphasis on the analysis of the concrete processes through which states were formed in Europe. The list of chapters gives us a very good picture of the contents of the book in general and of the type of processes of state formation with which the book is concerned.

The contents attest also to the way in which this book departs from other volumes in the Committee's series. The nature of some of these departures is stated clearly in the statements by L. W. Pye, the chairman of the Committee, in his foreword, and by the editor, Charles Tilly, both in his introduction and in his summing-up chapter. For example: "This eighth volume in the series of Studies in Political Development represents two new departures in the intellectual history of the Committee on Comparative Politics of the Social Science Research Council: a 'return to Europe,' and an attempt to collaborate with historians" (p. ix).

Tilly's major concern is to understand why the nation-state emerged as a major political form in modern Europe:

> The hard questions are: (1) what structural alternatives were possible; and (2) why this alternative rather than the others? I have identified the empire, the theocratic federation, the trading network and the feudal system as possible alternatives. I have then gone on to propose that the preexisting political fragmentation, the weakness of corporate structures, the effectiveness of specialized organization, the openness of the European periphery and the growth of cities, trade, merchants, manufacturers, and early capitalism weighted the outcome toward the national state. Such a statement implies that a number of other factors like the particular geography of Europe, its cultural homogeneity, and its largely peasant population did not affect the choices among outcomes so much. The way to check out such assertions is through a detailed comparative examination of the correlates of state-making and the alternative processes both in Europe and elsewhere. But this book can be no more than a preface to that noble enterprise. . . . [P. 31]

The chapters on the different aspects of processes of state formation constitute the core of the book and provide very detailed and well-organized data on such processes, data not readily available to social scientists and but rarely analyzed by historians in the framework of comparative social science problem setting. In this way, the book's approach

to its subject is in the institutional-historiographic tradition of such scholars as Otto Hintze, Marc Block, and E. Brunner.

The book's orientation is on the whole more systematically comparative and analytical than the work of these scholars, but the individual chapters are probably less so. The major contributions of each of these chapters have been ably surveyed by the editor. For reasons of space, I shall quote, at random, only two passages from Tilly's survey, which will yet convey the general flavor of the contents.

> S. E. Finer traces the relationship between the development of major features of the modern state (territorial consolidation, specialized personnel, integrity recognized by other states), its special case the modern nation-state (which adds self-consciousness of common identity, as well as some mutual distribution and sharing of duties and benefits) and the changing character of national armed forces (especially as summed up by "format," which includes basis of service, size composition and stratification). His principal method is to compare the transformations of France, Britain, and Prussia-Germany in both regards arguing causal connections by comparison. And his chief conceptual device is the identification of clusters of variables, or "cycles": (1) economy-technology-format; (2) stratification-format; (3) beliefs-format; (4) format options; (5) extraction-coercion; and (6) state-building. Having identified the cycles in this way, he then forms his argument as a series of statements about links among the cycles. [p. 51]
>
> Rudolf Braun adopts a rather different approach to taxation from that of Gabriel Ardant. He devotes himself more assiduously to the construction of a continuous account of fiscal policies in England and Brandenburg-Prussia from the sixteenth century onward. He is much more concerned to pin down the differences between the two countries, and to explain them in historical terms. Whereas Ardant emphasizes the obstacles to the success of different kinds of fiscal policies and the respects in which the economic infrastructure constrained the system of Taxation, Braun usually begins with the political situation which led the British or German rulers to adopt a given set of policies as a fact whose long-run consequences become the focus of historical investigation. [P. 85]

The specific contribution of each chapter has, of course, to be evaluated by specialists, but, taken together, the contributions seem to provide a much more dynamic picture of processes of state formation than can be found in most of the literature on modernization.

The emphasis of the picture presented in this book is on the historical-comparative approach much more than on what Tilly calls the developmental ("stage") and functional approaches which have, according to his summary, dominated the literature on modernization or on comparative political developments—although he stresses that in principle the three approaches are not incompatible. In this volume, such a broad historical comparative approach is represented in the chapter by Stein Rokkan which constitutes the fullest elaboration to date of his ideas about processes of state formation in Europe, as well as in Tilly's own analysis, in the first chapter of the book, of the historical background of Europe.

Throughout these chapters there is also a very strong stress on the

international system as influencing the process of state formation—thus emphasizing perhaps even more than before the unique nature of the European experience.

The way in which the more general theoretical conclusions are presented does not do full justice to the theoretical potentialities of the materials gathered in the various chapters, to the analysis of historical conditions of Europe presented in Tilly's introductory chapter, or to the material in Rokkan's chapter.

The concrete processes of state formation analyzed in these chapters, be they the formation of armies, of police agencies, taxation structures, or the like, are not specific to Europe. Such processes have existed as part and parcel of the formation and maintenance of all large-scale "traditional" states, including imperial systems like the Chinese, Roman, or Byzantine ones or such "patrimonial societies" as the Ahmenid Kingdom or ancient Middle Eastern or South East Asian states. Hence there arise some questions as to the differences between these processes in these "historical" societies and in the modern European setting and the ways these processes differ from those in modern states outside Europe.

The weakness or lack of validity of the answers of the older models does not diminish—as Tilly fully recognizes—the importance of these questions or the search for answers to them. Such answers are not, however, provided in this book; at most, some very preliminary indications can be found in the sense that the book presents a sort of "in-between" stage in the theoretical development of comparative historical studies in general and those of modernization in particular. But at the same time the richness of the materials, as well as the new analytical departures presented in it, provides important points of departure for future inquiry.

The Transition to Socialist Economy. By Charles Bettelheim. Translated from French by Brian Pearce. Atlantic Highlands, N.J.: Humanities Press, 1975. Pp. 255. $17.45.

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This is a collection of six essays, written in the mid-sixties, arising out of a series of lectures given by Charles Bettelheim at the Sorbonne. It is eight years since it was published in French under the title *La Transition vers l'économie socialiste* (Paris: Maspero, 1968). Bettelheim's views on the subject of the economics of socialism have been rapidly evolving in a direction radically different from the one he was pursuing when he began this series of essays. Indeed, this collection signals many of the changes which were to come in his views, and hence, though the book has been superseded by his later books, its belated publication is to be welcomed.

Early mechanical versions of Marxism describe the transition from capitalism to socialism as an irreversible, unilinear development. Many features of the Soviet economy which were thought to be ominous from a

socialist perspective were often excused by saying that the Soviet Union was in a transition to socialism but had not yet arrived there. But state ownership of means of production more than 50 years after the Revolution has not solved the economy's many problems. Bettelheim looks at this from a Marxist perspective.

The principal contradiction is that, while means of production are collectively owned, there is still wage labor. The control of means of production is in the hands of a bureaucratic priority which has the power of hiring and firing workers. In addition, many production and consumption decisions reveal that planning and market coexist. Commodities are still being produced and the law of value has not been superseded.

In Western literature on Communist countries, the inefficiencies of these economies in many respects have been blamed on the lack of a market mechanism, and on the lack of profit incentive for management of public enterprises and for farmers. For many Marxists, the market principle is anathema, and they would wish to hasten the centralization of decision making and the abolition of all exchange. The debate in the Soviet Union which centered around Libermanism, excellently surveyed by Bettelheim in this book, was about the scope of the price mechanism in a planned economy.

Bettelheim quite rightly finds much of this debate unsatisfactory, as questions were not framed in a theoretical manner. The works of Marx, Engels, and Lenin were rummaged for practical recipes on how to run a socialist economy but not for theoretical concepts needed for formulating the questions to be tackled. Bettelheim sees his task as one of constructing the theoretical concepts of an economy in transition to socialism from which he hopes can be derived technical (operational) concepts which will answer the practical needs.

One of the building blocks of such a theory is the concept of the mode of production and, inasmuch as transition from one mode to another is at issue, the dissolution of an old mode and the beginnings of a new one have also to be looked at theoretically. If Marxism is to be a systematic body of thought (or a science, as Althusser would have it) one should have a theory of all transitions, for instance, from feudalism to capitalism. Marx's discussion of the primitive accumulation of capital relates to the earlier transition and should be useful.

Taking up the basic Marxian concepts of production forces and production relations, Bettelheim's basic thesis is that in the stage of transition there is a noncorrespondence between property relations and relations of real appropriation of surplus. Thus, while the property relations correspond to collective (or at least state) ownership, real appropriation of surplus is still to a large degree dependent on the exploitation of wage labor. Bettelheim does not explicitly bring out the importance of wage labor. He deals with real appropriation in the form of profits of enterprises through the operation of commodity categories.

This noncorrespondence is, however, basic to all transitions. Only after the transition is completed do forces and relations of production match each other. Thus, the putting-out system in the early days of capitalism is

a transitional form, while factory labor corresponds to the capitalistic mode itself. In this transitional stage, politics mediates in economic life in a strategic fashion. Decisions of a technical economic nature, that is, use of prices in planning, are political decisions as to the pace at which the noncorrespondence is to be overcome.

During the transition, therefore, while the forces of production are not sufficiently developed to allow for full collective decision making, but have to be hastened in that direction, Bettelheim finds that planning which is conscious of social needs allows calculations in terms socially necessary to labor, but the imperfections of information and the inadequate integration of economic activity still justify prices and commodity categories to implement the strategic decisions made by Manning.

Bettelheim has raised the debate regarding transition to a theoretical level. This collection embodies his struggle in arriving at an adequate theoretical formulation. It is only in the last essay on the pricing problem in the Soviet Union that we see his accomplishment.

Space is inadequate for criticisms, but one must mention that economic relations are still thought of in technical terms of markets, planning by computers, integration of firms, etc. The nature of the wage labor category and the questions of economic and political democracy it raises for a socialist economy are hardly tackled at all. But Bettelheim himself has gone on from this to a political critique of the Russian, Cuban, and Chinese experiences in his later work which will soon be available in English. In the meantime, for Radicals, Marxists, and Socialists alike, this will be tough but beneficial reading.

Interest and Ideology: The Foreign Policy Beliefs of American Businessmen. By Bruce M. Russett and Elizabeth C. Hanson. San Francisco: W. H. Freeman & Co., 1975. Pp. xiv+296. $10.95 (cloth); $4.95 (paper).

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Bruce Russett and Elizabeth Hanson have presented a much-needed portrait of American big-business thinking in the 1970s. Their care, rigor, and adeptness in the use of a variety of empirical techniques give us a view of a generally conservative, hawkish group, though one apparently less apt now to choose military intervention as a national option. Unfortunately, conceptual and theoretical problems make the explanatory power of their book less than satisfactory.

Their aim is quite ambitious: to determine whether the roots of foreign policy beliefs held by American elites, specifically businessmen (we're not told if any of their sample of corporate vice presidents is female), can be found in strategic, bureaucratic, economic, or ideological considerations. The strategic consideration pertains to national security requirements; the bureaucratic one to specific organizational interests; the economic one to

economic system needs; and the ideological one to the force of public symbols (e.g., anticommunism).

Beginning with a useful chapter on the need to study the perceptions of elites by nontraditional methods, Russett and Hanson then review several varieties of economic interest theories. From these theories they develop very simple testable hypotheses (e.g., *"Businessmen will be more hawkish on a variety of foreign policy issues than will other elites"* [p. 36]), a process which continues throughout the book.

The third chapter introduces the major data set of the study: the responses, in 1973, of 567 vice presidents of *Fortune* 500 companies (industrial and financial) to 49 questions, most of which dealt with foreign affairs. The authors put many of the same questions to a sample of military officers, and they also had access to data from a 1971–72 survey of elites collected by Allen Barton and his associates at Columbia University. Their basic conclusion, that "domestic political ideology is a far more powerful predictor of foreign policy preference than is either economic interest and motivation or strategic ideas" (p. 126), is, however, probably as much an artifact of their narrow view of economic interest as it is a reflection of the data. For example, table 3.3 (p. 83) presents seven policy objectives toward "less developed countries," ordered by respondents according to their relative importance to the United States. Both the military and business samples attach the most importance to "a stable government capable of preserving internal order," even more than to "a government which retains the free enterprise system." Russett and Hanson consider the latter option to be an economic criterion, but not the former. One could easily make the case that a stable government is a necessary condition for American investment and hence should be considered at least partly an economic criterion. That businessmen and military officers may also be thinking this way is indicated by their greater concern for "a stable government" than for one "which maintains civil liberties."

The authors' restricted view of economic interest follows from their theoretical decision not to view ideological positions as class based. Rather, they conceptualize economic interest and ideology as being more or less unrelated and mutually exclusive. They understand the Marxist superstructure argument, but, in choosing to present interest and ideology as they do, they reject it. In the preface, they call themselves "non-Marxists" but not "anti-Marxist" (p. xi). They admit to a dislike for past U.S. foreign policy and disdain "value neutrality," but they fail to present a complete statement of their own theoretical position and biases, except for their avowed unwillingness to accept the *entire* Marxist system. On the whole, their orientation stands in the pluralist tradition. From their perspective all elites seem separate, if not equal, with no overlap worth mentioning.

Yet all of us have our own theories, however nonpublic they may be, and these move us in certain directions. Russett and Hanson, while trying to be open and unbiased, mistake a non-Marxist position for an atheoretical pose. This same orientation leads them to overemphasize empirical

rigor at the expense of substantive concerns. For example, one of the most intriguing findings of their content analysis of the business press (chaps. 6 and 7) indicates that the *Wall Street Journal* consistently displays more skepticism over American foreign intervention than do the other business sources sampled. Rather than inquire into the meaning of this apparent split in the business community, the authors instead get bogged down in methodological considerations, such as how much "weight" the *Journal* should be given in the total business sample.

Perhaps not surprisingly, economic reasons do not receive wide mention in the business press as cause for intervention. While the authors seem cognizant of the dangers of recognizing blatant economic statements as the sole indicator of economic motivations ("there is the possibility that economic interests, despite their inconspicuousness, were critically important, and predisposed the writers toward the strategic and ideological arguments they presented" [p. 232]), they nevertheless conclude that strategic reasons predominate (p. 239). Further, they give the benefit of the doubt to the importance of ideological considerations: "Ideological reasons were expressed with such intensity that they must be assumed to have had greater influence than is evident solely from the tables" (p. 239).

Despite these theoretical problems, Russett and Hanson have provided some necessary empirical description of a neglected area in social science. They are at their best in their historical sections (e.g., "How Liberals Become Doves," chap. 4) and in their multimethod approach to individual problem areas. For example, through survey research and correlational analysis of stock-market behavior with war-related events (chap. 5), they make a case for a business shift against the Vietnam War in 1967.

One final point: The authors have accepted research money from the Navy to help support their study. They describe the lack of restrictions from the Defense Department in carrying out their work. They are, in effect, "clean." One might dissent, though, from this bill of health. Even if the money has not corrupted the study, their use of it serves to legitimize the Navy as a source of social science funding. The Navy's interests in social science are not, shall we say, progressive, and it seems to me that any legitimization of its research here only serves to increase its power.

False Consciousness: An Essay on Reification. By Joseph Gabel. New York: Harper & Row, 1976. Pp. xxx+358. $25.00.

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The concept of false consciousness has been one of the most problematic concepts in Marxist thought. For Engels ideology and false consciousness were more or less synonymous: "Ideology is a process accomplished by the so-called thinker consciously indeed but with a false consciousness. The real motives impelling him remain unknown to him, otherwise it would

not be an ideological process at all." On this basis it would seem that only Marxism itself can escape the blight of false consciousness. However, while it is quite valid to argue for a dependent relation between ideas and social structure, it clearly does not follow that such a relationship necessarily implies error, distortion, and illusion. Lenin used the term "socialist ideology," for example, with other notions besides deception and illusion in mind. The corollary to false consciousness is true consciousness, and if all ideology is equated with illusion and error it becomes impossible to grasp the real sociological foundation of ideological thought. For it is through ideology, whether in the form of commonsense, political, or social and economic ideas and doctrines, that the individual in modern society relates to and makes sense of the social world. Contemporary society, with its complex division of labor and class structures, would be impossible without some form of ideological legitimation at the levels of government and politics as well as the more mundane level of everyday experience. It therefore follows that if ideology is defined as illusion and false consciousness—a closed, dogmatic system—its social function is minimal. The sociological concept of ideology understands it as a living force which binds the various and conflicting structures of society together into a social and historical unity. It is for these reasons that the Marxist concept of false consciousness may obscure more than it clarifies.

Joseph Gabel's study of false consciousness, first published in France in 1962, is one of the few attempts to analyze in some depth the exact meaning and sociological significance of the concept of false consciousness within the framework of what he calls "open Marxism." His starting point is the assertion that "false consciousness is not merely central to the Marxian doctrine, it constitutes its entire framework; a great many—if not all—of the problems that Marxist thought poses are, in the last analysis, problems of false consciousness" (pp. 3–4). It is important to note that Gabel does not really defend this argument in terms of an analysis of Marx's own writings or indeed of the Marxist tradition in general but relies almost entirely on one single text, Lukács's *History and Class Consciousness* (1923), and while reference is made to philosophers such as Bergson and Heidegger it is Lukács's ideas which dominate.

What, then, is Gabel's concept of false consciousness? Briefly, he argues that it is consciousness which has lost all real contact with the social world; the social world is constructed in thing-like terms as an external force which dominates the individual, rendering him passive and incapable of praxis. False consciousness, then, is equated with reification and alienation, key terms in Lukács's *History and Class Consciousness*. Gabel goes on to show how the alienated individual has lost all sense of the dialectical character of the social world, especially his grasp of the roles played in it of time and history. Thus, "False consciousness and ideology can therefore be defined as forms of sub-dialectical perception of social reality" (p. 42). Like Lukács, Gabel defines all ideology as false, including Marxism itself and presumably Gabel's own brand of "open Marxism." Indeed, from Gabel's text it is extremely difficult to know what he means by the opposite of false consciousness, what he calls "de-reification," since

he never states the criteria by which a true consciousness can be judged; he is less interested in the scientific aspects of Marxism (Marx's "positive science") than its philosophical elements, and he thus makes no attempt to relate the concepts of alienation and reification to social structure. Gabel remains on the level of vague generality as long as he is discussing ideology and false consciousness as sociological phenomena.

But the main point of the book lies in his extended analysis of schizophrenia as an exemplar of false consciousness and as an individual rather than collective phenomenon. A schizophrenic is identified as a person "dominated by spatiality" (p. 80), lacking any real grasp of time and history, of the past, the present, and the future; there is a loss of the ability to distinguish between subject and object; the individual feels crushed by the world; he has become a mere thing and object.

Of course, in a general sense there is much truth in these arguments, but the real problem lies in Gabel's confusion between a mental illness and a totally dehumanized phenomenon such as reification. Thus he quite rightly notes the reification embedded in the role of the official within the Nazi concentration-camp hierarchy but confuses this popular concept of schizophrenia with its scientific, clinical usage: The Nazi officials were not mad or ill in the same way as schizophrenic patients who are certainly not "sub-human people" (p. 133). Gabel's identification of schizophrenia, reification, ideology, and false consciousness is ultimately ludicrous; to suggest that ideologies have a "schizophrenic structure" and possess an "autistic nature" is to misunderstand the ways in which ideologies actually work in the modern world—as highly flexible structures whereby the individual understands the social world and acts within it. It is the sociological structure of ideology, therefore, which gives it a real content and a positive social function. In this sense ideologies contain both true and false elements, and it is a travesty of Marxist thought to conflate the highly complex notion of ideology with such an extreme form of mental illness as schizophrenia.

Ästhetik und Soziologie um die Jahrhundertwende: Georg Simmel. Edited by Hannes Böhringer and Karlfried Gründer. Studien zur Literatur und Philosophie des neunzehnten Jahrhunderts, Band 27. Frankfurt am Main: Vittorio Klosterman, 1976. Pp. x+281.

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This is one of the truly important books about Simmel. The contributors approach him and aspects of his work from various angles but always seriously and interestingly. There are three parts: six papers, with excerpts from the discussion, that were presented at a 1973 meeting on Simmel, sponsored by the Fritz Thyssen Foundation (also the sponsor of the series in which this volume appears); three additional contributed papers; finally, some biographical and bibliographical documents.

Karlfried Gründer, one of the two coeditors, writes that Simmel was chosen as the most attractive subject for the purpose of extending the investigations of the Thyssen Stiftung from the 19th century into the 20th. (Indeed, in his memoir of Simmel, which is not mentioned in this volume, Georg Lukács calls him a typical phenomenon of transition, though not only in a historical sense.) In an essay on the "contours" of his thought, Michael Landmann continues his devoted study of Simmel, providing a short piece that is nevertheless perhaps even more comprehensive as an overall map than Gertrud Kantorowitz's (translated in Kurt H. Wolff, ed., *Georg Simmel, 1858–1958*), of which, of course, he had the benefit. (But Landmann shares Simmel's obtuseness about the life experiences of types of people outside their own circles: "While according to Marx," he writes, "only the abolition of 'alienation' will reestablish full individuality, in the Simmel of the *Philosophy of Money* it is precisely the alienations that begin in the modern period which bring forth the individual belonging to that period" [p. 7]: Landmann raises no question about this misunderstanding of Marx.) The worthwhile general discussion following his paper touches on, among other topics, Simmel's relations with Stefan George and Rilke (as do other contributions in part 3), on Hegel, Marx, Kierkegaard, and the philosophy of life.

In an essay on Simmel and Rodin, J. A. Schmoll gen. Eisenwerth contrasts Simmel's and the art historian's language when discussing art and artists. In several articles on Rodin (perhaps the most important of which is reprinted in part 3 for the first time since its original publication in 1902), Simmel argues that the history of sculpture ends with Michelangelo and begins again only with Rodin, who symbolizes the "Heraclitism" of modern life. Schmoll provides insight into the discussion of art around 1900; especially poignant is Trotzki's remark in 1911 that "Simmel's 'new soul' is in reality the soul of metropolitan intellectuals" (p. 36). Again there follows a fruitful discussion.

In "Aestheticism and Sociology in Georg Simmel," Sibylle Hübner-Funk presents both a useful summary of Simmel's sociology and a Marxist, "sociology of knowledge" critique. According to Hübner-Funk, his sociology cannot overcome aestheticism. Hübner-Funk's paper provoked an extensive discussion, including a sensitive critique by Lenk. Hübner-Funk includes excerpts from Simmel's *excursus* on the sociology of the senses (translated in Park and Burgess, *Introduction to the Science of Sociology*), from Walter Benjamin's "Der Flaneur," and from Benjamin's and Adorno's correspondence on Simmel, and he clarifies observations on the difference between an aesthetic sociological perspective and a sociology of art. Underlying the discussion is the question, pointed especially at Hübner-Funk, of what lends itself to and what withstands sociological analysis.

The next two papers are more purely sociological. In "Georg Simmel's Significance for the History of the Role Concept in Sociology," Uta Gerhardt presents an inventory of three approaches to "role" actually taken by Simmel, but she does not consider the problem of where the role ends and the person or individual begins; the ensuing discussion gives a

good picture of the contemporary conceptualizations of "role." Julien Freund's "The Third in Simmel's Sociology" "codifies" Simmel's observations on the triad and hints at characteristics of it that do not, as they do in Simmel, depend on comparison with the dyad; some observations in the discussion could be developed into an analysis of the social scientist as "the third." To end part 1, Hannes Böhringer, coeditor, examines in his analysis of "traces of speculative atomism in Simmel's formal sociology" his usually neglected beginnings, which were influenced by Spencer, Fechner, and *Völkerpsychologie*.

In "Money or Life: A Metaphorological Study of the Consistence of Georg Simmel's Philosophy," Hans Blumenberg develops Simmel's philosophy of money by comparing it with his philosophy of life. Of special interest and timeliness is Blumenberg's juxtaposition of money to the traditional philosophical problems of omnipotence and immortality. Simmel's influence on Jaspers is traced by Hanoch Tennen in a methodologically exemplary and penetrating paper which raises vastly important questions. Donald N. Levine, Ellwood B. Carter, and Eleanor Miller Gorman's "Simmel's Influence on American Sociology," the only paper in English, was, fortunately for American readers, also published in the January and March 1976 issues of *AJS*. It is a boon for the historian of American sociology. The authors distinguish three types of influences Simmel has had (they plausibly explain them by reference to historical circumstances in general and the developmental stages of the discipline in particular): helping to define its subject matter, providing an analytical perspective, and articulating novel topics and propositions. They give excellent histories of the treatment of the stranger, social distance, the metropolitan mentality, small groups, interpersonal knowledge, secrecy, secret societies and related phenomena, conflict, and exchange. The person or human being who was interested in these and many other phenomena and problems unfortunately and characteristically remains unexamined; products are treated as if they were of interest only detached from their producer. Nor is there (but I know no other place where there is) anything on Simmel as a phenomenologist (Alfred Schutz is mentioned only as mediating between him "and a recent group of sociologists of 'everyday life' which includes Harold Garfinkel, Aaron Cicourel, and Jack Douglas" [p. 194]).

Part 3 opens with Simmel's inspired 1902 essay on Rodin and a previously unpublished, brief but delightful fragment of "The Fairy Tale of the Color." There follow two letters he wrote in 1897 responding to a question about his views on Zionism: Depending on the point of view, he answered, Zionism was either a utopia or a crime—he seemed blinded by his liberalism, as were so many Jews and non-Jews, even into the time of Hitler. The letters make one tremble—*post factum*, one hopes. In a beautiful (but unfortunately undated) dedicatory epistle to Bernhard Groethuysen, Charles DuBos, a student of Simmel, who admired him, recalls his student days with him in Berlin. Hans Simmel, Simmel's only son, provides excerpts from his memoirs which are extraordinarily rich on his father as a person. This is new and illuminating material, particularly

in regard to Simmel's economic situation, his relation to Stefan George, Rodin, and Bergson, the unsuccessful invitation to join Heidelberg University, his humor, the impact on him of World War I, and his last years in Strasbourg. At least these pages (preferably in a more complete version) and Simmel's paper on Rodin should be made available in English. The book ends with two short pieces by Michaël Landmann on Ernst Bloch's and Arthur Stein's memories of Simmel and bibliographical gleanings by and on Simmel from the last few years.

For the student of Simmel and his period, this work is indispensable.

The Sociology of Karl Mannheim: With a Bibliographical Guide to the Sociology of Knowledge, Ideological Analysis, and Social Planning. By Gunter W. Remmling. Atlantic Highlands, N.J.: Humanities Press, 1975. Pp. xv+255. $15.00.

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The reigning technocrats of sociology have never completely silenced Karl Mannheim. He continues to speak from the humanist margins and beyond the little empires of academic specialization to those larger questions of meaning and politics which properly constitute what C. Wright Mills called "the sociological imagination." In his relatively short and lively essay on the life, times, and work of Mannheim, Gunter Remmling reminds us that Mannheim was often abundantly possessed of the sociological imagination. Throughout his adult life, spanning revolution, depression, fascism, and war, he continually posed the big questions, although he frequently gave unsatisfactory answers.

In response to the intellectual and political conflicts of Weimar Germany, Mannheim's *Ideology and Utopia* (London: Routledge & Kegan Paul, 1936) posed the problem of meaning and valid knowledge in a context of doubt. His unconvincing solution was to push historicism to the limits and look for solace and synthesis in the efforts of detached intellectuals armed with reason and the sociology of knowledge. But in Germany Fascists were armed with guns; there was no free-floating space for intellectuals between fascism and antifascism. Not surprisingly, once he became a refugee in wartime England, Mannheim abandoned his original formulation in the sociology of knowledge and the abstract problem of meaning for a more fundamental problem of capitalism —irrationality, the inability to plan for the satisfaction of social needs. Impressed with the fact of continuity and the possibility of reform in England, Mannheim concluded that bourgeois democracies could survive only through social planning and educating the next generation to "substantial" rationality and commitment to ultimate values. From being an extreme relativist, Mannheim thus became a strong spokesman for a spiritualized welfare state as the alternative to laissez-faire capitalism and socialism. He diagnosed the problem correctly but, ever the detached

intellectual without a conscious class standpoint, theory of history, or concrete agent for planning and change, he looked naively to the leadership of intellectuals and well-doers now armed not with the super-relativistic sociology of knowledge but with pragmatism and Christianity. However provocative Mannheim's sociology of knowledge, it ended precisely where it began, in the clouds of idealism.

Such at least is one evaluation that can be drawn from Remmling's essay. Although Remmling never quite sides with the "left" or "right" critics of Mannheim, he gives us a fair assessment of the man as reformer trying to add a dash of Anglo-American pragmatism to the utopian dreams of Comte and St. Simon. Mannheim's agents of "orderly progress" were intellectuals and priests, ideas and values pragmatically fitted to conditions and possibilities, *not* classes, not ideologies, and not utopias. Yet with his implied criticism, Remmling remains basically sympathetic to Mannheim's project. The book is written in the style of the sociology of knowledge. We are made to understand Mannheim in relation to larger structures of philosophies and politics. The resulting book is an essay on and a demonstration of the sociology of knowledge and a useful summary of Mannheim's work classified into four stages, stretching in time from 1918 to 1947 and moving from such interests as philosophy and the sociology of knowledge to the sociologies of planning, religion, values, education, and power.

Remmling's essay on Mannheim constitutes slightly over 50% of his book. The rest is a less-than-satisfactory bibliographical guide to Mannheim, the sociology of knowledge, ideological analysis, and social planning. Though containing much of value, the references are limited mainly to the years before 1970. The selection also suffers from a narrow definition of the field. There are few references to planning in socialist countries, which in fact have the social conditions which make planning possible. There are few references to the development of the sociology of knowledge made in academic circles by critical theorists, ethnomethodologists, and structurally oriented Marxists, or in more politically involved circles by radicals trying to cope with ideology as it affects class and is constituted by racist, sexist, and elitist practices. However, the scope of the sociology of knowledge can be defined in many different ways; all in all Remmling's definition remains close to that of Mannheim. This is the strength and weakness of his book.

Max Weber's Theory of Concept Formation: History, Laws, and Ideal Types. By Thomas Burger. Durham, N.C.: Duke University Press, 1976. Pp. xviii+231. $9.75.

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This book is yet another attempt to address one of the standard questions in the history of sociological thought—the meaning and in-

terpretation of Max Weber's methodological logic, particularly the ideal type. Burger's attempt at sociological *satori* in contemplation of the master's classic *koan* achieves an enlightenment of mixed results, but overall it is a valuable contribution.

The core of his approach is the assertion that Weber's seemingly unsystematic methodological writings make sense only if one realizes that the domain assumptions of his methodology depend almost entirely on the philosophy of science of Heinrich Rickert. "Since almost all existing interpretations of Weber's methodological writings have failed to see (or to take seriously the thought) that the latter's unity and inner coherence can be found in Rickert's philosophy of science, they have frequently not been able to reconcile all of Weber's statements with each other" (p. xv).

Chapter 1 is an analysis of Rickert's methodological writings, specifically his theory of concept formation in the context of his rationale for history as a science. Chapter 2 discusses Weber's treatment of the same issues. The central claim is that Weber's methodological writings make sense only when it is realized that they are an answer to the question of what justifies history as a valid scientific undertaking. Burger argues that this central question and Weber's answers derive directly and completely from Rickert's thought. The third chapter treats aspects of Weber's methodology that were not specifically addressed by Rickert, but which he claims still flow from Rickert. The main focus is on the genesis of the ideal type. The final chapter is an interesting attempt to show the relevance of the ideal-type approach to concept development in modern sociology by reformulating Weber's approach in terms of "models."

There are valuable insights in this book. Among them is the treatment of Weber's development of ideal types as a response to specific historical research problems. And there are some valid clarifications of numerous aspects of the ideal-type approach, such as the emphasis on inductive development of ideal types and the realization that ideal types are not pretheoretical concepts, but contain propositions about relations among variables. Yet many of these important clarifications exist elsewhere in recent literature, and many of Burger's important insights about the ideal type do not depend on his analysis of Rickert. There are also some bothersome inaccuracies and omissions that detract from the generally sound and well-grounded logical analysis.

For example, in his determination to show complete convergence with Rickert, Burger makes a weakly supported and misleading interpretation of one of Weber's basic tenents of objectivity in the cultural sciences. Starting with the interpretation "that Rickert postulated the existence of absolutely valid values which can serve as viewpoints for historians" (p. 87), he tries to argue that all other interpreters of Weber are wrong and that, in fact, Weber accepts absolutely valid value viewpoints from which history can be interpreted. This is clearly erroneous, given Weber's existential insistence on the irreconcilable conflict of ultimate value viewpoints in human experience. As a result, Burger misinterprets Weber on a number of specific aspects of the general issue of objectivity. For example, he states, "Weber accepts the ideal of absolutely valid historical

knowledge, although with the qualification that it must always be tied to special viewpoints, and must therefore be one-sided" (p. 88). This does not imply, as Burger suggests it does, that Weber accepted absolutely valid values on which to base historical analysis. On the contrary, Weber recognized that human attempts at complete objectivity in pursuit of scientific truth are not infallible. Once values have legitimately selected the historical object of study, they cannot be totally prevented from illegitimately affecting the scientific investigation.

The exclusive focus on Rickert's philosophy of science also obscures important substantive theoretical concerns that influenced the formulation of the ideal-type method, in particular, the origins of the ideal-typical approach in Weber's critique of social evolutionism, as Guenther Roth has shown in his essay, "The Genesis of the Typological Approach" (in *Scholarship and Partisanship: Essays on Max Weber,* by Reinhard Bendix and Guenther Roth [Berkeley: University of California Press, 1971]). The author might have achieved a better balance with respect to this type of substantive influence if he had been more open to attempts to interpret Weber's ideal-type approach in the context of its actual use in his empirical and historical sociology, as Stephen Warner has recently done in his Ph.D. dissertation, "The Methodology of Max Weber's Comparative Studies" (University of California, Berkeley, 1972). Instead, he explicitly rejects such an approach, and the book suffers as a result.

In sum, the book is worthy of a critical reading. There is much of value in it despite some inadequacies. Unfortunately, however, its insights are buried in an unnecessarily pedantic style that out-Webers Weber.

The Legacy of Albion Small. By Vernon K. Dibble. Chicago: University of Chicago Press, 1975. Pp. x+255. $15.00.

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Although Albion Small is best known today for having founded the first department of sociology (1892) and the *American Journal of Sociology* (1895), it is not with these works that Vernon Dibble deals primarily in this recent addition to the Heritage of Sociology series. His concern lies with Small's thought, attempting to explain Small's stress on sociology as an empirical discipline and demonstrating the sources and internal logic of Small's fusion of scientific sociology and ethics. He also considers why the one has survived and the other has not. Close to two thirds of the book is devoted to Dibble's commentary, about one fourth to primary readings from Small, and the remainder to extensive (and useful) end notes and a short index.

Dibble makes the case that Small's political ideology and pedagogical philosophy led him to turn away from the established discipline of history to the new discipline of sociology. To paraphrase Dibble's argument, since Small was against both laissez-faire capitalism and radical alterna-

tives, he wished to turn out students who were both reformist and centrist. The way to accomplish this was to engage them in empirical study of the total society. "For the observation of actual social conditions both prevents complacency and instructs students in the complexities of society which utopians leave out of view" (Dibble's words, p. 20). To Small, being "objective" about society inevitably meant being holistic and also perceiving the gradualist nature of social change. Sociology as the comprehensive empirical social science would provide the knowledge for proper reform.

Dibble then dissects Small's views on sociology and ethics. To paraphrase again, Small believed that all ethical judgments are made in relation to particular circumstances and ends and are hence inevitably relativistic. However, the more circumstances and ends are taken into account, the closer ethical judgments approach to resting on universal standards. Sociology provides the content for ethical forms objectively by taking into account more and more of society's complexities and identifying the basic human ends that are displayed. Small identifies six "interests" as inherent human ends; interests in health, wealth, sociability, knowledge, beauty, and rightness. These ends contain within them ethical standards: The "good" is the satisfaction of these ends in larger amounts and balanced proportions. Thus, sociology's province includes providing ethical standards, since they inherently flow out of its observations, and indicating ways in which these standards can be further realized in the totality of the "social process." As Dibble sees it, Small's thought is logically consistent in fusing sociology and ethics through the "telicist ontology" (p. 92) that he accepts.

Dibble roots Small's views of sociology in his small-town background, his strong Protestant religiosity, his political views, and his acceptance of the then-prevalent role of university professors in providing moral leadership. He identifies four possible factors in later abandonment of Small's fusion of sociology and ethics: the impolitic nature of his claim that sociology is the totalistic social science; the increasing attachment of sociology to centers of power, with less need for moral sanction; the general shift away from Small's reformist yet centrist political position; and the shifting position of universities toward disinterested inquiry. While Small's insistence on sociology as an empirical discipline led to academic respectability and survival, his fusion of sociology and ethics was not historically viable.

The book is a contribution toward making accessible Small's thought and writings. The selections from Small include three letters, one autobiographical, the others proposing to President Harper of Chicago the need for statistics courses in sociology and the establishment of a journal of sociology; the chapter from his *General Sociology* entitled "Interests"; and two addresses, "The Social Value of the Academic Career" (1906) and "Some Researches into Research" (1924). Though relevant to this book, Small's essay "The Significance of Sociology for Ethics" (*Decennial Publications of the University of Chicago,* vol. 4 [Chicago: University of Chicago Press, 1903]) is not included. In addition to the primary read-

ings, Dibble piles lengthy quotation (from archival and other sources) on lengthy quotation throughout his text, stating that they are necessary to give the flavor and substance of Small's thought. However, more is quoted than necessary to attune the reader to Small's style or to sustain the argument of the book. In general, the book seems more complicated than it need be. Dibble agrees with Harry Elmer Barnes that Small's lasting influence on sociology came through his work in institutionalizing it in the United States; he could have developed further this aspect of Small's career.

Adam Smith's Sociological Economics. By David A. Riesman. New York: Barnes & Noble, 1976. Pp. 274. $21.50.

T. D. Campbell

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The bicentenary of the publication of Adam Smith's *Wealth of Nations* has been celebrated vigorously enough to justify the claim that there is a wide and resurgent interest in Smith's economic theory. This assures a welcome for any new study of Smith's work, particularly one which seeks to place his famous economic doctrines in the wider context of his social and political theory. We already have one major recent book covering the whole range of Smith's economics (S. Hollander, *The Economics of Adam Smith* [London, 1973]) and Riesman does not seek to emulate the comprehensiveness of that important work. Instead he sets out, as the title suggests, to demonstrate the sociological nature of Smith's economics in contrast to what he takes to be the standard overspecialized approach of present-day economists. By this he means that Smith's work was interdisciplinary and historical and—more controversially—that Smith was an economic determinist. The author makes it clear that he regards these characteristics as neglected virtues in economic theory and gives the impression that one aim of the book is to enlist the authority of the founder of economics on behalf of a more adequate, because more sociological, approach to that science.

The method of the book is to build up a picture of Smith's views by means of multiple quotations from and summaries of his writings as they relate, first to his methodology, and then to themes such as conduct and character, consumer behavior, social class, and the state. Although the quotations, often taken from very different contexts, are piled one on another, the result is not as unreadable as it sounds and the reader is given a very full and arresting picture of Smith's views on a variety of topics. There is, though, a lack of analytical backbone and critical discussion and sometimes the essentials of Smith's moral and economic systems get lost in a mass of detail. Nor is this a book for the historian of ideas who is interested in Smith's place in a developing intellectual environment and the significance of his conceptual scheme for his own time.

Many anachronistic comparisons, which may be of help to the general reader, will annoy the historical specialists.

Nevertheless this book will further the ongoing task of combatting the erroneous practice of reading back into Smith the narrow economic individualism of his more specialized successors, and sociologists in particular may find Riesman's attempt to present Smith in the tradition of Durkheim and Weber illuminating, although few of the comparisons with these authors are developed very far. There are, however, some distortions and omissions in the generally sound exposition of Smith's economic theory to which the reader should be alerted.

Riesman, correctly I think, looks to Smith's posthumous *Essays on Philosophical Subjects* (1795), especially "The History of Astronomy," for evidence about Smith's methodology. In a rather long and rambling chapter he brings out the Newtonian ideals which Smith set himself and stresses the psychological or aesthetic criteria of scientific truth (coherence, simplicity, and familiarity), which Smith used to explain scientific development. But the stress which Smith placed on the need to accommodate and predict detailed observations gets rather lost. There is also a tendency to overemphasize the extent to which Smith accepted customs as the overriding criterion in aesthetics, and hence in science also, and to underemphasize the place of final causation in his scheme, particularly as it is outlined in the *Moral Sentiments,* although indications are given that final causation is to be interpreted in functionalist terms. The chapter does bring out, however, that Smith had a sophisticated and developed scientific methodology which disposed him to pay attention to the details of human behavior as they form part of a wider system whose internal mechanism it is the task of the philosopher/scientist to exhibit for our admiration.

The account of Smith's moral philosophy is likewise sound but partial. In accordance with the purpose of his book, Riesman stresses that social acceptability is Smith's key to the understanding of moral behavior. He demonstrates how, in Smith's view, the mechanisms of sympathy lead to the creation and maintenance of social norms. For Smith judgments of moral propriety can ultimately be traced to the observer's capacities to imagine himself in the situation of the agent and to see whether his resultant "sympathetic" sentiments accord with those of the agent and to the desire on the part of both observer and agent to attain harmony of sentiments. Riesman does not stress, however, that the observer also seeks to sympathize with those who are affected by the agent's actions and tends to share the gratitude of those who are benefited and the resentment of those who are injured by the agent. It is in fact sympathy with the resentment of injured third parties that is the basis for Smith's explanation of the rules of justice, and Riesman's neglect of this aspect of Smith's theory leads him to underestimate the importance of Smith's analysis of justice as a bridge between his moral and economic theories. The relation between the *Wealth of Nations* and the *Moral Sentiments* is not adequately demonstrated as long as the latter is not seen as explaining the basis of the requirements of justice which form a framework for

economic behavior as it is analyzed in the former. Further, if Riesman had paid more attention to what Smith says about the strong and universal sentiments of resentment felt by those who are injured in certain fundamental ways, he might have balanced his exposition of Smith's treatment of moral diversity with an account of his treatment of the more persistent and unchanging aspects of human behavior. Also, despite the many examples he cites of Smith explaining the relativity of moral codes by reference to differences of economic circumstances, it is an exaggeration to say that, when Smith spoke of the spectator imagining himself in the situation of another person, he was *always* thinking of that situation in economic terms. Indeed, it is part of the burden of Riesman's book that the economic dimension of social situations is not perceived by the observer in isolation from its wider context.

Perhaps the most successful part of the book is its thorough presentation of Smith's account of consumer behavior in sociological rather than narrow economic terms. Men seek wealth for the social status it brings and not simply out of economic necessity. Moreover, what counts as economic necessity is, for Smith, relative to the expectations of particular classes in particular societies. This is well developed over three chapters and makes an impressive case for the social dimension of Smith's economic theory, but not I think an overwhelming argument for the assertion that he is a thoroughgoing economic determinist. Indeed much of the evidence presented can be taken as pointing to causal influences going the other way, that is, the influence of religious, military, and moral factors on economic behavior.

Less happy is Riesman's claim that Smith thought the laborer had a subjective sense of exploitation. Much has been made of Smith's anticipations of Marx's view of alienation, and it is true that Smith did point out some of the mental and spiritual disadvantages of a developed division of labor. It is also true that Smith thought that the property of the rich had to be protected against the envy of the poor. But this does not amount either to a Marxian theory of exploitation or to its subjective counterpart, the existence of feelings of alienation of exploited individuals. Indeed it is only by a conceptually sloppy amalgamation of alienation, anomie, and envy that the thesis has any plausibility.

A final chapter on the state gives a fair summary of Smith's less-than-dogmatic hostility toward state intervention in the economy, although in view of all the author and others have said about the social and legal institutions whose existence is presupposed in the *Wealth of Nations,* it is surprising that he does not qualify his assertion that, in Smith's view, a healthy economy "depends on 'human inclinations' rather than 'human institutions' " (p. 196). Indeed, the purpose of much of Smith's work is to demonstrate the origins of the latter in the former.

As a whole this book gives us a fair impression of the wide range of Smith's economic theories and makes a good case for their sociological nature, although not, I think, for his economic determinism. This claim, like others in the book, is not formulated with sufficient analytical rigor to provide a satisfactory basis for the treatment of the issues in question.

But, despite these conceptual deficiencies, Riesman's treatment of Smith's economics is still very much along the right lines.

Blue Collar Community. By William Kornblum. Chicago: University of Chicago Press, 1974. Pp. xvii+260. $9.95.

Ida Susser
Columbia University

In *Blue Collar Community,* William Kornblum attempts to answer questions about working-class America that have been frequently raised but seldom tackled in any systematic way. From the vantage point of a participant observer in a steel community on the South Side of Chicago, he describes local-level politics, the formation and transformation of social groups, the rise of local leaders, and the shifting circles through which people move. Kornblum familiarized himself with ward politics, present-day union politics, and the history of union struggles in the area. A major preoccupation of the research is with the formation and interaction of ethnic groups and in particular the conditions under which ethnicity becomes a significant factor in political action.

During his two years of residence in South Chicago, Kornblum's perspective on working-class behavior underwent several changes. He entered the neighborhood with the purpose of studying immigrant Slavs and spent his initial six months among people of Serbian and Croatian background. But he found it impossible to study a particular ethnic group outside the broader context of different ethnic groups. In the attempt to understand community relations, he now found it necessary to examine the particular industrial setting of the South Side.

Two themes thread through Kornblum's analysis. The first is that ethnicity is an important political tool manipulated in political competition for different ends. Ethnic solidarity is seen as a symbol developed and nurtured by politicians to assure their access to political office. Ethnicity is not, therefore, assumed as a given condition. Thus, for example, the factions within the Mexican group were temporarily united in the pursuit of specific common goals. However, as Kornblum makes clear, support of people from only one ethnic group is not enough to maintain political power. From the start, politicians must form coalitions with other groups and attract support from people with different ethnic backgrounds.

Kornblum's picture differs from the static view of ethnic territories and competition often presented in descriptions of American neighborhoods with well-defined ethnic boundaries. He emphasizes the shifting allegiances and coalitions between groups, as well as the formation and disintegration of these groups.

Kornblum's second major theme revolves around the effect of the structure of the steel mills on the processes of interaction in the communities surrounding them. As a foreman in one of the mills, Kornblum

describes the different job classifications, the work groups formed, and the kind of cooperation required. He traces the history of immigration and settlement and its effect on ethnic divisions within the work force. He describes the opportunities for social interaction in the factory, the knowledge of alternative job possibilities, and how they may help workers to advance. He also suggests that the lack of such opportunities may hinder the black workers from moving beyond the most unstable types of jobs. Moreover, black workers are at a disadvantage because they are forced to live further away from the mills and therefore do not have the neighborhood connections common to members of other ethnic groups.

However, Kornblum does not mention employer discrimination against black workers who applied for the better jobs, nor does he consider the effects of management policy on the ethnic distribution of workers. He documents historical instances where management's stated intention was to engender conflict among different ethnic groups within the plants, in efforts to combat unionism. However, he does not discuss the possibility that present-day ethnic divisions might be related to the employment policies of management.

After the description of the locally defined neighborhoods, the ethnic territories within them, and work organization at the steel mills, Kornblum turns to local-level politics. In the unions, he analyzes the formation of ethnic coalitions as they are affected by the division of labor, territorial segregation, and generational splits. He describes the way the original Slavic political coalitions maintained their power through their strong neighborhood connections. He also documents the slow climb to power of a black union leader who managed finally to win support from sections of both the Slavic and Mexican populations in the plant. He argues that the blacks had to build their own ethnic solidarity, just like any other group, in order to participate in political competition and to form coalitions with the other factions in the plant.

Kornblum illustrates a similar process in the power conflicts between ward politicians. He argues that in both union and ward the necessity to generate wider political support leads to calls for solidarity according to several forms of status.

Kornblum's contribution has its limitations. One problem with his approach to politics is a failure to analyze campaigns and coalitions beyond the apparent statuses being mobilized. He would add substance to his analysis and supply material for future theoretical developments if he were to delineate more carefully who belongs to which group (either ethnic or neighborhood groups), who actually responds to these calls for solidarity, and what are their associated characteristics, such as income, job security, and educational background. Kornblum might have provided a more detailed economic breakdown of voters in different campaigns or in the status groups upon which he relies as an explanation. By examining issues which appear to stir people in each campaign, he could have attempted to correlate voting patterns with the effect of major issues on people with differing ethnic and neighborhood allegiances. However, he provides few examples of such issues.

Kornblum maintains that work in the mills is both a central feature of his South Chicago area and basic to union politics. He might have illuminated the controlling factors for the shifting ethnic and neighborhood allegiances which he documents if he had considered work-related problems as possible issues around which factions and alliances developed. If he had examined such issues as economic hardship, compulsory overtime, or even management-union relations or contract negotiations, he might have found that the ethnic divisions in the work force have contributed to the masking of issues connected with employment through calls for ethnic solidarity.

Kornblum sometimes fails to take the necessary second step. He explains the behavior of political *leaders* in situational terms, as the manipulation of different status identities in the pursuit of power. However, for political *followers* his analysis does not consider material or political interests and he relies on symbols of solidarity, status, and (although he does not use the words) "primordial ties" as explanations for their behavior. For Kornblum there appear to be two types of human beings: the economic and political person who seeks his own advantage no matter what his background or his neighborhood, and the person who simply votes according to traditional territorial and national divisions.

Also, a study of local-level politics should include a discussion of substantive issues and, where possible, the voter breakdown according to those issues, whether or not they become publicly voiced by political leaders; *Blue Collar Community* does not do so. A second aspect fundamental to considerations of local-level politics but not discussed by Kornblum lies in the area of informal political protest. Wildcat strikes, parents organizing for adequate day care and education for their children, and church and other groups forming to prevent changes in the neighborhood and to stop or initiate federal projects have provided the focus of much political activity in the American city, at least in the past two decades. A description of these groups and their interaction with ethnic and territorial solidarity would have provided a more complete view of local-level political processes and the pressures under which they operate. Information about such informal protest groups and an accompanying analysis would have provided a useful contrast to Kornblum's description of the coalitions and alliances vying for formal political office.

In sum, *Blue Collar Community* fills some of the gaps in our understanding of working-class life. It incorporates the bonds developed at work, as well as in the organization of politics, to arrive at a view of ethnicity as socially constructed out of a diverse array of primary-group loyalties. It dispels illusions of the stability of American ethnic groups.

But Kornblum could have gone even further in the analysis of the organization, creation, and reproduction of the social relations which characterize the urban community. His analysis is limited by such loose and unexamined concepts as ethnic and territorial solidarity. These two must be seen as consequences as well as causes. In the last analysis the divisions Kornblum describes cannot be understood without reference to a much broader analysis of the political and economic pressures that

impinge upon and threaten the community itself. It is in the systematic linkage of what happens outside the community—in the steel industry, at union headquarters, in national as well as local politics, and so forth—to what happens within it that Kornblum's work is weakest. Like so many community studies, Kornblum tends to isolate artificially the South Side from the broader social, economic, and political contexts reflected in its particular patterns of organization.

Constructing Social Life: Readings in Behavioral Sociology from the Iowa School. Edited by Carl J. Couch and Robert A. Hintz, Jr. Champaign, Ill.: Stipes Publishing Co., 1975. Pp. vii+292.

Murray Melbin
Boston University

As we become aware of how complicated and conditional social interaction is, we shift our approach to the subject. Gone are the global theories, the behavior category lists claiming exhaustiveness, and the simplistic hypothetical links between motives or norms and actions. The focus is narrower now. Researchers train their intellectual energies on a hopefully manageable portion of the whole. This book of essays by colleagues at the University of Iowa begins with the question, "How do two or more persons organize their behavior to do something together?" That powerful question heralds a sizable task. So it is made a background theme, and many of the authors choose to focus on only the initial stages, the formative moments, the openings, of concerted action.

The first part of the answer to the question is that concerted behavior happens directly through sensory channels such as seeing, hearing, and touching, through such media as written messages, clocks, and telephone communication, and through the assignment of representatives—that is, spokesmen—to deal with the social environment. A number of the essays in this collection are expository surveys of a selected interactive mode. There is one paper on touch, another on the impersonal synchronizing power of clocks, and several dealing with the matter of selecting a representative to act on behalf of the group. The second part of the answer to the question is that a *structured process* emerges in social interaction. Here the analysis of the formative stage of interpersonal action results in a sequence of phases being identified, and these elements of openings are tagged with an array of labels, some newly coined. The terms include: "accountable relationships," "bilateral accountability," "coordinated behavior," "congruent functional identities," "coacknowledged prescriptions," "committed relationships," "copresent social context," "interactive context," "internal solidarity," "solidarity responsiveness," "shared focus," and "reciprocal attention." Their kinship with ideas of symbolic interaction is apparent, but explicit definitions of these terms are scarce in

the text. The reader is left to puzzle out which concepts from one essay match different concepts in another essay.

The theoretical background and approach used in these papers rest on basic ideas of G. H. Mead, Durkheim, and Goffman. For example, the authors are consistent in following the strategy of formulating their descriptions of social interaction to account for simultaneous behavior by two or more persons, rather than referring to a ping-pong type of social sequence. The theme of a probabilistic future in the human mind is also part of symbolic interaction theory. It includes Mead's idea that people anticipate a generalized reaction from others to their conduct and extends to how relationships develop via awareness that individuals will act in unison in a future encounter. The past is the basis for projecting people's future intentions and plans, and "the construction of a highly responsive relationship is often dependent upon prior construction of and commitment to a shared future which provides a secure context for mutuality" (p. 87). Thus the role of probability thinking in social conduct is brought to the fore. It is an aspect that deserves more attention than it has received. For ordinary persons have shown themselves to be more probabilistic than we usually propose (except in ideas about information processing) in our concepts of human behavior.

A number of propositions are set forth about the conditions under which coordinated social action will occur. All refer to the fine-grained phases of the onset of interaction. Does it matter? Does it add knowledge? The significance of the distinctions is that *different* forms of breakdown would follow from faults in different phases of this progression. If the various conditions for mutuality do not differ in their consequences and if those unfulfilled conditions all result in the same types of misunderstandings, missed achievements, or arguments between the parties involved, then the phases need not be distinguished. We could specify the conditions for one broad phase, the opening or formative one, and accomplish as much in our theoretical explanation. The propositions are not converted into testable form in the book. In another instance, the idea that people usually stick with the representatives they choose (chap. 11) is studied, but the manipulation of the independent variable went awry and the findings were difficult to interpret. The hypothesis that "the greater the bondedness [between representative and constituents] the greater the likelihood of success [of the representative] in negotiation" (p. 195) is not tested. Sincere, empathic representation may plausibly explain high motivation to succeed in a negotiation, but will it carry weight in a predictive scheme that also includes being wily or skillful in collective bargaining for a union or in disarmament talks for a nation? There is enough research to show that the behaviorist and the symbolic interactionist aspects of Mead's ideas can be studied empirically. More of this ought to be forthcoming from the Iowa school.

The editors invite the reader to accept this collection of readings as an exploratory statement and to join with them in a constructive discussion of the issues. It is unfortunate that the authors of these pieces were not

encouraged to provide more linking discussion. Many statements are made and left hanging without disclosure of the reasoning behind them and without further development. In essay after essay there are preliminary exploratory assertions, often without formal statements, explicit hypotheses, concrete predictions, or empirical tests. The total effect is an extended, unsystematic exposition, with the reader left to sort and organize and formulate a theory. This school, as well as others, should identify how its analyses differ from other approaches and show coherently where its work could lead.

The Labelling of Deviance: Evaluating a Perspective. Edited by Walter R. Gove. New York: Halsted Press, 1975. Pp. 313. $15.00.

John Hagan
Indiana University

For more than a decade now, many (probably most) sociologists of deviance have talked to one another, to their students, and to government officials as if they believed that (1) deviant labels were applied independently of the behaviors or acts of those labeled, and that (2) the labeling then caused such behaviors or acts to occur repeatedly. These propositions were accepted primarily on faith and remained without confirmation or falsification for a period long enough to embarrass some but enthrall others. It was a period of "creativity and growth," and a new literature based on the "societal response to deviance" emerged. But all eras end, and this one certainly showed fatal signs at the Third Vanderbilt Sociology Conference, the proceedings of which were organized and edited in book form by Gove. Most significantly, this volume demonstrates that "it is the behavior or condition of the person that is the *critical* factor in causing someone to be labelled a deviant" and "labelling is *not* the major cause of the development of stabilized deviant behavior" (p. 295, emphasis added). These conclusions are affirmed by nine experts in eight areas: alcoholism (Lee Robins), mental illness (Walter Gove), mental retardation (Robert Gordon), physical disability (Richard Smith), crime (Charles Tittle), juvenile delinquency (Travis Hirschi), heroin addiction (William McAuliffe), and sexual deviance (Edward Sagarin and Robert Kelly).

The findings themselves produce interesting responses. Hirschi explains the disparity between the popular beliefs and the data by noting that "labelling theory has prospered in an atmosphere of contempt for the results of careful research. Little wonder that such research is now returning the compliment!" (p. 199). Not surprisingly, John Kitsuse and Edwin Schur react quite differently when asked to respond to the proceedings. Their argument, predictably, is that the assessors pursued the wrong questions. They acknowledge that ambiguities in classic statements of the labeling viewpoint may have made such questions *seem* central but main-

tain that the real issue is how norms emerge in the first place (pp. 276, 290). Schur infers from this that "the central focus, then, is on *characterizations of behavior* rather than on behavior itself" (p.287). This devaluation of the behavioral variable may seem disconcerting to those readers of Schur's works advocating "radical nonintervention," legalized heroin distribution, and the decriminalization of "victimless crimes." Were the behavioral consequences of these policy recommendations of only secondary interest to this labeling theorist-cum-advocate?

This reservation aside, I would like to articulate a set of research priorities that proceed in part from the observations of Kitsuse, Schur, *and* Hirschi. In brief, I believe that the data collected in Gove's volume contain important leads for reorganizing our research in order to investigate the question posed by Schur and Kitsuse—namely, how norms emerge—and, Schur's injunction notwithstanding, what the behavioral consequences of this emergence are. The goal is to remedy the empirical deficiency that Hirschi emphasizes—in other words, to give labeling theory the inductive benefit of the data we have in hand. Let us return, then, to the data.

A fascinating finding of this volume is that social resources do have some, albeit not a critical, influence on the application of labels; when this influence operates, the less punitive labels are applied differentially to more "resourceful" persons, while the more punitive ones are applied differentially to less "resourceful" persons. For example, when contaminating behavioral variables are controlled, there is limited evidence that criminal (p. 170) and delinquent (p. 194) labels are applied somewhat more frequently to the socially disadvantaged, whereas psychiatric (p. 57) and physical disability (p. 151) labels are reserved somewhat more frequently for those who take advantage of their social resources. At this point, it is important to emphasize that social advantage implies not only access to resources, but also the ability and motivation to use them. It is the essence of social class that there is a differential learning of the ways and reasons to act to one's advantage. Thus, Gove (p. 53) reports evidence from the mental health literature to support the proposition that "the upper classes . . . seek treatment more quickly and for a wider range of behavior than persons from the lower classes." Similarly, Smith (p. 152) finds that the socially advantaged have better access to physical disability labels, in part because "clients who do not submit their own evidence and therefore must rely on agency facilities to document evidence of disability . . . are more likely to be initially rejected." The point is that some labels, under some conditions, become advantageous, and the ability to take advantage of these conditions varies by social class.

The interesting issue is how one learns to evaluate the relative utilities and liabilities of various labels. Apparently this learning process is socially conditioned. The researchable problems, then, are how one learns that some normative deviations carry more punitive sanctions than others, and how the learning of these normative lessons is linked to class background.

My suggestion is that the learning of these lessons clearly begins in childhood and should be studied as such. My hypothesis is that children learn, with variation by social class, that some forms of deviation are punishable, others treatable. Children also learn, again with variation by social class, that patterns of deviation that go untreated can become punishable in their advanced form (e.g., paranoia become homicide)—and thus the urgency of early response.

On the other hand, to focus on the decision makers in agencies of social control, as most of us have over the last decade, is to miss the earlier link between the social-control process and the class-connected lessons of socialization. We live in a society where predatory behaviors and punishing labels are directly related. This lesson is taught from childhood, and our social-control agencies are structured on this principle. For example, although some social workers and sociologists may insist on confusing the issue, mental hospitals do intend to "treat," while prisons intend to "punish." What I am suggesting is that the crucial link between socialization and control networks is that advantaged children learn that some forms of deviance are more acceptable than others (e.g., mental illness vs. crime and delinquency). In addition, they learn that early response to such deviations promises treatment rather than punishment (e.g., approaching a mental institution early may avoid imprisonment later). It is in this context that class-related differences in the application of labels become more meaningful and the very existence of different types of deviance and agencies of social control becomes understandable.

Summarizing, the behavioral differences that Gove and his assessors emphasize are real, substantial, and sometimes unalterable (e.g., physical disabilities and mental retardation). However, this does not discount the fact that many of these behaviors are conditioned in childhood, often through the medium of class-linked experiences, and that the learning of norms and the application of labels can be studied from this viewpoint. If we understood better both how norms are learned in the first place and how class influences the process, we could begin to understand how agencies of social control serve to ratify and perpetuate this process. Gove's book can return us to these basic issues.

Women and Crime. By Rita James Simon. Lexington, Mass.: Lexington Books, 1975. Pp. xvi+127. $12.50.

Otto Pollak

University of Pennsylvania

Rita James Simon's book deals with a question which is both relevant and timely, namely, the impact of the women's movement on female crime. It is in essence a book on the impact of positive social change on deviance. Since opportunities to commit crimes against property can be assumed to have increased for women as their participation in the labor force has

increased and as positions which make fraud and embezzlement possible have become increasingly available to them, the rate of commission of such crimes by women might be expected to have risen. The book represents a statistical analysis of comparative arrest data from uniform crime reports for the years 1953–72 for violent crimes and property crimes. The data show that the arrests of men for participation in violent offenses have increased over the two decades almost four times as much as the arrests of women. For property offenses, however, the arrests of women have increased threefold compared with those of men. These figures would suggest that increased participation in the labor force has been accompanied by an increase in arrests of women for property offenses, not necessarily for participation in violent crimes.

The author points out, however, that the higher-status positions in traditionally male occupations have not yet been attained in significant numbers by women and that before women's participation in crime can be expected to match their representation in society, women must have corresponding opportunities to commit crimes.

The author also presents interesting evidence about differentials between women and men in convictions, imprisonment, and the use of parole. These data show that there is still a protective bias toward women who commit crimes. The essence of Simon's findings in this respect can be best presented in her words: "In sum, the picture today about how women fare at various stages in the criminal justice system is that although one in 6.5 arrests are women and one in nine convictions are women, only about one in thirty of those sentenced to prison are women. These ratios have not changed drastically over the past two decades, even though these years have seen a women's movement develop and expand, and an increase in the proportion of women working full time outside their homes" (p. 108).

I see the essential contribution of this book in its insistence on statistical analysis in the face of political rhetoric. It is also important to note that the author clears the record regarding the official position of the women's movement on the possibility of an increase of female crime as a result of increasing opportunities to commit such crimes. She points out that the leaders of the movement have made strenuous efforts not to take a utopian stance in this respect and have expressed willingness to pay the price of increased female crime for increased female opportunity. In my opinion, the book has to be commended as a valuable pioneering contribution to the study of the social costs of social improvement, although the author is careful to point out that the evidence is not yet sufficiently available.

The only negative note which I feel compelled to make is that the book deserves to have been better edited than it apparently was. Cezare Lombroso is misspelled "Lombrozo," and neither he nor Sigmund Freud is to be found among the references, although they are mentioned in the review of the literature.

The Prison: Policy and Practice. By Gordon Hawkins. Chicago: University of Chicago Press, 1976. Pp. xi+217. $10.95.

Changing Prisons. By J. E. Hall Williams. London: Peter Owen, Ltd., 1975. Pp. ix+210. $14.95.

Work Furlough and the County Jail. By Alvin Rudoff. Springfield, Ill.: Charles C. Thomas, 1975. Pp. xiii+198. $12.50.

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These three books deal with policy questions and empirical issues concerning imprisonment. They appear at a time when the evident failure of rehabilitation programs to reduce recidivism has led many commentators and scholars to reassess the efficacy of deterrence and the moral functions of punishment. It is to the credit of each of these authors that he has avoided becoming embroiled in a new ideological debate and has seriously attempted to take stock of where we are by examining the principles and practice of contemporary imprisonment.

Of the three books Gordon Hawkins's is by far the most important. Although, unlike J. E. Williams and Alvin Rudoff, he offers no new data, his exhaustive and skillful review of the empirical research (most of it sociological), commission reports, and philosophical jurisprudence on the topic of imprisonment is the best currently available. *The Prison* will be a very useful reference for sociologists who work in the field, and perhaps even more important for nonprison specialists who wish to catch up with the current state of scholarship on the subject.

Hawkins's chapter, "The Effects of Imprisonment," constitutes a very lucid and analytical treatment of the by now somewhat tiresome sociological debate on the "origins" of the prisoner culture and social system. Clemmer's theory of "prisonization" is very closely examined in the light ot three distinct lines of empirical research. Hawkins concludes that "it should be some consolation that the belief that all who enter prison are ineluctably doomed to deterioration proves, on examination, to rest on no more rational basis than the antithetical idea that, if only we knew how, panacean programs could be devised which would transform all offenders into model citizens" (p. 80).

Two of Hawkins's other chapters offer a critique of the literature on prison guards ("The Other Prisoners") and prison labor ("Neither Slavery nor Involuntary Servitude"), topics too often either neglected or treated superficially in prison studies. With respect to the guards, Hawkins points out that they have frequently served as convenient scapegoats for larger failings in penal policy. Drawing heavily on the seminal work of Daniel Glaser, he also argues that the typical description of the prisoners and guards as locked in daily combat is excessively melodramatic. The scholarship on the subject, according to Hawkins, indicates great variation in the attitudes and roles among prison officers and suggests that the failure to link empirical study to policies of recruitment and training has

undermined the possibility of establishing humane prisons where inmates can find encouragement and support if they are looking for them. While more impressed than Hawkins by the strength of organizational imperatives as determinants of the guard's role and ideology, I found the meticulous examination of the American and British scholarship on the subject most valuable.

In his discussion of work in prison Hawkins points out the many confused and sometimes contradictory justifications for prison industries and vocational training. Is work to be considered a punishment or is it to be thought of as a privilege? Is the object of prison labor in a rehabilitation program training for work or training by work? No doubt the way prison work is defined is determined by the prisoner's integration into the larger society. Here I found most interesting the discussion of prison labor policies of the other Western democracies which seem to have gone much further in extending to the prisoner the rights of citizenship.

Hawkins's book is more than a road map of the contemporary prison scene because the book is organized around key theoretical and policy issues. *Changing Prisons* by J. E. Hall Williams is more like the road map, albeit a detailed, accurate, and useful one, of the English terrain. He wrote the book at the invitation of the Contemporary Issues Series, after concluding "that the time seemed ripe for an objective and detached survey of the developments which were taking place in the Institutions under the control of the Prison Department" (p. 1). The book deals with the key problems and policy choices faced by the British prison system since 1970 and draws extensively from commission reports, white papers, and internal documents with which I, for one, was not previously familiar. The very careful and scholarly analysis of the current state of things in British prisons makes this book eminently worth reading for those with very specialized interests in penology, but of less overall importance than Hawkins's monograph.

What I found most important in *Changing Prisons* was the author's sensitivity to organizational and administrative variables which are properties of the whole prison system rather than of the individual prison. For example, Williams several times stresses the way in which population pressures in the British system have, in recent years, caused administrative shock waves throughout the system. To take another example, the abolition of the death penalty and the increased length of sentences have produced a plethora of long-term prisoners who do not fit easily into the existing web of British penal institutions. Likewise, intensified violence in the early 1970s has raised the question for the penal administrators as to whether the "trouble-makers" should be concentrated in a single maximum-security facility (as the prison officers' union demands) or dispersed throughout the system. I know of no other work on prisons which is so attentive to the fact that prisons do not exist as isolated institutions but are functionally interrelated in a system which makes fateful decisions about distributing prisoners, staff, and resources. Written from this perspective, the chapters on classification, security, and prison labor are particularly interesting.

One criticism of the Williams book is that it reflects something of an insider's bias. It was written after the author had completed a period of service on the Parole Board for England and Wales. At several places I felt that sympathy for the administrators detracted from the objectivity of the analysis. For example, the author tells us that "no one can fault the Prison Department for not trying to adapt to the needs of the present day conditions . . . if there is room for criticism it should be directed elsewhere, to the inflexibility of public attitudes, to the courts . . ." (p. 193). In another instance Williams dismisses out of hand the demands which were submitted by a nascent prisoners' union. The demands—free access to the prisons for the press, for example—did not seem to me so clearly impractical and unacceptable as to justify the summary rejection.

Unlike Hawkins or Williams, Rudoff does not attempt a broad critique of penal policy. In fact, in *Work Furlough and the County Jail* he purports to be evaluating a work-furlough program run for misdemeanants and minor felons out of the Santa Clara County Jail's minimum-security unit. I say "purports" because much of the book deals with subject matter unrelated to the work-furlough program—the staff of the minimum-security unit and the maximum-security female prisoners, for example. In an effort to include as many of the results of the survey as possible, a great deal of extraneous and irrelevant material is presented.

Rudoff's unusual conclusion that the jail's work-release program reduces recidivism is very dubious. To support this assertion of the program's success he points out that for both furloughees and nonfurloughees the number of arrests in the 18 months after release is fewer than the number for the 18 months prior to commitment. While a few of his other "measures" are slightly more adequate, none of them support the burden of the argument. The fact that those participating in the work-furlough program were preselected precisely because they were considered to be better risks undermines the validity of an experimental design even when the correct indicators are used.

The author's conclusion that the work-release program is also a success because of the several hundred thousand dollars contributed over a two-year period by the furloughees to the county and to their families is more interesting than the recidivism point, and I would have liked more discussion of it. Despite the fact that a considerable amount of money has been earned by jail inmates participating in the program, Rudoff is critical of the program's emphasis on placement without regard to the quality of the job. He believes that experience with dead-end jobs that offer no potential for upward occupational mobility accounts for the fact that the work furloughees' attitudes deteriorated more during the work-furlough period than did the attitudes of the nonfurloughees.

To the author's credit is a very frank and self-critical discussion of some of the problems involved in carrying out the research. The relationship between the student research assistants, who were stationed at the minimum-security unit, and the staff degenerated to the point where the students were posting antagonistic drawings and cartoons on their office door. Rudoff also notes that the presence of the research team had the

effect of increasing the cohesion between the institution's rehabilitation and custody personnel. At the very least it would seem to me that this would caution us to look at the staff's survey responses with some care. More important, it calls attention to the need to address the question of how value-free and unobtrusive research can be conducted in such highly politically charged settings as penal institutions.

Thinking about Crime. By James Q. Wilson. New York: Basic Books, 1975. Pp. xix+231. $10.00.

Jack P. Gibbs
University of Arizona

Large portions of this book are revisions of earlier publications, and criminologists can now react to the challenge posed by a more complete statement of James Q. Wilson's ideas. The challenge reduces to a negative view of criminological theories—they are virtually worthless in contemplating and formulating policies for controlling crime. This review focuses on that negative view not just because of its import for sociology but also because it is Wilson's *only* major theme in a book that treats such widely diverse subjects as the politics of crime, the police, heroin, courts and criminal corrections, and the death penalty.

Wilson's negative view of criminological theories stems from a very specific premise: criminological theories identify alleged causal variables that cannot be manipulated readily (if at all) to realize policy goals. Stating the argument in Wilson's own terms: "Ultimate causes cannot be the object of policy efforts precisely because, being ultimate, they cannot be changed" (p. 50).

The notion of ultimate causes obfuscates Wilson's argument, which actually requires no reference to causation. Any policy deliberation that touches on empirical matters reduces to a question in this form: If such-and-such is done, what will happen? An answer is a prediction, and sophisticated policymakers are interested in predictions rather than the metaphysics of causation. Of course, theories are always conditional, that is, some condition must obtain before a prediction follows from a theory. Viewed that way, Wilson argues that criminological theories identify alleged antecedent correlates of crime that cannot be "easily and deliberately altered."

Wilson treats Sutherland's theory and the Cloward-Ohlin theory as exemplifications, but he offers a simpler illustration: "Criminologists have shown beyond doubt that men commit more crimes than women and younger men more (of certain kinds) than older ones. It is a theoretically important and scientifically correct observation. Yet it means little for policy makers concerned with crime prevention, since men cannot be changed into women or made to skip over the adolescent years" (p. 50). The argument has more implications for criminological theory than Wilson recognizes. If policymakers cannot manipulate all conditions, the

only theories that have policy implications are those that identify *sufficient* conditions for conformity to criminal laws. In any case, the manipulability of an alleged sufficient condition is crucial, and whether the condition is also necessary becomes irrelevant. An emphasis on sufficient conditions is alien to what are now conventional sociological research methods, meaning that the necessary/sufficient distinction is not central in correlation analysis, causal models, or path analysis. For that matter, the growing indifference of sociologists to the amount of variance explained by a model is not conducive to an impact on policy. Yet Wilson appears discontent not so much with the small amount of variance explained as with the focus of criminological theorists and researchers on independent variables that are not readily subject to manipulation.

Wilson's argument leads him to suggest that criminologists have slighted research on the deterrence doctrine. Surely there is evidence that criminologists dismissed the doctrine prematurely, and Wilson correctly points out that the subject received scant attention from the Task Force on the Assessment of Crime. On the other hand, Wilson's numerous observations on programs to increase the actual or perceived risk of punishment (including apprehension) are hardly compelling evidence of deterrence. For that matter, Wilson appears insensitive to the awful evidential problems in assessing the deterrence doctrine and to the limitations of recent research (even the distinction between deterrence and other preventive mechanisms of punishment goes unemphasized). Yet Wilson is actually concerned with the preventive effects of legal reactions to crime, not just deterrence; and he ends by emphasizing the possible importance of incapacitation, but limited evidence precludes a forceful defense of that emphasis. However, Wilson does not pretend to be an authority on deterrence; hence, his admonishments reduce to a reasonable question: Since the deterrence doctrine has real policy implications, why not devote more attention to it?

If criminologists reject Wilson's allegations, their counterarguments will be strained. Modifying attitudes or values, altering interactional patterns, and expanding the legitimate opportunities are logically possible ways of applying criminological theories, but selling their implementation to interest groups is another matter. Indeed, Wilson could have extended his argument by emphasizing that application of a criminological theory requires another theory to manipulate independent variables. Consider an illustrative question: What is the empirical relation between racial-ethnic integration of schools and exposure of minority juveniles to definitions that favor conformity to laws? Such a question would have to be answered in order to apply the differential association theory, and the Great Society's crime-prevention programs came to grief possibly because they are not based on defensible theories of application. Wilson alludes to that possibility in making observations on diverse facets of the Great Society (e.g., poverty, race relations), facets so diverse that his insights and arguments cannot be summarized here.

Wilson is sensitive to the importance of pure theory and research, so he argues only that criminologists are inordinately preoccupied with the

"pure." Yet he fails to see that the deterrence doctrine cannot be assessed thoroughly without at least statistical controls for relevant *extralegal* conditions (e.g., possibly unemployment); but the identification of those conditions is a matter for theory, even though the conditions may not be subject to manipulation.

Beyond making vague allusions to ideology, Wilson is silent as to why criminologists (especially those in sociology) are ostensibly reluctant to engage in work that could have policy import. The reluctance is not irrational, and the reasons for it transcend the fear that work on pure theory will cease. Wilson simply does not confront the dilemmas of engaging in policy-oriented theory and research, one being that such engagement may contribute to the preservation of the political and economic status quo. Yet his book should prompt sociologists with criminological interests to contemplate the possibility that someday they will find themselves "on the outside looking in," condemned as irrelevant by those (including deans) who command resources. Wilson notes that sociologists have given way to economists in the renewal of deterrence research, and that is a testimonial to the economist's greater sensitivity to policy issues. Sociologists may elect to refrain from what they consider to be "whoring," but it is indeed an old profession; and, unfortunately, the decision to refrain is something more than a moral choice.

Crime, Punishment, and Deterrence. By Jack P. Gibbs. New York: Elsevier Scientific Publishing Co., 1975. Pp. xi+258. $12.95.

Charles R. Tittle

Florida Atlantic University

Jack Gibbs begins his book with a narrow definition of deterrence that focuses on individual inhibition in response to fear of punishment. He then tries to show that it is impossible to determine if punishment or the threats of punishment actually deter because (1) cognition cannot be directly observed, (2) linking internal characteristics to manifest behavior is at best inferential, and (3) the available data are inappropriate. Most of the book is therefore devoted to demonstrating the difficulties and futility of deterrence research. Gibbs denies the relevance of most current work that claims to address the deterrence question and criticizes its general inadequacy. Having proven that it is impossible to learn anything about "deterrence," he then suggests we study instead the "preventive effects" of punishment. Several approaches to that end are outlined, and the reader is introduced to some refined measures of various properties of punishment and prevention, along with several provocative ideas for future research.

Perhaps the strongest part of the book is the third chapter, which argues that punishment can lead to a reduction in crime through at least nine mechanisms besides deterrence. These include widely recognized factors such as incapacitation as well as lesser-known ones like reforma-

tion of offenders and prevention of retributive crimes that occur when victims and relatives do not obtain a sense of revenge through official punishment. Thus he convinces the reader that even if it could be shown experimentally that punishment reduces crime one would have to rule out other interpretations before one could draw the conclusion that deterrence causes the reduction. Moreover, Gibbs repeatedly makes the point that meaningful research on the deterrence question must focus on how punishment or punishment threats are perceived and must show the relationship between such perceptions and actual characteristics of punishment.

There is great merit in this work, and nobody interested in the deterrence issue or in deviance, law, or criminology can afford to ignore it. Yet there are, in my opinion, some glaring defects. In the first place, much of the argument revolves around a semantic distinction concerning the definition of "deterrence." I believe the author would have made a greater contribution and could have spared us much confusion if he had simply addressed the issue of what effect threats of sanctions have on behavior (no matter what it is called). Within that framework, he could have developed the idea of different kinds of effects that might be produced by fear as well as by other mechanisms. The weakness of the definitional straw man is all too obvious when Gibbs finally recognizes the fruitfulness of studying "preventive effects."

Second, I believe many will be misled into stifling pessimism because Gibbs too easily dismisses possible solutions to evidential problems. For example, he gives the impression that when one uses official statistics one might as well not try to separate deterrence from other variables. Yet already several creative efforts to estimate incapacitating effects independent of deterrence have produced promising results. There is no a priori reason to rule out such solutions for other problems. Likewise, Gibbs assumes that investigators cannot take into account etiological variables in tests of hypotheses. But again he seems to discount prematurely the possibility of statistical control. Similarly, while recognizing perception studies as critical, he displays little vision in thinking about their potentiality. His judgments of impossibility appear to me to be based on a narrow methodological perspective. Actually, Gibbs seems a little uncomfortable with his own stance, since there are numerous examples of ambivalence. For instance, he begins the first chapter by saying that his book is a "continuous denial of immediate prospects for satisfactory answers" (p. 1). Yet he expresses the belief that social scientists have dismissed the deterrence doctrine prematurely. On one hand he calls for a "moratorium on conventional research" (p. 2), but on the other he admonishes us to do more "exploratory work" (p. x). And despite his pessimism, he cannot resist advancing astute practical proposals of his own for solving research problems.

Finally, some of the arguments are tortured and unnecessarily complex, leading only to insignificant points, while other ideas are glibly rejected with hardly any argument. Nevertheless I have to recommend this as an exceptionally thorough and important addition to the deter-

rence literature. It has all the characteristics one has come to expect of Gibbs's work. It subjects the deterrence question to withering scrutiny, involving minutely precise conceptualizations and seemingly endless typologies that are enlightening although tedious. It addresses most issues with airtight logic and underscores the necessity for theory. It is chock full of important insights that will be appreciated especially by the involved researcher. And it even systematically considers policy implications. Thus acquaintance with this book will force the reader to be acutely aware of the need for care if he is contemplating research on the behavioral effect of sanctions.

Justice by Consent: Plea Bargains in the American Courthouse. By Arthur Rosett and Donald R. Cressey. Philadelphia: J. B. Lippincott Co., 1976. Pp. xvi+227. $10.00 (cloth); $3.95 (paper).

Gregg Barak

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A testimonial to this recent book by Arthur Rosett and Donald R. Cressey unwittingly exposes the fundamental weakness and/or contradiction underlying their work. On the back cover, the testimonial states that the "particular genius" of *Justice by Consent* "is its ability to integrate in an intelligent manner the theoretical foundations of our criminal justice system with the dramatic differences of its routine operations and to do so in a way that . . . does no violence to the profound concepts [e.g., due process, rule of law, etc.] involved." Realizing that the adversary model (determining guilt or innocence) of criminal justice adjudication has already proven itself bankrupt in practice, Rosett and Cressey, nevertheless, cling to the model by emphasizing the "courthouse's" function in distributing pain with fairness.

The intended effect of their thesis is to make adjusted or negotiated punishments (guilty pleas) both rationally and morally palatable, leaving the procedural as well as the substantive sides of the criminal law unchallenged. The authors recognize the conflict inherent in their twofold aim. On the one hand there is the issue of expedience in processing the assembly line of criminal defendants; on the other, there is the general interest in maximizing those aspects of the formal administration of criminal justice that legitimate it in democratic societies. They argue that strict adherence to the latter objective is neither utilitarian nor just in accomplishing the former. Hence, by implication, due process and jury trials have little utility.

The authors maintain that since those individuals customarily prosecuted (those cases sifted out of the total number of arrests) are in fact guilty of the crimes they are charged with the proper role of the court becomes one of deciding what to do with criminals. In other words, as the issue of legal guilt becomes a moot point, trials are wasteful at worst and perfunctory at best. Moreover, the litigation involved is both expensive

and time consuming, especially when the criminal dockets continue to expand at a rate disproportionate to the available resources. Conversely, what is utilitarian and, therefore, rational is plea bargaining, or what Rosett and Cressey have more accurately described as guilty plea negotiations. Their argument maintains that these negotiations act to delicately balance the formal needs of the criminal law with the individual needs of the offender and the victim and with the needs of the larger community. Finally, utility and rationality are linked with what is "just," since the authors argue that "strict compliance with severe and rigid laws abstractly resembles justice, but it is not just in effect." Instead they suggest that just effects can be determined and achieved better through professional experts—prosecutors, defense attorneys, and judges—engaged daily in "doing justice." Despite the overwhelming pressures on these court functionaries to keep the caseloads moving, to cooperate with the larger bureaucratic concerns of the criminal justice system, and to satisfy their respective organizational interests, Rosett and Cressey insist that professionals are administering "justice by consent" as opposed to "justice by coercion."

In this book the two orientations of bourgeois legality (Rosett) and liberal sociology (Cressey) cooperate to both defend and reconstruct a reality of plea bargaining in the American courthouse. Such a defense of the law in action serves not only to mystify the very *raison d'être* of the criminal law but also to reify the existing social order. In the end, these authors provide us with another contemporary justification for further institutionalizing the present criminal justice system within the political economy of state capitalism: *bargain justice* actually comes closer than *adversary justice* to resolving the inherent conflict between "treating like situations" alike and "giving each person what he deserves."

Rosett and Cressey more than once acknowledge that most practitioners of criminal justice, including police, prosecutors, defense attorneys, judges, or penal authorities, rarely, if ever, ask why there is a system of criminal justice or what purpose the criminal law serves. However, this omission on the part of criminal justice workers is shared by the authors themselves. They present an uncritical and conventional discussion of the notions of retribution, deterrence, isolation, etc., and, as a result, their analysis fails to develop an explanatory framework for understanding "crime," "punishment," or "justice" in capitalist society. At the close of *Justice by Consent* we are simplistically informed that "the problem of preventing crime is principally one of giving more citizens a stake in conformity. The effective strategy is to make crime unthinkable. . . . The courthouse procedures and ceremonies can both stimulate and reinforce people's willingness to conform" (p. 187).

As it has been pointed out elsewhere by Isaac D. Balbus (*The Dialectics of Legal Repression* [New York: Russell Sage, 1973]), both Marx and Weber recognized the inseparableness of formal legal rationality and capitalism in the development of the modern liberal state. Concomitantly, within capitalist society formal legal equality and extreme economic and political inequality exist side by side. Balbus rightly con-

cludes that "the principle of formal legal rationality is a legitimizing principle fraught with tensions and contradictions, a mixed blessing to those with a long-run interest in the preservation of the capitalist order." Rosett and Cressey ignore the problematical issue that the criminal law, by treating economic unequals "equally" (e.g., bail), is fundamentally unjust to begin with, and they proceed to maintain that justice can be found in neither the statutes nor the judicial decisions, nor, more important, in the larger political economy. Instead, they claim that justice can be found in the courthouse itself—in the way judges and lawyers daily distribute punishment and negotiate the contradictions inherent in bourgeois legality.

Justice by Consent, with all its analytical problems, is still a valuable work on the administration of criminal justice. Rosett and Cressey provide the introductory student with, perhaps, the most comprehensive and best descriptive account of plea bargaining in the American courthouse. Unfortunately, their explanation of the purpose of the administration of criminal justice and their inadequately developed theory of crime and punishment generally prevent this book from making a significant contribution to the field.

Making the Papers. By Edie N. Goldenberg. Lexington, Mass.: D. C. Heath & Co., 1975. Pp. xiii+165. $13.00.

Marilyn Lester

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Edie Goldenberg's *Making the Papers* is the most comprehensive contribution to the resurgence of sociological research on access to newspaper coverage and its importance for society and for subgroups therein. No one concerned with the problems of hegemony or the role and functioning of the press as a resource in the society should let this work pass. The book will also aid resource-poor groups which lack knowledge on how to become newsworthy.

In this work, Goldenberg describes the conditions which both promote and inhibit the seeking and gaining of access to media coverage. The book focuses on groups which fail to obtain sustained access to coverage owing to a lack of money, status, and institutional affiliation. These conditions affecting success involve characteristics of (*a*) the groups seeking access, (*b*) the target to be influenced through coverage, (*c*) newspapers themselves, and (*d*) the relationship between resource-poor groups and the papers.

The resource poor, like the resource rich, pursue or sometimes try to avoid media coverage, depending on their needs and purposes at any given time. A group's needs must conform to a newspaper's criteria of newsworthiness. So, for example, when the group goals are seen by the press to deviate from prevailing social norms, or when they represent a

variation on a theme already receiving attention, the potential for access is increased. But, as others have shown, when "deviations" such as threats and demonstrations become repetitive, they lose their newsworthy character. Resource-poor groups have to achieve a delicate balance to be newsworthy, a balance between presenting a deviating image and preserving some measure of legitimacy. That is, access is more likely to accrue to groups which project images of moral superiority over their targets and appear representative of a large population but simultaneously violate routines—a difficult fence to straddle. Goldenberg illustrates varying degrees of success in terms of these and other group characteristics among four resource-poor groups in Boston in the late 1960s and early 1970s.

As if meeting these conditions were not enough, the author discusses features of the press which abet or inhibit access of poorly endowed groups. A resource-poor group will have a better chance of receiving coverage if the newspaper's intended targets are "liberals," if its editorial policies sympathize with such groups, and if the reporting staff for local news is large enough to span both the conventionally newsworthy groups and the poor. Papers which promote advocacy journalism in contrast to traditional objectivity reporting are good prospects for the resource poor. Goldenberg also discusses space, competing stories, structure of deadlines, and layers of hierarchical organization within the news-gathering organization as particulars to take into account should a group have a choice of which papers to approach for coverage. The organization of the reporting staff is critical. Of great importance to resource-poor groups is the newspaper with "issue specialists" and special columnists instead of the traditional beat structure or general assignment reporting. Moreover, Goldenberg outlines characteristics of the reporter which will facilitate his or her success at covering the resource poor.

In the final chapters, the interaction between reporters and resource-poor groups is discussed. For example, contact can be initiated by the group or the reporter. The former is most successful if the group contacts a specific, sympathetic specialist on the news staff as opposed to contacting a general assignment reporter or submitting a press release to the city desk.

Several problem areas detract from the overall effectiveness of the work. First, while the book is a compendium of conditions affecting access to the papers, there is a general lack of elaboration of the topics covered. Goldenberg moves quickly from one condition to the next as if she were preparing an outline rather than a book. This is most clearly manifest in her chapters on media organization (6–8), where the analysis appears facile owing to its shortness and lack of depth. Also, there is a curious absence of reference to Tuchman's recent work on the functioning of the media. I found the book disappointing precisely because its potential for offering a *rich* analysis of the access process went unrealized.

Second, the author never makes it quite clear how she derived the analysis. She begins with a tedious series of definitions of access as if she were going to deduce hypotheses logically and then test them in her field

sites in Boston. It is possible, however, that the concepts and conditions were developed inductively. It is impossible to tell which method is followed because she provides no discussion of her methodology other than a description of her site and the fact that (not how) she conducted interviews.

While Goldenberg attempts to link theory to data, she also falls short in this regard. Her presentations of illustrative empirical materials is so short and capriciously placed as to be practically useless to the non-Bostonian. Thus, there is no evidence in the book of the extensive field research which she claims to have conducted. This omission can have two explanations: either space limitations prevented a thorough integration of theory and research, in which case the publisher needs a lecture, or there was a lack of systematic analysis of the data, with a focus instead on developing a list of formal conditions to be merely illustrated by the data.

Despite these shortcomings, there is now a set of hypothesized conditions which other researchers can explore in more detail in diverse arenas in order to extend our knowledge of the press and how access is gained to it.

The Unseeing Eye: The Myth of Television Power in National Politics. By Thomas E. Patterson and Robert D. McClure. New York: G. P. Putnam's Sons, 1976. Pp. 218. $7.95.

John Meyer
Stanford University

Once more a social research report slays the dragon of media influence. This time the medium is TV, the occasion is the 1972 presidential election, and the study is a three-wave panel following about 650 respondents in a single metropolitan area from September to November in 1972. The data are analyzed in a style antedating *The People's Choice,* what with change scores and bivariate tabulations, but it probably doesn't matter much. The language is popular and shrill: People who watch the network news (in contrast with newspaper readers) do not become more informed than others, as the campaign progresses, about the candidates' stands on the issues. Nor do they acquire different images of the candidates. Even political commercials have little effect on candidate images (which mostly result from, rather than cause, preferences for candidates), though they do appreciably inform people about candidates' public stands on issues.

The authors add in one minor way to the prevailing discussion. By coding systematically the network news shows and political commercials they show *why* the news shows inform people so little (let alone change them). These news shows have astonishingly little information—much less than political commercials—about candidates' positions on issues. They prefer action shots of candidates speaking or shaking hands.

Aside from this concrete observation, the study has little to offer. It is a late and popularized version of an established form, asking the same

question, getting the same answer, and expressing the same surprise. The question is, of course, how the media messages affect the individual in the audience.

Other questions must be asked if we are to understand the effects of television on society. Most of them start from the central fact that the member of the audience is not an individual at all in important respects: he is $1/n$ of a nationwide audience in his own eyes and from the perspective of others, including journalists and political campaigners. If we want to understand the effects, we must understand the impact of this transformed status of the individual on all sorts of relevant other parties. For instance,

1. How do the existence and availability of the nationwide TV medium and audience affect the positions and plans of candidates and campaigns? Are these not stylized in a different set of clichés than would otherwise be the case?
2. Does, for instance, the TV system lower the influence of intermediate social and party organizations over political campaigns and candidates?
3. How does the TV system's existence affect the depiction of the campaign by journalists of all sorts? Do they come to interpret the campaign by, on, and for the tube as more real than the campaign of organization and interaction and issues in the network of social interaction?
4. Specifically, does the existence of a relatively unitary national TV system in this country create a greater emphasis on national purposes and issues, as contrasted with local issues and issue implications, than would otherwise be the case? Are the issues of importance to localities and differentiated subunits lost to the national drama—lost even in the minds of members of these subunits?

Questions such as these can be studied empirically but require something more than another audience survey disparaging media effects. One has to compare systems with different media structures and look at their effects, not only on audiences, but on specialized elites in society. Even effects on audiences may lie more in what people learn is real (i.e., what other people know) than in effects on their own views: in this sense, the media create a pageant.

Such issues as these are hard to suppress in the work at hand. The 1972 campaign featured candidates barely remembered by our authors —McGovern and Nixon—who are seen as serious people struggling to get their issues access to the equally serious populace through the frivolous media. Though this picture is needed by the authors for their moral indictment of the television medium, in retrospect something seems wrong about it. Perhaps the media do more than just ineffectively communicate messages to individuals. Perhaps they affect the messages themselves, and even the senders. Perhaps the whole apparatus of 1972—the candidates, images, issues, and campaigns which now seem so phony—was really a media show much more than it was a real show that got garbled in transmission.

We will not be able to address these issues unless we are able to com-

pare media systems and to compare their effects on more elements of society than simply media audiences.

The Study of Political Commitment. By John DeLamater. Rose Monograph Series. Washington, D.C.: American Sociological Association, 1973. Pp. i+104. $2.75 (ASA members); $5.00 (nonmembers).

James L. Wood
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John DeLamater's *The Study of Political Commitment* could be of interest to political sociologists, students of non-Western nations, and methodologists. In a study of Yugoslavia using survey research data, DeLamater examines the conditions that produce political commitment, which is defined as "the individual's support of his society's political system . . ." (p. 2). The topic and data are most interesting; yet certain difficulties in analysis make this less of a contribution than it might be.

How does DeLamater explain political commitment? His major strategies are to construct a typology of political commitment, which consists of symbolic commitment, normative commitment, ideological commitment, and functional commitment; to distinguish between those who are oriented primarily to their specific state (Croatia, Serbia, Slovenia, or Macedonia), or to the federal republic of Yugoslavia; to make comparisons among the four states of Yugoslavia; and to examine a series of relatively unrelated hypotheses.

Drawing on his typology and the federal-state distinction, DeLamater suggests a number of hypotheses. Some of them are: (1) "Those who are ideologically committed are oriented to the republic"; (2) "Those who are symbolically committed are oriented to the state"; (3) "Functional commitment [i.e., commitment to the rewards of the political system] increases as the individual's contact and experiences with the larger system increase" (pp. 16–17).

DeLamater goes to great lengths to develop empirical measures of the types of political commitment. In fact, he spends too much time in the body of the monograph describing and justifying these measures. We are given an abundance of information of the following type: "In each state, most if not all of the intercorrelations between variables 323–328 are significant at $p < .001$" (p. 29). This information is important in view of his many difficulties of statistical comparison, but it belongs in an appendix instead of in the body of the monograph.

To his credit DeLamater reports many negative findings as well as data supportive of his hypotheses. Some of his results are consistent with his hypotheses, as in Hypothesis I which asserts that ideological commitment will be associated with orientation to the federal republic. The correlations between ideology and federal orientation are supportive of this hypothesis for the overall sample ($r = .23$), Croatia ($r = .37$), and

Macedonia ($r = .24$). In further support of this hypothesis, there is no relationship between ideology and orientation to the state in these instances. Other hypotheses, such as Hypothesis III which associated functional commitment with federal efforts to develop the state, did not receive confirmation.

There is an unresolved conflict throughout the monograph between testing hypotheses and making comparisons among the four states of Yugoslavia. The author's preference appears to favor hypothesis testing, but he often engages in comparisons. For example, he notes that "relative to other states . . . , Slovenia shows low means on all four types of commitment" (p. 44). It is not obvious why this should be the case, but ex post facto, he suggests that the magnitude of commitment may be directly related to economic development. However, in this case as in others, DeLamater's comparative analysis seems based more on his statistical findings than on a thorough historical knowledge of the states of Yugoslavia. Comparative statistical analysis should always be complemented by thorough historical knowledge of the units compared.

DeLamater's data are often more complex than his ideas. In the Macedonian data there is a "unique" ideological-functional intercorrelation which is especially pronounced among the less educated (p. 47). We are not told why this is the case, nor does it easily relate to his hypotheses or comparisons among the four states. It is a "finding," but not much else.

DeLamater's major weakness is that he never develops and tests an integrated theory of political commitment. He might well focus on the impact of economic development on political commitment as a central theoretical issue, but he does not. He presents hypotheses, but they are not related in a hypothetical-deductive sense, and they do not point to the explanation of a common phenomenon—that is, they do not constitute a theory that attempts to explain varying types of political commitment. There is too much data reporting instead of using data to support a theory or develop a theme. The improvement of our knowledge about political commitment is thus primarily limited to how well his data validate his typology and how well specific data support or reject specific hypotheses. Even DeLamater admits that it is unclear whether his conclusions are generalizable to other countries (p. 83).

The author's failure to develop a theory of political commitment has more general implications for the new monographic style of publication used in this study. In addition to the ASA's Arnold and Caroline Rose Monograph Series in Sociology, Sage Publications, Bobbs-Merrill, and General Learning Press publish monographs, which are longer than an article but shorter than a book (often between 50 and 125 pages). Should these monographs be more like books or more like articles? That is, should a general theme be developed, or should more narrow research findings be reported? In view of the limits of DeLamater's study, it is clear that these monographs should—like good books—develop a theme and use the extra space to document it.

Small Groups and Political Rituals in China. By Martin King Whyte. Berkeley: University of California Press, 1974. Pp. 271. $12.50 (cloth); $3.85 (paper).

Paul J. Hiniker
University of Chicago

This small book uses refugee interviews and Chinese Communist party documents to build inductively toward generalizations about when and why *hsiao-tsu,* small political study and criticism groups, operate effectively in changing the behavior and attitudes of their members. Whyte argues that some types of Communist organizations containing the pervasive *hsiao-tsu* are inherently better than others in producing the desired attitude changes in the contemporary Chinese populace.

The book is divided into 11 chapters. The first four provide an introduction to the subject and deal with the general significance, origins, and ideal forms of the political study and criticism groups that form a significant part of contemporary Chinese daily life. In the next five chapters Whyte draws upon his 100 refugee interviews to examine the operations of *hsiao-tsu* among cadres, students, peasants, workers, and labor-camp inmates by presenting comparisons to two detailed personal case histories in each of the five organizational settings. The remaining two chapters deal with Whyte's conclusions regarding *hsiao-tsu* effectiveness in different organizational contexts.

Principally, Whyte examines the case studies in five different organizational settings against the ideal criterion of a "strict political atmosphere" which is gleaned from a collection of Party documents about what type of *hsiao-tsu* is maximally effective. His interesting summary of the CCP effectiveness criteria boils down to the twin contentions that *all* activities of study participants must be judged by official norms such that no participant may compartmentalize his activities as between study and nonstudy groups and that effective social pressure will be mobilized in the group such that *no* social support can be found anywhere by a study participant for any deviant opinion or behavior. From these two strong criteria and his observations about Chinese organizational life, Whyte develops four subordinate hypotheses linking study effectiveness and organizational structure: *hsiao-tsu* are judged more effective if study is at the center of political life, if strong leadership exists, if there are few organizational supervision problems, and if the groups contain a high degree of solidarity within their larger organizational context. His major conclusion, paralleling the work of Etzioni, is that because of their varying structural characteristics cadre organizations and schools will be able to develop stricter political atmospheres than labor camps, communes, and factories and therefore can influence members' attitudes more effectively.

The quality of the book is high, but it contains a number of flaws. First of all, the method of using refugee reports to assess the effects of *hsiao-tsu* begs the question of effectiveness since the data sources invariably reflect

instances of *hsiao-tsu* failures. Thus Whyte is, indeed, on much firmer grounds in making generalizations about the comparative organizational effectiveness of *hsiao-tsu* than he is in making any absolute generalizations about the overall effectiveness of political study, which he finds minimal. Second, anyone who looks forward to data and conclusions on the psychological mechanisms of attitude change through *hsiao-tsu* will be sorely disappointed. Whyte takes the CCP ideal of "strict political atmosphere" as the unimpeachable criterion of success in changing attitudes and makes little effort to establish according to external Western or non-Western social science canons whether this "ideal" is in fact the most effective means of achieving lasting attitude change. Thus the crucial characteristic of "the cult of confession," which Robert Lifton judged a central part of the eight characteristics of the totalistic environment of Chinese thought reform practices, is nowhere examined in this work. Nor does Whyte examine Leon Festinger's criterion of incentive effects in attitude change, which emphasizes low levels of inducement as opposed to high coercion or reward in producing lasting attitude change rather than temporary compliance. Finally, nowhere in the comparison of *hsiao-tsu* cases with the CCP criterion of "strict political atmosphere" is the criterion ever fully realized in practice. The interviewed subjects were always able to compartmentalize the applicability of official norms because of their overlapping membership in family or recreational groups as well as study groups or because of the cyclical nature of political campaigns which allowed for breathing spells following periods of intensive politics. In addition, the subjects were always able to find social support for their deviant views through an independent network of trusted friends. Indeed, the mitigating effect of the privacy of the home appears even more pronounced in Whyte's work because he neglects to study the operation of *hsiao-tsu* in the urban neighborhood or residence committees which contain tens of millions of housewives and elderly Chinese. In short, the CCP criterion of "strict political atmosphere" utilized by Whyte seems too rarified and impossible to attain in practice for extended periods in contemporary Chinese life; how one assesses the impact of approximations is unclear. There is no evidence in the book that any Chinese subject is ever fully encapsulated by the "strict political atmosphere" of *hsaio-tsu*. For this individualists may be thankful; but from Whyte's work they are left unsure in their hope.

In passing this book through the sieve of strict scholarly criticism I may have asked too much of the work under study. All in all, it is a solid work on a most important topic, a work that is in its comparative and historical perspective, clear and well organized in its presentation, rich in its sensitive depiction of the diversity of contemporary Chinese daily life, and balanced in its conclusions on the effects of *hsiao-tsu*. Whyte's massive interviewing effort should be applauded by all who seek to know more about contemporary Chinese life. His book is a major contribution to scholarship on contemporary Chinese society.

Quebec: Social Change and Political Crisis. By Dale Posgate and Kenneth McRoberts. Toronto: McClelland & Stewart, 1976. Pp. 216. $4.95.

Gerd Schroeter
Lakehead University

This is clearly the best available book for introducing the English reader to the "Quebec problem" and giving him enough information to make his own intelligent predictions about the likelihood of Quebec's separating from Canada. It is a much more balanced analysis than other recent popular books on this topic, such as the Milner's *The Decolonization of Quebec* or Marcel Rioux's *Quebec in Question,* and seems to embrace all the relevant literature in both languages published until the end of 1975. In contrast to previous studies in English, it also includes references to Quebec newspapers, especially *Le Devoir*. Posgate and McRoberts claim that they wrote the book because only a fraction of the ongoing debate over Quebec independence has so far impinged upon the consciousness of English Canadians.

The approach throughout is amazingly typological, analytical, and dispassionate for an analysis of such a divisive topic. Chapter 1 provides a general framework by first pointing out the distinction between legitimacy and effectiveness of government (following Lipset) and then discussing the concept of "development" as applicable to Quebec and the rest of Canada. The central government has so far proved to be effective enough to maintain confederation by using techniques such as bargaining, making concessions, and even sending troops into Montreal in October 1970, but for many Quebecois the legitimacy of this government is nil. The basic question is whether the federal institutions should therefore be dismantled or whether renewed effort must be invested in fostering Canadian unity. Thus, while the Prime Minister contends that "bilingualism is what Confederation is all about" and the airline pilots were allowed to go on strike over this issue, many Quebecois reject bilingualism, are not interested in greater access to the civil service in Ottawa, and see an independent Quebec as their single goal.

The authors put great stress on the differences in the dynamics of development between Quebec and the other regions of Canada. From the Canadian perspective, development has been a matter of response to external forces, first British and now largely American, whereas Quebec's development has always been one of resistance to outside influences. Related to this is the observation that politics has become much more of an independent variable in Quebec than it has generally been in Western democracies, where politics reflects social changes rather than shaping them. Throughout the book the authors attempt to represent the perspective on problems of identity and social change, as well as the interpretation of past events, held by Quebecois rather than by English Canadians; this is its particular merit. If there is one difficulty with the text it is that the distinctions between Quebecois and French-Canadian are often

vague. A simple solution would have been to label those Francophones who are profederalist and antiseparatist "French-Canadians" (whether living inside or outside of Quebec) and to consider all those who favor independence "Quebecois"; in this way the name of the Parti Québécois carries a specific connotation as well. The authors don't draw such a distinction and seem to be using the terms interchangeably.

Chapters 2–8 give a comprehensive, chronological account of the history of Quebec from the time of the Conquest (1763) to the present. At the beginning of the 19th century there appeared the agrarian mythology which saw French-Canadians as a rural, farming population dominated by the church. This stereotype, together with extraordinarily high birthrates, soon became an important component in the struggle for *survivance* (survival) and has only recently been replaced by an equally strong emphasis on *rattrapage* (catching up to social development elsewhere). Rapid economic development took place after 1920, especially in the generation of hydroelectric power, in mining, and in pulp and paper production, while the proportion of the population on farms declined sharply and the cities mushroomed; but the myths lingered on, and political modernization lagged. As recently as 1943 a nationalist cleric was claiming that "by tradition, vocation as well as necessity, we are a people of peasants. Everything that takes us away from the land diminishes and weakens us as a people and encourages cross-breeding, duplicity and treason" (quoted p. 65). In 1960 the Liberals launched the "Quiet Revolution" in Quebec by having the provincial government assume the initiative for social and economic changes which had until then been controlled by the federal government and the Anglo-Canadian and American economic interests. Some of the most important early steps were to create a Department of Education, to nationalize the hydroelectric companies (thereby forming HydroQuébec), and to take over programs affecting health and welfare from the central government. As a result, contemporary Quebec is more urban, more industrial, more secular, more educated, more open, and has more citizen participation than could have been imagined even 20 years ago.

The contention has often been advanced that a significant cause of the rapid social changes in Quebec was the need to accommodate the growing bureaucratic middle class. Posgate and McRoberts pay considerable attention to this stratum, but they also discuss the increasingly radical working class with its aggressive union leadership, which concentrates strictly on bringing about social and political changes within the province and has few ties with the labor movement in the rest of North America. In fact, there are two very different types of cleavage in Quebec: there are the ethnic conflicts between Quebecois and the non-French residents, some affiliated with businesses going back for generations, others recent immigrants; and there are also the sharp socioeconomic differences within the Francophone population, some of whom benefit from federalism while others agitate for independence. Thus, there is no simple answer to the question: What does Quebec want?

One issue that most of the population agrees on, however, is the central importance of language, and the present government has passed Bill 22 which designates French as the only official language in Quebec. Yet, there is still considerable ambiguity about the right of access to English-language schools for children of non-anglophone immigrants, in the same way as there is uncertainty about the extent to which the use of French will be enforced in the private sector of the economy. The language issue is so significant because it is at the heart of ethnic survival and since all demographic projections agree that there will be some decline in the Francophone proportion of the Quebec population over the next 25 years. It is an accepted fact that the rapid assimilation rate for French-Canadians outside Quebec is due to their neglect of the language. Now that Quebec is faced with the lowest birthrate in North America, the French language has now become its major symbol of cultural survival.

But surely the burning question concerns the likelihood that Quebec will separate from Canada in the near future. On this topic the authors again present the latest information, but, although sympathetic to nationalist aspirations, they refuse to commit themselves to a prediction. Only the Parti Québécois (PQ) has taken a clear stand in favor of independence, and it won 24% of the popular vote in 1970 and 30% in 1973, although it gained only seven and six seats, respectively, out of 110 in the National Assembly. Research has shown that the PQ gets a disproportionate share of the Francophone vote and that between 60% and 77% of its supporters during the past several years have been avowed separatists. All this leads to the conclusion that over 30% of the Quebec Francophone population actively supports political independence and that about 50% are opposed, with the remainder still undecided. Hence, the authors hedge their bets about the future: although claiming that "independence is a distinct possibility" (p. 211), they also insist that "there is no historical inevitability to Quebec independence" (p. 196) even though "[the] presence among all classes of alienation from the federal system has placed the Canadian system in serious jeopardy" (p. 174).

This is a lucid and compelling piece of analysis which deserves widespread attention. The book is so carefully documented throughout that every reader will have a much better appreciation of what has become a tenacious and profound crisis, second in importance only to the plight of Canada's native population.

Eclipse of an Aristocracy: An Investigation of the Ruling Elites of the City of Cordoba. By Juan Carlos Agulla. English translation by Betty Crouse. University: University of Alabama Press, 1976. Pp. xiii+151. $8.00.

Shirley Harkess

University of Kansas

Eclipse of an Aristocracy contains the plan for a book. Since the slim volume (90 5¾ × 8¼ pages of text) is the English translation of a work

that appeared in Spanish in Buenos Aires in 1968, one wonders whether its appearance now in English is not somewhat after the fact. It is unlikely that the book for which the volume under review is a sketch is ever to be forthcoming.

As a plan, or perhaps summary of a research project, *Eclipse of an Aristocracy* is tantalizing. Juan Carlos Agulla, a professor at a university of Cordoba, Argentina, at the time the research was conducted in the mid-sixties, seeks to document the gradual evolution in the nature of an elite through which new elements become incorporated as established ones disappear. Although his study is of one city in Latin America, it has relevance for other urban areas in the region and for nations like Colombia, where the ability of the elite to adapt to changing conditions by selectively recruiting new members has been notable. It is Agulla's contention that this evolutionary process is the social and political correlate of a similarly incremental growth in industrialization. As such, his analysis is intended to counter Marxian and other interpretations that view Argentina's recent past as a record of discontinuous conflict. Confining his study of stratification to the elite allows Agulla to make a convincing case—if we can accept his evidence.

The author surveys positional leadership in Cordoba, where higher education has historically been a central social institution, at four selected points between 1918 and the present (1924, 1937, 1951, and 1960). His principal finding is that the proportion of positions of leadership occupied by members of the "ruling elite of the Cordobese aristocracy" declines from a high of 77% to a still significant proportion of 28%. Unfortunately, the operational definition of neither group is given so the reader can neither ascertain how this finding was arrived at nor assess its validity, thus controverting the publisher's claim that the book "is an exemplary study . . . because of its methodological rigor (it could serve as a model for similar urban studies worldwide)." A similar difficulty dogs his second major finding that the ruling elite abandons the political sector for legal and educational institutions.

To explain the continued representation of the Cordobese aristocracy among positional leaders Agulla turns to an examination of their origin (birthplace, foreign extraction, and father's occupation), occupation, and political party membership. Verging on circularity, he shows how the possession of a university degree by members of the aristocracy (the *sine qua non* for membership?) permitted individuals to enter the group via marriage if they had not been born into it. Throughout the period studied, the university-educated tended to specialize in the pursuit of their professional occupations. The lack of other sources of income and prestige made them inclined to represent, as professionals, interests other than their own and hence, in Agulla's view, adapt to the process of industrialization. Which interests they represented depended, to some extent, on their political party membership. The adaptability and flexibility of those with university degrees, however, insured their demise as a social group: "[T]he fact that they never had had a great economic capacity and that they are very open to receive new members belonging to

other social sectors makes them capable of incorporating into the new social classes which emerge from the process of industrial development of the City of Cordoba" (p. 88).

In addition to the problems already alluded to are several which combine to make *Eclipse of an Aristocracy* incomplete as a book. First, the data presented cry out for more information of a historical nature: What was Cordoba like? what really happened there? What went on at the university? What is the meaning of a university degree in Argentina and in Latin America generally (Agulla never clarifies the crucial distinction that his "doctorates" are the equivalents of a B.A. or M.A.)? etc. Detached from its historical context the analysis of events obviously important to the author seems curiously unreal to the reader, particularly one who may not be a student of Argentina. This problem is compounded by another of equal importance—the lack of a conceptual framework. Only the briefest mention is made of relevant theory in the text; Mosca and Pareto are acknowledged, but few others. And the concepts central to the analysis do not receive adequate elaboration. For example: "These facts led us to establish some conceptual differences between *power elites*, exercising control (political, economic, cultural, etc.) of the entire power structure, and who exercised this power in a normal fashion (that is, by representing a social stratum), and *ruling elites* who, though representing different institutional sectors of the community in the power structure, did not exercise power in a standard fashion, that is, as belonging to a single social stratum (the aristocracy or the bourgeois [sic], for example), nor could they control the entire community power structure" (p. 4). Last, specific conclusions are reached that do not flow directly from the information being presented apparently in their support.

The reader could fill some of these gaps if he or she knew Agulla's previous work (*De la industria al poder* [Buenos Aires: Ediciones Libera, 1966] with Delbert C. Miller and Eva Chamorro Greca) or, better, Miller's own work, *International Community Power Structures: Comparative Studies of Four World Cities* (Bloomington: Indiana University Press, 1970); one of the four cities was Cordoba, in whose study Agulla assisted. We are given a translation of Agulla's article in *Aportes* ("Power Structure and Power Elite in an Urban Community," vol. 2 [October 1966]) as an appendix, but it is not enough. Since *Eclipse of an Aristocracy* cannot stand alone, perhaps we should consider it a footnote to previous work rather than a plan for a future publication.

Finally, the volume suffers badly in translation. The translation by Betty Crouse bears all the marks of a literal draft. Constructions are labored and vocabulary misleading, if not incorrect. Editing would have helped, but other than in providing an index, its effects, if any, are not evident.

Patterns of Political Leadership: Lebanon, Israel, Egypt. By R. Hrair Dekmejian. Albany: State University of New York Press, 1975. Pp. xii+323. $20.00.

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Harvard University

Dekmejian does three useful things. He collects together the social backgrounds of 409 cabinet ministers—64 in Israel (1948–73), 159 in Lebanon (1943–73), and 186 in Egypt (1952–73). He uses the information as a basis for describing the development and struggles of three very different efforts at nation building. And he compares the three sets of cabinet ministers.

The comparison yields a few mildly surprising formulations: the Egyptian elite is the most formally educated and the Israeli elite the least, even at one point called deficiently so (p. 252); Israel has more ministers with religious ordination than Lebanon or Egypt and thus a lower degree of political secularization (p. 227); more Egyptian and Lebanese ministers than Israelis have been trained in "the West" (more Israelis in Russia, p. 236).

Essentially, however, the account does not upset our daily newspaper understandings. Egypt and Israel both recruit ministers from the military while Lebanon relies heavily on lawyers. The Israeli and Lebanese systems are arenas for professional politicians, whereas the Egyptian system is more authoritarian, using apolitical experts (hence the high number of ministers with Ph.D.'s). Ministers come and go twice as quickly in Egypt and Lebanon as in Israel. The Israeli elite is the most representative of the general social class composition; the Lebanese the least, and note is made (with the Lebanese civil war getting under way) of the Lebanese elite's insensitivity to the problems of the poor, which may destroy the democratic-confessional system (p. 245).

Unfortunately the first chapter demeans the account with its claims. The study of elites, we are told, is significant because leadership is a universal. "Leadership as a unit of analysis" is characterized by "precision and elegance" (p. 3), and "the empirical-quantitative method assures precision and certainty" (p. 6). *Ensha'allah!* But no quantitative methods are reflected except in tables and percentages, only a paragraph noting that it might be possible to use factor analysis, path analysis, circulation indices, or Markov chains (p. 244). We get special pleading that no one studies elites (despite reference to some 50 people who do) and, more plausibily, that comparative studies of elites are rare. It is thus doubly odd that so little use is made of previous elite studies in the Middle East (e.g., Frey on Turkey, Zonis on Iran, Quandt on Palestine and Algeria, Porath on the early Palestinian movement, Waterbury on Morocco) to generate an initial comparative basis. (One reason perhaps is that those studies deal with attitudes and behavior, whereas Dekmejian focuses only on social background.)

Throughout, there is impatience with previous social theorists: sugges-

tions about the middle-class impetus in the Egyptian revolution (Binder and Beeri are mentioned) are dismissed as untestable due to incomplete data, and Lasswellian or Marxian notions are several times dismissed as economic determinism. Instead there are simply three "political cultures" (p. 5): *zu'ama'*, great families, and confessional groups in Lebanon; *aliyot* in Israel; Free Officers and conflicts between Nasser's charisma and revolutionary colleagueship in Egypt. (The reader is assumed to know Arabic and Hebrew terms like *zu'ama'*, *aliya*, *galut*, *yeshuv*.) At one point: "Ironically Weber's conception of charisma as a creative force did not apply to the individuals close to the leader [Nasser]; instead charisma induced conformity at the top" (p. 181)—but the latter was precisely Weber's point regarding Bismarck and not only those close to him but the whole parliamentary and bureaucratic system.

Dekmejian concludes, "Enduring peace in the Middle East may well depend on whether the [Egyptian and Israeli] elites perceive each other as equals in effectiveness" (p. 253). He considers that in the limited sense of moving them toward such a perception, the 1973 war was a good thing (pp. 251–52). Perhaps. But somehow it seems odd that an analysis of a revolutionary Arab socialist state (Egypt), a welfare-socialist state (Israel), and an unstably gerrymandered confessional state (Lebanon) can be satisfied with a notion of power as that which is exercised by cabinet ministers (p. 4).

The Syrian-Lebanese in America: A Study in Religion and Assimilation. By Philip M. Kayal and Joseph M. Kayal. Boston: Twayne Publishers, 1975. Pp. 260. $12.50.

Ayad Al-Qazzaz

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Despite the widespread ethnic awakening in this country and the increased interest of social scientists in minorities in the past two decades, the Arab minority did not capture the interest of social scientists until very recently. The smallness of the group, plus the Arab-Israeli conflict, discouraged many social scientists from choosing this group as a subject of concern and study. During the past few years, however, this trend has been reversed to some extent, with the publication of at least four full-sized books and over a dozen articles. The increased interest is due largely to the fact that the Arab community today is much more visible than it was. Several people of Arabic background have achieved prominent and visible positions, including Ralph Nader, Danny Thomas, Gibran, Senator James Abourezk, Omar Sharif, Fasiq al-Baz, and Michael DeBakey. Furthermore, the Arab-Israeli conflict, particularly after the Six Day War, took a new dimension, encouraging Arab-Americans to get organized on a political level to influence American foreign policy toward evenhanded policies. Thus, such groups as the Association of Arab-

American University Graduates and the National Association of Arab-Americans have come into being.

The book under review represents a welcome addition to the small library on this subject. It consists of 15 chapters of various lengths, ranging from five to 30 pages, plus a foreword by Michael Novak, a preface, and a bibliography. Methodologically, this book is different from other recently published ones on the subject, such as Elkholy's *The Arab Muslims in the United States* (New Haven, Conn.: College and University Press, 1966), *The Arab American,* edited by E. Hagopian et al. (Wilmette, Ill.: Medina University Press International, 1969), Zeadey's *Arabs in America: Myths and Realities* (Wilmette, Ill.: Medina University Press International, 1975), and *Arabic Speaking Communities in American Cities,* edited by Aswad (New York: Center for Migration Studies, 1974). Those books and others tend to consist of case studies of limited, small communities residing in one particular area and employ questionnaires or participant-observers as a technique to gather the necessary information and data. In contrast, the book under review deals with all Americans of Syrian-Lebanese origin regardless of residence and time. Furthermore, the author relies heavily on historical data to sketch the origin and development of this community over the years.

Three points deserve our attention in this short review:

1. It is impossible to state with certainty when the first Syrian emigrated to the United States or who he was. However, by the last quarter of the 19th century the number of Syrians was significant enough to draw the attention of the people of this country, as manifested by articles in local newspapers and by the adoption of the term "Syrian" as an entry in the classification of immigrants by the immigration office. The author of the book informs us that most early Syrian immigrants came to this country to escape political and economic oppression by the Ottoman Empire, which was manifested through heavy taxation, army consumption, and poverty. These persons came to America because for them it was a land of plenty and opportunity for all. Most of them were of Christian faith. Because they were uneducated and unskilled, they had to start at the bottom of the scale and move up. They worked as peddlers, grocery store owners, blue-collar workers, etc. Today, however, the children and grandchildren of these Syrians are represented in practically all walks of life, particularly in professional and industrial occupations. Indeed, the Syrian community is filled with immigrant Horatio Alger heroes. They include nationally known manufacturers, like J. M. Haggar, who founded the Haggar Company of Dallas, Texas; Elias Sayour, who founded Saybury Housecoats of New York; and the founders of the Farah Slacks Company of Texas and the Jerro Brothers Shoe Company of New York (p. 106).

2. The identity of Syrian immigrants passed through three stages. In the beginning they were called Turks, as Syria was once part of the Ottoman Empire and the early emigrants carried Turkish passports. Later, they were identified as Syrians (i.e., by the country from which they came). More recently, they have been identified more and more as

Arab-Americans, that is, by their nationality. To a large extent, this change has coincided with the political and social changes taking place in the Arab world. The Arab-Israeli conflict, from its inception and particularly since the Six Day War, subsequent massive Israeli military incursions in Lebanon, and the negative image applied indiscriminately by the mass media, which depict the Arabs as bloodhounds, fleshmongers, and a species with unquenchable appetites for bloodletting (p. 216)—all of these have forced these immigrants to form organizations to defend themselves and to influence American policies.

3. The future of the Syrian-Lebanese community and its ability to maintain itself as an ethnic group with a unique culture and style of life are in great doubt. Particularly among the generations born in this country the process of acculturation and assimilation is rapidly engulfing this community, as manifested in intermarriage, language, and other cultural components.

According to the author, the Arab-American community is "only marginally and selectively involved in the institutions of their ethnic community." This is due largely to the institutional incompleteness of their community and to the irrelevance of their primary institutions to present socioeconomic status and objectives. Furthermore, there has been a change in generations whereby the transmission and maintenance of ethnic identity have become irrelevant and unnecessary in today's urban society (p. 231).

The book suffers from several shortcomings. The most serious is the author's loose usage of several important terms, such as "Arab," "Middle East," and "Moslem," with no attempt to define them. Three examples will be sufficient: (*a*) The term "Middle East" is used as identical with the term "Arab world," although in fact, over one third of the Middle East (Turkey and Iran) is not Arab. (*b*) A person is classified as an Arab if he participates in the cultural style of the Middle East (p. 34) although many Iranians and Turks, for example, despite their participation in the Middle Eastern style of life, do not identify themselves as Arabs. (*c*) "Today, for all practical purposes, we have in this country three basic, distinct social groups with little or no contact among them: Syrian Christian, Syrian Jew, and Arab Moslem from Syria" (p. 61). Here the author equates the term "Arab" only with "Moslem," while in fact, anyone who speaks Arabic and identifies himself as Arab is an Arab, regardless of his religion. Indeed, many of the pioneers of the Arab Nationalist movement were of Christian faith.

Another error is the author's claim that Lebanon has a slight Christian majority (p. 55). In fact, students of Lebanon have known for many years that Muslims represented the majority of Lebanon, as their birthrate tended to be higher and there was a tendency among the Christians to emigrate.

Despite all these shortcomings and others, the book is must reading for all students of the Arab minority in this country and worthwhile reading for all students of minorities in America.

Polish Americans: Status Competition in an Ethnic Community. By Helena Znaniecki Lopata. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1976. Pp. xvii+174. $5.50.

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In *Polish Americans,* Helena Lopata has made a valuable contribution to the study of ethnicity in the United States. Relying on her own research and that of fellow students of Poland and Polish-Americans, she has described and analyzed the foundation and development of the Polish-American community (Polonia) and its relationships to the broader national culture societies of Poland and America.

In the first chapter she outlines some of the problems of identifying Polish-Americans. This is often difficult, because Poland as a nation did not exist during much of the emigration and because the emigration took place in several waves. One solution she offers is to identify members of Polonia by recovering the networks of interaction among individuals and organizations that develop and are maintained in the context of shared cultural symbols (p. 6).

Chapters 2 and 3 examine the impact that the social structure of Polish society in the late 19th and early 20th centuries subsequently had on Polonia's relationship to Poland in later years and the development of Polonian institutions. The author argues that Polonia never fully supported the Polish national effort because of the persisting cleavage between the intelligentsia and the former peasants. Indeed, millions of dollars and a division of men were sent to Poland in the fight for independence; but although the intellectuals strongly supported the creation of a new Polish state, the masses never identified with the Polish national culture and supported friends and relatives only for personal reasons. Lopata argues that this was because in Poland the peasants had been virtually ignored by these same intellectuals and excluded from the national culture (pp. 22–23).

She also argues that the development of many of Polonia's institutions can be traced to particular character traits found among Polish peasants and a peasant tradition of excessive status competition (pp. 19–20). She rejects an idyllic peasant stereotype and describes Polish peasants as being individualistic, uncooperative, and competitive. Once in America, with greater opportunities for competitiveness, organizations proliferated and competition flourished (p. 46). The high level of status competition proved dysfunctional, however. It caused political schisms, weakened the community's capacity for concerted political action, and shamed the Polish community in the eyes of the larger society (p. 55). Furthermore, it encouraged professionals to build careers within the community (p. 56), prevented organizational leaders from coalescing and engaging in interorganizational enterprises (p. 62), and even had far-reaching effects on the family. Because of intense competition with other families, relations within the home tended to be cold and authoritarian.

Chapter 4 explores the relationship between Polonia and American society. There has been a long history in America of hostility toward Polish-Americans and other "aliens" (pp. 70–71). The latest revival came in the 1960s in the form of the "ethnic joke," just as third-generation Polish-Americans were achieving upward mobility and emerging from Polonia. Lopata correctly points out that this recent wave of character defamation was a jolt both to those who had left Polonia one or two generations ago and to those who were first competing in the larger society. She speculates on the possible sources of this bigotry and documents the attempts of the Polish community to defend itself.

In chapter 5 the author explores changes in the social structure of Polonia. She examines the education, occupations, and life-styles of the different waves of emigrants, their children, and grandchildren. The clear differences among the cohorts highlight the complexity of the Polish-American identity today. Lopata chastises ethnic researchers who operationalize Polish ethnic identity only in terms of how much the second and third generations have retained elements of the Polish folk culture. She cautions that a broader Polish identity linked to Polish national culture may well be on the horizon (p. 114).

In chapter 6 Lopata develops the concept of the "companionate circle." She defines this as "a loosely bound group of people from the same ethclass who interact with one another, belong to the same organizations, lead a similar style of life, and are identified by others as belonging to the same circle" (p. 119). The boundaries of these circles are penetrable. Thus individuals can easily move in and out of them, participating in the ethnic community to varying degrees. She then describes companionate circles of different elites in Polonia, how circles of elites interact, and how the structures of companionate circles differ from community to community.

Although I agree with most of what Lopata says, there are some points which I must dispute. First, because the social distance between the intelligentsia and the former peasants has persisted and will probably block any adoption of the Polish national culture by the second and third generations, the basis for this cleavage must be explored in detail. From my reading of *Polish Americans,* it seems that the peasants were never quite sure who the intelligentsia were. The intellectual's image of being the "cultured man, participating widely in the nation's cultural heritage, a man with a knowledge of history, literature, the arts and good manners" (p. 18), made it difficult for peasants to differentiate the proletarian avant-grade from disaffiliates of the old aristocratic order. If the former peasants perceived the intelligentsia only as survivals of their feudal past, their resentment is understandable. It simply reflected the old social class conflict in Poland. If they perceived the intelligentsia as part of the urban proletariat, their resentment may have been based on the anticlericalism of the revolutionaries. Unfortunately, however, neither of these hypotheses is fully explored.

Second, status competition may be an important dynamic social process in peasant as well as ethnic communities, but without comparative data it is difficult to believe that this phenomenon has been especially

characteristic of Polish peasant society or Polish-American communities, at least to the extent that it explains why Polonia's institutions developed differently from those of other ethnic groups. Furthermore, several other factors besides the "character" of the people could lead to status competition, and these also should have been examined.

Finally, I question Lopata's solution to the problem of establishing a meaningful Polish-American identity. Polish-Americans have always used pluralism as a crutch to legitimate and rationalize their segregation from the larger society, yet they are rejected as being only "pseudo-ethnics" by Polish nationals. Lopata suggests that Polish-Americans have two options: abandon any ethnic identity or adopt elements of the Polish national culture (pp. 147–48). Even if there were no resentment against the intelligentsia, however, I doubt that Polish-Americans could import and adopt a foreign culture without experiencing anomie. I would argue that a meaningful ethnic identity for Polish-Americans can develop only from the Polonian ghetto experience. This seems to be the only option, even though such a culture would be "inferior" to the Polish national culture offered by the intelligentsia.

The current rejuvenation of interest in ethnicity may come more from rejection, defamation, and the need to defend oneself than from any romantic rediscovery of primordial roots. If so, this raises serious questions as to the role of ethnicity in American society. Regardless, our understanding of ethnic Americans is greatly enhanced by Lopata's new book.

Social Analysis: A Marxist Critique and Alternative. By V. L. Allen. London and New York: Longman Group Ltd., 1975. Pp. xxiii+316. $19.00.

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The Cathari (the "Pure"), adherents to a heresy prevalent in southern France in the 12th century, afford a historically obscure example of the sectarian mentality. Indeed, their very obscurity is relevant to this ideal-typical construct because it serves to indicate the general historical fate of movements generated by such ideas. They advanced an oft-repeated soteriology based on a dualistic world view posed in terms of the realms of good and evil. True to form, these children of light sought to remain uncontaminated by the world of evil through a rigorous exclusionary policy. Thus, for example, the Old Testament was rejected as a tainted text, too much a part of the world of the flesh, while the New Testament, they would have one believe, emerged out of whole cloth. V.L. Allen's recent book proceeds in a similar vein: a thoroughly humorless book, devoid of subtlety and any semblance of irony, it is, indeed, the product of a mind hermetically sealed in a sectarian catacomb, whose rarefied atmosphere permits only the invocation of sacred liturgies.

The text purports to offer a critique of academic sociology, with special emphasis on organization theory, from the position of one "committed" not only to Marxism, but also to Kuhn's notion of scientific revolutions. This analysis, the author boldly states, is part of an inevitable collective process of emancipation from capitalist society. The blithe certitude of Allen's dialectical materialism is contrasted throughout with the varieties of nonconformist bourgeois sociology, exemplified by such figures as Gouldner, Bottomore, and Birnbaum. What characterizes academic sociology (including the above-mentioned internal critics), simply, is its static quality, while Marxism, in contrast, provides the sole possibility of analyzing dynamics. Bowing to Kuhn, the author characterizes the gulf between static and dynamic analysis as "incommensurable." The former mode of analysis, furthermore, serves a legitimating function: description proceeds to explanation, which summarily melds with a justification of an existing state of affairs. Or, static theory is synonymous with ideology; in particular, the ideology of the capitalist mode of production ("Adam Smith's theorizing was a necessary prop for the powerfully developing economic system of *laissez-faire*" [p. 72]). Dialectical materialism, on the other hand, becomes the dominant theory only when capitalism is laid to rest by a revolutionary overthrow. From Allen's perspective, this exemplifies the identity of theory and practice, although one is tempted to suggest that what it most clearly expresses is his denigration of thought from a manifestly mechanistic brand of Marxism.

The perplexity facing a reviewer reflects this fact, for the author, instead of developing an argument, merely offers a litany of ill-specified distinctions which are supposed to illustrate the polar opposition of Marxism and academic sociology. What is one to make of a person who can actually write that the word "dialectics" causes most theorists to "recoil" because "the political implications . . . are too much for the vast majority of Western sociologists to stomach" (p. 250)? Consider the passage where Allen contends, without elaborating, that Blau and Etzioni use the terms "change" and "dynamic" in a "decorative manner," thereby giving them "an imprecise and insipid quality" (p. 176). One further quotation is enough: "Sociology . . . has proceeded to reification via mystification caused by theoretical confusion" (p. 22). Indeed!

The book contains chapters on Weber, Etzioni, and March and Simon as representatives of static theorists and their limited understanding of organizations. It treats such topics with what is said to be an appreciation of such Marxists as Lukács, Gramsci (spelled incorrectly throughout), and Althusser. Further, it seeks to approach organization theory by integrating the work of an apparent mentor, W. Baldamus, which focuses on the issue of the significance of efficiency and skills in discussions of power differentials. Nowhere, however, does the text appear to stake a novel course of investigation; it is for true believers only.

Knowledge and Politics. By Roberto Mangabeira Unger. New York: Free Press, 1975. Pp. ix+336. $12.95.

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Roberto Unger has given us a work stunning both in its ambition and in its breadth of scholarship. The premise of the book is that within the liberal tradition all of epistemology, economics, psychology, sociology, ethics, theology, and jurisprudence lead to a common cul-de-sac. In their modern forms each of these sciences, says Unger, creates an inconsistent or self-defeating dichotomy between abstract generalities (universals, theories, rules) and concrete particulars (facts, desires, individuals). Unger is at his best in exposing the various perceived guises of this common flaw in modern thought.

The paradigm of this conflict obtains in our "liberal" theory of knowledge. If, as liberal epistemology claims, facts do not fall into natural "self-evident" predetermined categories, then it is our theories that must determine what is to count as a fact. "A fact becomes what it is for us because of the way we categorize it." We therefore can make no direct appeal to reality because "reality is put together by the mind" (p. 33). One requires some sure knowledge of concrete particular reality, however, if, as we also believe, we can choose among conflicting theories by recourse to facts. The book gives profuse illustration of similar, derivative difficulties in other branches of science. Examples range from the conflict between public roles (universals) and private life (particulars) in the individual personality, to the conflict between legislation and adjudication of laws.

Unger expects a great deal from philosophy, the energy in forming this book being a demand that philosophy actually resolve such conflicts. The liberal instinct is to expect less, to coexist with the competing claims of theory as defining fact and facts as sustaining theories, and to seek some balance in a historical process in which facts and theories are continually redefined. Similarly, the liberal instinct is to see a historical balance of the conflicting claims of impersonal general moral rules and personal particular feelings of mercy or retribution. Without some such balance the authenticity of scientific progress as uncovering reality is placed in doubt. Yet Unger rejects both psychological and historical constructs which might validate the evolution of knowledge by reference to universally shared categories of the mind (whether born of a teleology of history or an inherent similarity of men). Instead, he beckons us back to the age of faith, of intelligible essences, of natural law, and of feudal society to solve these riddles. We have, in fact, come to expect rather little in the way of reassuring certainty from our philosophy, science, law, or religion. Unger craves this kind of certainty, thinks we all do, and bids us look to the consciousness of individuals—and to social consciousness as realized in social organization and experience—for the source of our desired tranquillity. "The division of the world into an order of ideas and an order of

events, with their corresponding methods of logical analysis and causal explanation, must not be accepted as the eternal fate of thought. Between the order of ideas and the order of events, there is a third realm, the order of consciousness, mind, culture or social life." Moreover, "every general way of thinking that dominates the ideas of an age has to be understood as a phenomenon of consciousness rather than just as a theory" (p. 107).

Our escape from the liberal impasse accordingly lies in anticipating the present drift of social consciousness. Appropriately, therefore, Unger's construction of a philosophy to replace Liberalism begins with a theory of the "welfare-corporate state," which is to say of a conciousness or culture to replace liberalism. Liberalism is about to be overthrown by a secular version of pantheism. "The principle of common meaning that ties together the features of the emergent consciousness is a religious conception of the world and of society" (p. 179). Thus for Unger the answer to our disquietude remains religion just as in the Age of Faith, but now secularized, an evolving reference for man and nature, a converging reverence for individual men and their individual works. Unger's hope is that human evolution may lead toward organic communities, based on shared values and thereby realizing the essential goodness of human nature. This is religion without theology. We should pin our hopes on the self-realizing goodness of human nature; we should agonize less about the paradoxes the mind has created and observe-enjoy the evolution of organic social consciousness more.

Unger's work is utterly vulnerable to piecemeal attack by positivists at one extreme and by dogmatic theologians at the other. Such attack will surely be made and just as surely will miss the point: this book is not merely a logical argument; it is meant instead to appeal to the realm of consciousness, as would a painting or an architectural construct. *Knowledge and Politics* is at once exciting and frustrating, exciting as a piece of poetry, frustrating for being rendered in prose.

Objectivity in Social Science. By Frank Cunningham. Toronto: University of Toronto Press, 1973. Pp. iii+151. $8.50.

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Rarely, if ever, do we reflexively examine ontological, epistemological implications of mainstream professional sociological enterprises. The press of contributing to our discipline and elaborating on prior accumulations of research and scholarship in our specialty constricts our focus. Sometimes, however, in a relaxed unguarded moment, we inquire of ourselves, "Of what lasting significance is my work . . . does it really matter?" On such an occasion you may turn to a book like Frank Cunningham's *Objectivity in Social Science,* saying to yourself, "Maybe this book will relieve my discomfort concerning the validity of my re-

search endeavors and the assumptions I have made about causality." Unfortunately, such discomfort is not resolved.

Cunningham undertakes his book for two reasons. First, he states that he has lost patience with classroom agnosticism and the hypocrisy of antiobjectivists. To say it is impossible to debate the relative superiority of one position over another is unacceptable to him. He is impatient also with the hypocritical pseudo-objectivist who employs the language of rigorous scientific objectivity, piously prefaces his views with statements about the need for objectivity, and then substitutes mere rationalization for conclusions in order to affirm a view "he antecedently holds or believes it opportune to hold. . . ." (p. vii). Second, Cunningham does not want to acknowledge the possibility of being deluded himself. He is a "scientific" Marxist. He believes that arguments favoring neutrality imply that people are deluded if they believe they adopt explanatory views objectively.

For these reasons he seeks to provide "a convenient map for finding one's way about in the tangle of issues surrounding the question of objectivity in social science" and "a set of arguments sufficient to convince the perplexed and previously wrong-headed of the (objective) falsity of social scientific anti-objectivism" (p. ix). At most, his arguments will illuminate possible sources of our discomfort.

The convenient map which Cunningham produces does little more than tell us about the alleged pitfalls of antiobjectivism. Except for a few pages of definition, the reader has no direct clue about the substance of objectivity. We are told what it is not, but we are not told what it is.

An objective inquiry, Cunningham states, must satisfy two criteria. (1) It must be possible for descriptions and explanations to reveal the true nature of the subject matter inquired into, that is, "the qualities and relations of a subject matter independently of an inquirer's thoughts and desires regarding them." (2) It is impossible for "two inquirers holding rival theories about some subject matter and having complete knowledge of each other's theories (and the grounds of each) to both be justified in adhering to their theories." Thus, for the conclusions of a social science theory to be objectively true, the quality and relations attributed by its conclusions must be those actually possessed by and apparent from its subject matter. Cunningham then lays the ground for the chapters which follow. First, we are disabused of misconceptions about objectivism: it is not positivism, behaviorism, determinism, inductivism, or detachment from extrascientific demands of social, political, and economic life. Second, we are instructed that positions held by proneutrality "antiobjectivists," such as argument from values, argument from selection, and historicism, are insufficient by themselves.

I believe Cunningham correctly and skillfully dispatches the positions of argument from values and argument from selection. In his discussion of historicism, however, he exposes a crack in the foundation on which his argument rests. Mannheim's historicist argument seems to be a general statement of specific antiobjectivist arguments such as the nature and

history of science, linguistic relativism, perceptual relativism: the content of knowledge at any particular time and place is affected by the "temporal and social conditions of its emergence." Cunningham describes what he calls the theory-ladenness view: "If the theories one holds determine what his perceptions and interpretations of things described by the theories can be, so that his adoption or abandonment of a theory cannot depend on his recognition of qualities and relations of the things concerned, then it would be plausible to argue that their adoption or abandonment is dependent on his values or non-scientific circumstances of his historical period" (p. 21). Consequently, the author notes, the antiobjectivist encounters two issues: (1) "no two theories of sufficient generality could ever really disagree since they could not be said to be purporting to explain the same things"; (2) no proponent of this position, itself part of a theory, could consistently claim that a position which contradicts his own is false while his own is simultaneously true. This second consequence, Cunningham contends, forces the antiobjectivist to choose between not believing his own theory and asserting that it is objectively true. This "paradox" is predicated on an assumption on which the entire book is based: if a person sincerely believes a theory, he concomitantly believes it to be objectively true.

This assumption is the crux of Cunningham's attack on subsequent antiobjectivist positions of the Kuhnian approach to the nature and history of science, of linguistic relativism, and of perceptual relativism. He spars with antiobjectivists by using excerpts from their works to construct dialectics. Sometimes he catches the antiobjectivist defending his position as if its objective truth could be ascertained. Then he uses that contradiction to discredit the position, instead of the view itself.

He claims that to hold a view, a theory, without necessarily believing it to be true would be "thorough-going skepticism." Is thoroughgoing skepticism so unreasonable? Notwithstanding the specter of nihilism implicit in Cunningham's use of the phrase, do we as social scientists consider views tenable without also believing them to be true? Of course. Not only are we circumspect with regard to the correspondence between "observational laws" and "theoretical laws" but also we are reluctant to claim that we have reliable and valid observations.

Cunningham shows his naiveté about research when he states that "nobody has or could ever hold" the position that "rational acceptance or rejection of any theory or view requires some criterion, but then rational evaluation of a criterion would always be circular or generate an infinite regress" (p. 34). In a chapter entitled "The Social-Scientific Subject Matter," he tries to remove from the reach of antiobjectivists the position that human beings are different from the objects of natural sciences and that the possibility of objectivity allowed for natural sciences is inapplicable to social sciences. His case depends in part on two problematic contentions (p. 95): (1) measurability is not a requirement of objectivity for any science; and (2) not "all mental phenomena are in principle incapable of measurement." To support the second contention the author points to techniques of questionnaire construction and survey-data analysis entail-

ing the mathematical expression of underlying attitudes and beliefs. Had Cunningham discussed such endeavors with more practitioners, from stages of instrument construction to the choice of statistical measures and probability distributions, he might realize that researchers can be thoroughgoing skeptics indeed. Were he familiar with the work of modern physicists such as Heisenberg, he might even raise the question whether we can attribute objectivity to either social science or natural science.

Explorations into the nature of reality as a multidimensional space, some points of which receive consensual collective agreement and are thereby passed on as a collective mindscape called culture, punctuate the growth of knowledge in the 1970s. John Lilly's experiments on the reticular activating system of the human brain, for example, call into question previous assumptions regarding the construction of material reality. In sociology, the advent of ethnomethodology manifests a comparable intrusion into more conventional constructions of reality. Even though the ontological basis for our socially constructed reality may be changing, however, it does not follow that antiobjectivists are socially conservative nihilists who reject compassion and programmatic attention to human rights and equality. Although I share Cunningham's concern about classroom sterility and the attendant absence of commitment to social change, I also regret his avoidance of the substance of the antiobjectivist and the objectivist positions. Instead of merely exposing weaknesses in the supporting arguments of Polanyi, Kuhn, Hempel, and other antiobjectivists, a critic should devote attention to their position itself. This book neither does so nor offers persuasive presentations of objectivism.

Social Movements in Organizations: Coup d'Etat, Insurgency, and Mass Movements[1]

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Organizational change and conflict are conceptualized in social movement terms. Using an analogy to social movements in the nation-state, a resource-mobilization approach is employed to examine the range of social movement phenomena in organizations. Three major forms are distinguished—the coup d'etat, bureaucratic insurgency, and mass movements—which differ in their breadth, location in the organizational social structure, goals, and tactics. Major attention is given to the occurrence of social movements in corporate hierarchical forms, although the approach is applicable also to social movement phenomena in federated and in voluntary associations. A number of illustrative hypotheses are given.

Power structures and political processes of organizations are important components of organizational analysis. Theoretical perspectives as disparate as those of Cyert and March (1963) and Zald (1970*a*, 1970*b*) contain notions of political processes or political structure as central components. And a large number of empirical studies examine the role of politics and power in the allocation of resources (Pfeffer and Salancik 1974) as determinants of organizational satisfaction (Tannenbaum 1962); as factors in the evolution of procedural rights, due process, and quasi-judicial processes (Evan 1965; Pondy 1964); and as influencing decisions on major capital investments or acquisitions such as computers (Pettigrew 1973). Moreover, a number of papers treat power distributions as the outcomes of organizational tasks, contingencies, or dependencies (Crozier 1964; Thompson 1967; Perrow 1970; Zald 1962; and Hickson et al. 1971).

Although this literature is rich and growing, it has two noticeable shortcomings. First, because so many of the organizational studies are synchronic, the study of things political is only rarely linked (either as independent or

[1] We are indebted to Ivar Berg, Anthony Oberschall, David Snyder, and the reviewers of this *Journal* for many of the illustrations used in this paper and for discussions of the issues raised. An earlier version was presented at the meeting of the American Sociological Association, New York, September 1976. The Vanderbilt University Research Council has aided in the preparation of the manuscript.

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as dependent variables) to the study of organizational change. Second, studies or theories of organizational politics rarely take note of their kinship with investigations of political processes in nations and communities (but see Zald 1965). Thus they ignore a massive amount of thinking and research that might be relevant to work on organizations.

The fundamental assumption of this essay is that social movements or phenomena resembling them occur in organizations. That is to say, just as political sociologists study social movements in nation states because they represent the cause and effect of societal change occurring outside institutionalized channels, so, we believe, the student of organizations needs to examine social movement phenomena in organizations because they too may be the cause and effect of emerging organizational change.

Implied by our assumption is the use of social movements in the larger society as models of or analogies to activities within organizations. For example, in the fall of 1975 Robert Sarnoff, president of RCA and son of the founder of the company, took a trip around the world to visit RCA's foreign plants. He had recently announced the reassignments of some of the senior executives; also, the company had suffered alarming financial reverses from the 1973–75 recession and Sarnoff's strategic decision to enter the computer industry. In his absence, senior executives convinced RCA's board that Sarnoff's reorganization was inappropriate and that he should be replaced. It is reported that when he returned he was handed a letter of resignation for his signature ("His Master's New Voice," 1975). The phrase that might characterize this bloodless change in leadership is "organizational coup d'etat."

In a similar vein, in the late 1950s Brigadier General Hutton (faced with official opposition based on written agreements among the Department of Defense, the army, and the air force) developed the armed helicopter. Starting with baling wire and lashing machine guns to the frame, a group of midrank officers evolved the quick-strike, mobile air cavalry. The army's air force is now the third largest in the world, behind the United States Air Force and that of the Soviet Union (Bergerson 1976). The term that best describes the action of the Hutton group is "bureaucratic insurgency."

Finally, there have been many protest movements and rebellions within organizations. Fights between local and national unions with management and the national union on one side and the local on the other (Berg 1962; Serrin 1973), the recent cleavage within the Board of the New York Stock Exchange leading to the ouster of the president ("The Big Board's New Mr. Big" 1976), and organizational schisms within the Southern Methodist groups (Wood and Zald 1966) can all be characterized as mass movements in organizations.

These phenomena are important for sociological analysis because they affect the major priorities of organizations, the control of organizational

resources, organizational survival, and growth. Moreover, they not only occur within organizations but also reflect the larger trends and politics of society. That is, struggles for executive office, clandestine product development, student revolts, seizure of plants, the admirals' revolt, and the fight for control of unions can be viewed as autonomous events, but such analysis misses an important point: social movements in organizations are often the situs for the working out of political issues and trends of social change. Organizations exist not only in environments (the "in" thing to study these days); they exist in society.

The central argument of this paper is that much conflict in organizations occurs outside normal channels. It is "unconventional opposition." Just as in nation-states, when conflict occurs outside institutionalized channels, the tools of social movement analysis are brought into play, so too in organizations: much unconventional opposition and conflict can be subjected to social movement analysis. To support this argument, we consider the three types of social movement phenomena distinguished briefly above: organizational coup, bureaucratic insurgency, and mass movements. These types are analogous to major types discussed in the literature of social movements. Different kinds of organizations and organizational conditions facilitate the occurrence of one type or another. Moreover, these forms of opposition occur at different locations in the social structure; they use different tactics; and they have varying effects upon the organization. A resource-mobilization perspective is used here to analyze the determinants which may affect the occurrence of each form, the processes of interaction between authorities and partisans, and the factors which determine outcomes. We discuss the possible implications of the social movement for the organization, and we also evaluate the utility of our analogical model.

ANALYTICAL PERSPECTIVE

Analogical theorizing proceeds by demonstrating that concepts and images used for describing and understanding one set of phenomena are counterparts of aspects of some other phenomena. Analogies have only weak theoretical utility if the phenomena have very different components; that is, if the analogy resembles the phenomena in question only in surface ways, we can speak of it as a metaphor or simile. Metaphors and similes are weak analogies. On the other hand, to the extent that the process and structures are isomorphic, that the things being compared resemble each other in great detail and in the interconnection of their parts, analogies become increasingly appropriate, and we can speak of them as strong analogies (see Willer 1967; Nagel 1961). We argue that there are many similarities between organizations and nation-states and that in fact our comparison of social movements in organizations with those in nation-states represents a strong

analogy. (The differences between nation-states and organizations and the limits to the analogy are discussed in the Conclusion.) In this section we show first that a body of literature has been developing which implicitly or explicitly treats social movements in organizations in terms similar to those used in analyzing nation-states. Then we comment on the approach to social movements used here. Finally we articulate the main dimensions of organizational politics and polities relevant to our analysis.

Previous Literature

Although they have not conceptualized them in social movement terms, a number of writers have described events in organizations that parallel descriptions of social movements in nation-states. There have been many studies of social movements within a particular organizational setting. In educational organizations, Heirich (1971) examined processes of polarization and the spiral of conflict at Berkeley, Kelman (1970) focused on events at Harvard, Daniels and Kahn-hut (1970) reported on social movements and faculty involvement at San Francisco State, and Lipset and Altbach (1969) looked at the university student in politics and in university politics in different nations. In industrial settings, Britt and Galle (1972) extended Kerr and Siegel's (1954) classic study of strike proneness to show how the volume of conflict varies with proneness to conflict, extensity, and intensity of conflict. In other contexts, Martin (1954) and Wicker (1975) described riots in prisons which can be seen as protest movements; and Adams (1970) examined ideology, conflict, and heresy trials in Protestant seminaries. Finally, there is an increasing literature on change, ideology, and forms of protest and insurgency in the professions (Needleman and Needleman 1974; Ross 1975; Epstein 1970; Gerstl and Jacobs 1976). This literature will eventually provide a rich base for secondary analysis. But from our point of view it is limited by its failure to set the problem of unconventional opposition and conflict in a comparative and more middle-range theoretical perspective.

In contrast, we have identified six articles which begin to grapple with the problems of conceptualizing social movement phenomena in organizational settings. These articles use terms and modes of analysis very similar to those employed by analysts of political process, conflict, and social movements in the larger society.

In a prescient and almost forgotten article, Selznick (1943) examined the battles for control of voluntary organizations such as political parties and trade unions. Although most of the article deals with the general theory of bureaucracy, the final section examines the relationship of the rank and file to leaders, the growth of factions, and the ideologies of centralization and autonomy.

In two articles that parallel Selznick's concern, Burns (1955, 1961) discusses the ways in which formal and informal groups may encompass political goals and develop strategies and rhetoric to support their ends. In the earlier article (1955), Burns distinguishes between cliques and cabals in organizations. While both types of informal groups were vehicles for handling uncertainty about status and role function, cliques were made up of older employees with no status differences, oriented toward protection, reassurance, and withdrawal from the realities of failure. Cabals, on the other hand, were made up of younger workers, had a distinct leadership hierarchy, and were oriented toward improving their own position in the allocation of power and resources by means of the cabal itself.

In his later article (1961), Burns shows how two aspects of organizational politics intertwine: the pursuit of self-interest and the mechanisms by which external and internal conditions of organization are translated into institutional change and changing allocations of resource and status. Burns draws his illustrations from studies of universities and businesses.

Bucher and Strauss (1961) argue quite explicitly that the processes of professional segmentation can be described in social movement terms. They suggest that professions are loose amalgamations of "segments," pursuing different objectives, using different means, held together more or less delicately under a common name at a particular period in history. Citing differences within the medical profession between general surgery and urology, for example, the authors argue that differences in sense of mission, work activities, methodology, clients, interests, and associations lead to the formation of "segments" similar to social movements and that since professions occur within institutional arrangements, the dimensions of social movement analysis (i.e., ideology, goals, participants, leadership, and tactics) can be used to evaluate the struggle over possession of resources.

Whereas Bucher and Strauss examine segments of professions, Leeds (1964) examines the development of nonconforming enclaves within hierarchical organizations with commitments to normative goals (e.g., military units, religious organizations). She explores how dissident charismatic leaders and their followers deviate from organizational priorities and form partially autonomous enclaves with specialized missions. One of the advantages of Leeds's analysis is that it begins explicitly to discuss authority responses and their influence on outcomes.

Leeds describes the protest-absorption process as a structural weapon in the hands of authorities for converting the nonconforming enclave into a legitimate unit by giving it autonomy, in exchange for an agreement that it will abide by the regulations of the organization. She contends that the primary consequence of the protest-absorption process is the introduction of change.

Finally, Lammers (1969) employs quantitative techniques to study 20

mutinies and 20 strikes. He distinguishes three types of protest movements in organizations—promotion of interests, seizure of power, and secession—and argues that the form of the movements varied with the compliance structures of the organizations (Etzioni 1961). Lammers's article discusses the determinants of the incidence, duration, and outcomes of protest movements in military and industrial organizations. Although we believe his emphasis on the relationship of compliance structure to the type of movement is too simple (see Zald and Jacobs, in press), his article is important not only because it is an early example of the use of quantitative comparative techniques in the study of organizational social movements but also because it shows how a deep analysis of the structure of these organizations enriches our understanding of social movement processes within them.

These six articles are important to us for several reasons. First, they are explicitly framed in terms of concepts parallel to those used in the description of politics, unconventional opposition, and social movements. Thus they confirm us in our belief that it is useful to make the analogy more explicit. Second, all of these articles provide rich illustrations of a number of the processes that must be accounted for. Yet from our point of view they are limited, because they do not carry the analogy far enough. That is, they either limit themselves to a narrow range of organizations (e.g., voluntary organizations in Selznick [1943], hierarchic organizations in Leeds [1964]), or they limit themselves to a narrow range of social movement phenomena (e.g., mass movements in Lammers [1969], bureaucratic insurgencies in Leeds). Our task is more encompassing: it is to set the determinants of the occurrence and outcomes of the broad range of types of social movements in the full range of organizational settings.

Perspectives on Social Movements

Since our approach draws consciously on analogical techniques, the description of social movements in organizations employs the descriptive dimensions of social movements in society. A social movement is the expression of a preference for change among members of a society. Social movements differ in the *breadth of support* within the society (people who believe in the goals of the movement, contributors of time and money, activists and/or cadre), in their *goals* (the extent of change required and the level of concrete specification of goals), in the *strategy* and *tactics* of their carrier organizations, and in the *location* of their supporters in the social structure. (These dimensions are elaborated below.)

There is disagreement among sociologists about the appropriate or most important analytic mechanisms for explaining the growth, decline, and change of social movements. Some sociologists emphasize the distribution and intensity of grievances and deprivation; others emphasize the role of

ideology; still others focus on the processes of resource mobilization and social control. We draw upon the resource-mobilization perspective.

The emerging resource-mobilization approach to social movement analysis (Oberschall 1973; McCarthy and Zald 1973, 1977) has two components that make it especially useful. First, unlike the earlier collective-behavior and social-psychological approaches, it examines the cost of participation in social movement action as well as the distribution of grievances. That is, it examines what Oberschall calls the risk-reward ratio. Second, it treats mobilization of resources from whatever source as a central topic. This is especially important in the study of social movements in organizations because such key resources as votes of stock, police intervention, legal support, board member votes and strike funds may come from outside the group of immediate protagonists. Amount and sources of resources and risk-reward ratios are then combined with dimensions of organizational structure and process to explain the forms and rates of social movements in different organizations.

The resource-mobilization approach is closer to a political sociology than to a collective-behavior tradition. First, it treats many of the issues that social movements deal with as rooted in the enduring cleavages, status relationships, collective definitions, and traditions of the social system. Second, it examines the infrastructure of society that supports or hinders the development of collective action. Infrastructure supports include means of communication, work schedules, and discretionary resources. Thus the preexisting networks, associations, and organizations of involved groups may facilitate or hinder collective action. Finally, the resource-mobilization perspective is concerned with the responses of authorities and the multiple linkages among authorities, partisans, and reference elites (Gamson 1968; Lipsky 1968).

This last point is especially important in understanding both the choice of tactics and the outcomes of social movements. Tactical choices are dependent upon prior relationships between partisans and authorities, the degree of consensus on goals between partisans and authorities, the amount of trust, and the relevant resources that partisans control. Authorities, for their part, must respond to partisans. They attempt to control or facilitate social movements depending upon their resources and capabilities for control and the extent to which social movement goals are consonant with their own goals. Since one purpose of our analysis is to account for social movement outcomes, analysis must concentrate upon the situation, the response of authorities, and the social movement itself.

Organizational Polities and Politics

Societies vary in their forms of government. The terms "tyranny," "democracy," "federation," and "republic" are crude descriptions of forms of gov-

ernment. So too do organizations vary in their forms of government. Many of our words describing them parallel those for societies. "Bureaucracy," "corporation," "federation," "cooperative," "voluntary association"—all have an element descriptive of the form of rule of an organization. Just as the political processes of society can be characterized in terms of the rights, duties, and privileges of differently situated individuals and groups in relation to each other and to the formal institutions of government, so too do the organizational constitutions define rights and duties of members and officers in relation to decisions and behavior (see Swanson 1971; Zald 1970*a*, 1970*b*).

The organization constitution defines legitimate mechanisms for attempts to reach decisions and allocate resources. Behavior within these legitimate channels, processes, and positions is termed "conventional politics." Unconventional politics are attempts to reach decisions or effect the allocation of resources outside these legitimated channels, processes, and positions. Unconventional politics include both proscribed or "illegal" behavior and behavior toward which the constitution of the organization is largely neutral. Social movements in organizations are forms of unconventional politics.

Elsewhere the major components of polities and of political analysis of organizations have been treated in some detail (see Zald 1970*a*, 1970*b*). Here we comment on three aspects of organizations that are critical to our later analysis: the dispersal of power and authority, the relation of organizations to the larger society, and the choice of action options for members situated in different positions in the organizations.

The living constitutions of organizations differ in their dispersal of power and authority. Our crude nominal labels such as "corporate hierarchical," "federation," and "voluntary membership association" often relate to the constitution of authority and the claims of organizations over their members. Thus a corporate organization is one in which the corporate officers, no matter how selected, have rights of discretion and disposal over the resources of the organization. "Hierarchy" refers to clear strata in the organization, to superordinates' and subordinates' statuses. On the other hand, a federated organization restricts the rights of officers: in contrast to corporate hierarchical forms, federated organizations retain rights for the units and rights for the selection of officers that are not gainsaid in the corporate form. *Within* federated or corporate forms there may be wide variations in the internal distribution of rights. Both universities and businesses may be examples of corporate hierarchical organizations, but traditions and norms of tenure in the former and requirements of corporate loyalty in the latter may lead to a much more open politics in the university than in the business. Similarly, many labor unions and the New York Stock Exchange can be

characterized as federations, yet the extent to which central officers can intervene in operations of the peripheral units may vary widely.

Obviously the corporate-federated distinction does not exhaust the analysis of organizational constitutions. Other components are the inducement-incentive contracts of the organization that set the terms of what members expect of the organization and vice versa and the goals and means of the organization that set the boundaries of legitimacy and use of resources.

Organizations are webbed by the larger society. Thus the relationships of members in authority are conditioned by the discretion and responsibility imposed on some organizations by the larger society. Rights of tenure, conditions for dismissal, use of punitive sanctions, and so on may be determined or limited by the larger society. At a very general level, the conditions for organizational discretion in relationship to goals, products, and personnel are all conditioned by societal norms. More particularly, since the society may enjoin authorities within organizations from punitive actions or require the organizations to permit or even encourage group formation, the constraints imposed by the larger society may provide a powerful stimulus for social movement formation. Similarly, laws surrounding stockholder rights, the financing of proxy fights, and the like affect the cost of mobilization and the ability of organizational authorities to control and respond to internal social movements.

Finally, the formation of social movements in organizations is closely related to the microecology of aggrieved groups in the organization, their chances of remedies within the organization, their options for exit, and the costs and benefits of switching versus fighting (Hirschman 1970).[2] Organizations vary in the extent to which they control important incentives or sanctions desired or hated by members and the extent to which the members can easily exit and obtain the same incentives elsewhere. Sailors at sea must mutiny; soldiers can more easily desert. Because of differences in ownership of property and in organization, members of Protestant congregations can secede; members of Catholic churches face much greater difficulty in commanding the resources to create a new church (fellow members are more committed to the particular structure, and the central organization owns the physical facilities). Younger officers have too much invested in the

[2] Albert Hirschman's *Exit, Loyalty and Voice* aims at predicting when individuals within organizations protest and when they leave. Its subtitle, *Responses to Decline in Firms, Organizations and States*, indicates the generality of the choice between exit and voice. Moreover, several students of national politics have found his book, though concerned primarily with organizations, to be equally applicable to the expression of choice in nation-states. The strength of Hirschman's analysis is that it forces us to think of two modes of expressing discontent (exit and voice) together, whereas most of us have treated these separately. Economists have focused on the exit option; political scientists have focused on the voice option. For several papers on exit and voice, see *Social Science Information* (1974, 1975).

military to resign; an officer with 20 years of service can retire at half pay. All these are examples of situational determinants which affect the choice of organizational members to engage in social movement action or to leave the organization. They are situational determinants of the forms in which discontent is expressed.

Given the range of polity forms, the extent to which society may or may not impose constraints upon particular types of organizations that facilitate or hinder internal social movements and the microecology of groups and individuals, it is beyond the scope of this paper to offer a general theory of social movements in organizations. The variety of social movements and the range of polity situations are just too great to be fully analyzed in a single essay. Instead, we devote most of our comments to social movements in corporate hierarchical organizations. We discuss movements in federated and other organizations only briefly, to indicate the direction a fuller analysis would take.

SOCIAL MOVEMENTS IN CORPORATE HIERARCHICAL ORGANIZATIONS

Corporate organizations are those in which the units and facilities are "owned" by the group legitimated as the corporate office. That is, legitimate authority resides in the center (e.g., with board of directors or chief executive officers). Among themselves corporate organizations may vary in the extent of decentralization-centralization of this legitimate authority. Federated organizations, on the other hand, are those types in which the units (locals, departments, chapters, or partners) have clear property rights and discretion which is established in the constitution of the focal organization (and possibly backed up by force of law). Moreover, they may have legitimate rights in the selection of executives and the establishment of policy. Voluntary membership associations, in contrast, can be either corporate or federated, but their distinguishing characteristic is that the members contribute resources upon which the central authorities depend. Thus authorities of voluntary associations have less discretion vis-à-vis organizational policy and are often in a precarious position vis-à-vis members (Clark and Wilson 1961). Federated and voluntary organizations might therefore be called "upside down" organizations, for the flow of dependence is inverted from our conception of hierarchically arranged organizations.

All of these organizations may experience social movements. Hierarchical organizations differ from others in two major regards which have consequences for their political life. First, concentration of authority and power in a hierarchic structure means that there are higher costs of dissent and departures from organizational policy. Second, in such organizations subordinates have less normal access to the choices of major policy and of successors to executive offices. Conversely, in federated and voluntary

organizations, units and members control both the resources that can be devoted to combating policies and legitimate access to organizational choice. This leads to two general and interrelated propositions: (1) federated and voluntary organizations are likely to have more open politics than corporate hierarchical organizations; (2) when unconventional politics take place in corporate hierarchical organizations, conspiratorial forms are likely to be more prominent.

The argument is not that conspiracies are absent from federated organizations or voluntary associations. Where the stakes are high, a minority may reject the well-institutionalized channels of a federated organization in attempting change. Nor are we arguing that corporate organizations are likely to have more phenomena resembling social movements. By raising the probability of success or lowering the costs of participation, open political systems may encourage unconventional political behavior. The major thrust of our argument is that there is a push to conspiracy in corporate hierarchical forms. Let us discuss three types of social movements in corporate organizations: coups, insurgencies, and mass movements. For each type we define the phenomena, specify determinants, discuss process, and describe likely outcomes. Illustrative hypotheses are given.

Organizational Coup d'Etat[3]

In his book *Coup d'Etat*, Luttwak (1969) argues that a coup is not an assault from the outside; it is a seizure of power from within. Second, he points out that a coup is usually politically neutral, with no immediate goal other than succession. Whatever policy emerges is a matter for the postcoup regime. Finally, he asserts that the technique used in a coup is not to confront, overwhelm, or smash down the adversary through sheer weight of power. On the contrary, it is a technique of judo in which the opponent's own advantages of weight and balance are turned into weapons against him. Luttwak therefore defines a coup in a nation-state as "the infiltration of a small but critical segment of the state's apparatus which is used to displace the government from the control of the remainder" (p. 12). Many coups involve subalterns, but a coup may involve one segment of a ruling junta ousting the current head of state.

By analogy, an organizational coup can be defined as the infiltration of a small but critical group from within the organization's structure to effect an unexpected succession. The term "infiltration" is used to denote the secrecy with which the plan is carried out. The small but critical group includes the palace guard of the chief executive officer (CEO) or some combination of inside and outside directors. The primary goal is succession,

[3] This section is drawn from a paper by Berger (1975).

though less proximal goals of policy and change are implied in the political action.

Organizational coups can be contrasted with expected replacements (such as limited terms in office or retirement) and sudden successions which result from death. In the former, the succession is expected and conducted with a high degree of public awareness. In the latter, the succession is unexpected, yet once the initial shock of death wears off, replacement activities follow the regular institutionalized procedures. Organizational coups, on the other hand, are unexpected and deviate substantially from routinized procedures. That is to say, they are planned and executed without the CEO's knowledge or public awareness; otherwise the CEO would be able to retaliate. In addition, they involve the quick appointment of a new successor, which is at variance with the more usual procedure of search, trial, interviews, and selection. Five examples of organizational coups have been uncovered; here are brief illustrations of the climactic events in three of them.

> [*Interpublic:*] For Marion Harper, Jr., the sky fell shortly after 10:00 A.M. on Thursday, November 9, 1967. The setting was the windowless boardroom of the Interpublic Group, the world's largest advertising business, on the forty-fourth floor of Manhattan's Time-Life building. The occasion was a special meeting of Interpublic's board of directors with Harper presiding as chairman and chief executive officer. . . .
>
> . . . For nineteen years Harper, now fifty-one, had been in charge of the burgeoning organization. His energy, his ideas, his ambitions had pushed, dragged, bulled Interpublic into a $700 million business. . . .
>
> A twelve page agenda had been prepared and distributed in advance. . . . When Harper brought the first item before the board, one of the directors interrupted and requested permission to question Taggart, the chief financial officer, on the group's current financial situation. . . . Harper, suspecting nothing, allowed the question. . . . Another director spoke. Taking note of the gravity of the situation he moved that the first order of business be the replacement of the chief executive officer. The motion was immediately seconded. The vote: six ayes, one abstention. [Wise 1968, p. 136]

> [*Ford Motor Company*] It was a press conference compounded of equal parts of vinegar and butter. The scene was a Detroit gathering called by auto mogul Henry Ford II last week to announce that the Ford Motor Company board of directors had voted to relieve President Semon E. Knudsen of his duties.
>
> The bitter taste stemmed from Knudsen's calling of his own press conference an hour and a half earlier to say he was "puzzled by this sudden and unexpected action." Beaten to the punch, Chairman Ford was maneuvered out of the standard references in such situations to "a mutual understanding" and was forced to fall back on . . . "Sometimes these things just don't work out."
>
> The butter, on the other hand, was what wouldn't melt in the mouth of Knudsen's arch-rival, ambitious Lido A. Iacocca, whose satisfied smile was in sharp contrast to the sober expressions on the other Ford executives present. ["Behind the Palace Revolt at Ford" 1969, p. 138]

> [*RCA*] Robert W. Sarnoff always had a tough act to follow. His father, David, a one-time wireless operator who rose to brigadier general, had taken a relatively small company in the fledgling electronics business and, over four decades, built it into the giant RCA Corporation. Bobby—he was never able to shake either the nickname or the label of "the general's boy"—had ideas of his own, and after his father left the top spot at RCA, he pursued them vigorously. But two recessions and one gross miscalculation took their toll—last week, in a move that shocked Wall Street and company officials, RCA's board of directors ousted Bobby Sarnoff, 57, as chairman and chief executive officer. ["His Master's New Voice" 1975, p. 79]

In the Ford Motor case and the RCA case, the stories relate how senior executives organized and presented a bill of particulars to outside board members and key stockholders and pushed for the coup. In the most recent case we have found, that of the firing of Franklin Jarman of Genesco, a similar conspiracy from below is reported (Mullaney 1977).

Determinants.—What are some of the conditions that make coups more likely in one kind of hierarchic corporate structure than in another? First, the subalterns must be quite dependent on the executives for their positions. The ethos of business corporations is to stress executive loyalty at the same time senior executives serve on the sufferance of the CEOs (Kanter 1977). If they did not serve on the sufferance of the CEO, they could criticize him. On the other hand, in universities deans and professors have tenure and can call for the resignation of the president. While there might be some negative consequences, they cannot be fired. We have not found any cases of coups in universities. We have found several cases in which deans and/or professors have openly called for the resignation of the president or the circumscription of his role.

Second, a coup, in which subordinates want one of their own to be the CEO, cannot work in organizations in which the CEO is often or usually brought in from the outside. One could conspire to have a university president forced out, but the conspirators would not have a guarantee that the successor would come from inside. The same argument applies to metropolitan school systems. In contrast, in business corporations, especially large ones, the CEO is rarely brought in from the outside. Third, the conspirators need access to those board members who control the key resources, votes. And some board members may be more important than others. They may control large blocks of shares or major lines of credit or contrast. Fourth, corporations often have officer-directors. Unlike almost any other corporate form, there is often continuing interaction between the CEO's subalterns and his employers (indeed some subalterns may even serve as "employers" if they are internal directors).

Finally, when are coups d'etat most likely to occur? Chief executive officers with recent records of bad decisions and poor performance are more likely to be faced with coup attempts than others. Yet even CEOs who have

decent records of profit performance are not immune if they have created enemies. We have found several cases in which interpersonal hostilities and power battles led to the coup. For instance, if the CEO has recently taken steps leading to the demotion or lowered power of subalterns, it is likely that he has incurred their enmity. Until the demotion is fixed and they are isolated from power, the potential for a coup attempt is very high.

Processes of interaction.—Regardless of the sources of their grievances, once a conspiratorial group has agreed to attempt to force out an executive, it must gain access to key board members or stockholders to carry the battle. Two authority processes must be considered. First, the CEO must be neutralized. Otherwise he can counterattack either by mounting a persuasive argument or by isolating and dismissing the conspirators. Second, board members and stockholders must be convinced. They must see the high cost of maintaining the current CEO arrangement. In two of the cases we have examined, board members had been contacted at earlier times but took no action then. The second time around, a more persuasive case was made. Moreover, it is clear that the CEOs were always surprised by the action and were often out of touch with their offices. In the RCA case described above, Sarnoff was visiting plants in Australia when the conspiracy took place. In the Genesco case, Franklin Jarman was on his honeymoon in Jamaica.

In other cases a coup attempt may occur when the CEO is sick, and a sick CEO may recognize that a force out is coming. For instance, recently Donald Kircher was replaced as head of the Singer Manufacturing Company (Hough 1975). He had warned subalterns and the board a year or two earlier that any attempt to use an interim management arrangement to replace him while he was sick would be fought. But when he became sick again and realized that he would not be able to resume the mantle, he easily gave up the office.[4]

Outcomes.—What are the outcomes of a coup? At least two types must be distinguished: the consequences or results for the participants and the consequences for the structural operation of the organization. The coup attempt may fail, as when conspirators marshal support but board members dismiss the attempt as self-serving for the conspirators, and the CEO isolates or dismisses them. If successful, the coup may lead to a change in personnel, a change in the system, but have few other consequences, little change of the system. Students of coups in nation-states have remarked that coups often have little impact upon the larger society because they are not related to any underlying structural change. Coups in organizations

[4] CEOs who are forced out often receive remunerative severance contracts to erase their sorrow: appointment as consultants with no duties and pay close to what they received while in office are common.

may have relatively few consequences below the elite level. The coup leads to a change in the chief executive and possibly to a few shifts down to the assistant vice-president level. Beyond that there is no necessary change. Product lines and company policy may be only tangentially at stake. Whether a coup leads to change other than personnel shifts depends upon the connection of the coup to any underlying trends in the organization and its environment. The coup is very important to the immediate participants, but the irony is that a company's long-range profitability is heavily dependent upon industry profitability and the position of the company in the industry. Within the normal range of company performance, who becomes CEO is unlikely to have great impact upon the company (see Lieberson and O'Connor 1972).

In the cases we have examined, organizational coups do not relate to political issues in the larger society, but they do relate to societal or industrial change. First, the CEO's stance on major strategies may have been found to be in error (e.g., the RCA case). Second, a coup may relate to hostile merger or takeover bids. Takeover and merger bids may lead to conversations between the company that expects to take over and factions in the executive office. Thus forcing out a CEO may be partially related to political factions within the organization more or less favorable to the company buying it out. The parallel at the nation-state level occurs when the coup anticipates loss of a war. The coup puts in office a CEO and ruling group more favorable to the potential occupying power or prepares for continuation of the war.

The main themes of our analysis of coups can be expressed as a set of hypotheses (one each for occurrence, process, and outcome).

> *Hypothesis 1:* In corporate hierarchical organizations that (*a*) do not protect the positions of senior executives and (*b*) do promote within, (*c*) provide senior officers access to board members, and (*d*) experience poor performance or other undesired situations attributable to the CEO, coup d'etat attempts are more likely to occur than in other types of corporate organizations.

> *Hypothesis 2:* Successful coups are facilitated by surprise CEO neutralization and by the support of the more powerful board members and shareholders.

> *Hypothesis 3:* The first-order effects of a coup are the reshuffling of executive positions; larger changes in the organizational system are rare.

Bureaucratic Insurgency

Bureaucratic insurgency differs from a coup in its target: its aim is not to replace the chief executive but to change some aspect of organizational function. It differs from a mass movement in extent of support and number of adherents. It resembles a coup in that for much of its duration it may be

conspiratorial. It resembles some mass movements in that its goals are limited to change in specific aspects of the organization. In nation-states insurgency, as we are defining it, is analogous to the action of a pressure group or professional movement (McCarthy and Zald 1973). It typically involves a limited mobilization of personnel. At the organizational level, bureaucratic insurgency in corporate organizations is an attempt by members to implement goals, programs, or policy choices which have been explicitly denied (or considered but not acted upon) by the legitimate authority of the focal organization. The activity of the insurgents therefore takes place outside the conventional channels of politics of the organization.

Determinants.—Bureaucratic insurgency is most likely to be found in organizations or organizational units which have strong normative elements or organizations which are dependent upon staff who have strong professional-ideological and moral commitments. These normative commitments provide personnel with reference bases to evaluate organizational products, priorities, and procedures. (On normative, coercive, and utilitarian compliance bases, see Etzioni [1961]; see also Clark and Wilson [1961] and Zald and Jacobs, in press.) The participants in bureaucratic insurgency may also include the subalterns of executives, but usually they range deeply into the organization, throughout middle management and, in organizations using professionals on the line, down to the line staff.

When the desired activity or change has been explicitly denied, bureaucratic insurgency may take the form of conspiracy. Here the insurgents know they are pursuing disapproved lines of action (i.e., using organizational time and resources in ways which have been countermanded by authority). If the insurgency is reformist or narrow, discovery of the conspiracy may lead to repression, not necessarily expulsion.

Often, however, insurgency operates in gray areas where organizational behavior has not been explicitly prescribed. Thus insurgents attempt to establish their own definition of the situation or shift the weighting of priorities. Bureaucratic insurgency is also aided by factionalism or sympathetic support from the organization elite. For example, members of the control apparatus may overlook information which would suggest that a conspiratorial insurgency is actually taking place. The well-known phrase "You can do it, but I don't want to know about it" is a case in point.

A conspiratorial bureaucratic insurgency is thus facilitated by a low capacity for surveillance on the part of the central authorities. The effect of limited surveillance is compounded by organizational complexity; the larger the organization, the more diverse its structure, the more autonomous the units, and the more imprecise the reporting system, the more likely it is that an insurgency can continue for long periods of time.

We can delineate three major subtypes of insurgency: program or product development, whistle blowing, and policy choice. In program- or product-

development insurgency, the insurgents accept the overall structure of authority in goal setting but attempt to introduce new techniques for accomplishing goals or refinements of organizational programs. Middle-level officials or line professionals with discretion in allocating organizational resources, the insurgents pursue their own concept of organizational programs or product development while watching over their shoulder for elite interference. Social workers running group programs or allocating welfare funds, army officers developing the armed helicopter, engineers developing the air-cooled engine at General Motors, or HUD bureaucrats attempting to push cities to have more racially integrated housing may all operate and be involved in this first type of bureaucratic insurgency.

Whistle blowing is a form of insurgency in which an insurgent deviates from loyalty norms to describe the disjunction between organizational functioning and public expectations. Whistle blowing may require only one insurgent. For instance, A. Ernest Fitzgerald, a systems analyst for the Defense Department, testified to the Senate on the massive cost overruns in building the Lockheed C-A5 cargo plane ("Defense: Ernest Fitzgerald RIF" 1969). Fitzgerald acted by himself, but more organized whistle blowing is feasible. Underground newspapers can serve as outlets for private information that will discredit the elites.

A third type of bureaucratic insurgency focuses upon the main goals and policy choice of the organization. In 1949 the "admirals' revolt" questioned policies of the Defense Department that seemed to lead to a diminished role for the navy ("Revolt of the Admirals" 1949). Once the insurgency came out in the open, it began to resemble a mass movement. The difference between mass movement and insurgency lies in the greater openness of the former, the number of people involved, and the extent to which multiple organizations or units are involved. The admirals' revolt was an insurgency at the top that was taken to higher authorities, Congress and the public. (The route was to Congress because the Defense Department was controlled by the executive.) One suspects that this form of large-scale and open insurgency at the top occurs only in corporate situations in which the insurgents have some degree of protection (e.g., good retirement plans or high job mobility) or in which the issue, the reward, the prize, is vital to the interests of the contesting elite.

External support and authority response.—Whereas the organizational coup d'etat requires the involvement of key board members, a wider range of external supports is necessary in insurgency. For instance, whistle blowing is fully dependent upon external support, for by definition organizational authorities have been unresponsive and typically have attempted to quash complaints. The insurgency may also be aided by explicit material support. Army officers developing the armed helicopter cooperated with small machine-tool companies which helped the officers machine and construct

weapon supports and modify the helicopters. Bureaucratic insurgents may also arrange for client groups to request procedures or programs which ultimately serve the insurgent's *and* the client's ends.

If monetary or technical resources are not required, the major support is likely to come through professional and movement perspectives. That is, the insurgency is fueled by societal social movements and a professional ideology which has been previously learned or is currently fashionable. The social and ideological support of a radical caucus or an association of radical urban planners provides such reference support (see Ross 1975).

The response of authorities to insurgency depends at least in part upon their perceptions of the opportunity costs of compliance and the extent to which the insurgency is defined as compatible or incompatible with elite preferences and priorities. Thus some insurgents may be aided by some members of the elite; others may be seen as so removed from elite operative goals that the elite's aim is to quash the insurgency.

Outcomes.—Outcomes of insurgency include failure and repression, continued segmental operation, enclave support, and total incorporation.

1. Failure and repression: when authorities discover insurgency and find it opposed to their definition of organizational priorities, they may suppress or disband the insurgency. Members may be expelled from the organization or punished. Officers and priests can be sent to undesirable posts; they can be forbidden to pursue their line of action. However, authorities must calculate not only the degree of threat presented by the insurgents but also the consequences for relevant others, both in and out of the organization, of taking a given line of punishment against the insurgents. To the extent that the insurgents are in fact moral exemplars in the organization, overreaction leads to the possibility that the authorities themselves will be discredited.

Even if the insurgent group is disbanded, however, it can still bring about changes in an organization. If the authorities or other members of the organization take over the decision premises and orientations of the insurgents, over time the organization may change. (Such a paradox parallels the impact of social movements in society: for instance, the Populist movement in the United States gained many of its goals even though many of its leaders never attained power.)

2. Segmental operation is the maintenance of the insurgency over long periods of time with no formal recognition from the center and without change in organizational products or goals. It occurs most often in organizations with multiple goals where there are continuing ambiguities and dilemmas in elite control.

3. Enclave support is likely when external pressures lead the executives to recognize and tolerate the insurgency. This outcome (as in demonstration projects or the setting up of separate departments or units) is likely if

organizational goals are multiple and units are only partially interdependent with each other. The formal recognition of an insurgency is similar to Leeds's (1964) process of "protest absorption," but she addressed a much narrower range of phenomena.

4. Total incorporation of the insurgency depends upon the ability of the insurgents to get executive compliance or agreement. If over time the developed program or product can be shown to be consonant with the executive goals, the organization may fully incorporate insurgents' perspectives, and the insurgents may be promoted. Such total incorporation eliminates the raison d'être of the insurgency. The adoption of the armed helicopter and related strategies of mobile infantry as a major component of warfare is an example of a totally incorporated insurgency.

Three hypotheses summarize our argument about insurgency:

> *Hypothesis 4:* Insurgencies are most likely to occur where professional and normative commitments provide an independent base for perspectives on goals, products, and policies.
>
> *Hypothesis 5:* The more complex the organization and the more difficult the surveillance, the longer the duration of insurgency.
>
> *Hypothesis 6:* The outcome of insurgency depends upon the extent to which the insurgents threaten authority and the costs to authorities of suppression.

Mass Movements: From Protest to Rebellion

As noted earlier, bureaucratic insurgencies may span many levels in an organization. They range from small cabals at the top to the concrete and concerted efforts of middle-level managers and professionals. When small, they may have a conspiratorial cast. On the other hand, as they become more organized, they may develop coordinating committees or caucuses which begin to resemble mass-action movements. As their numbers grow and their tactics move toward withdrawal of labor, petitions, and boycotts, they begin to resemble mass movements.

Mass movements at the nation-state level range from movements of protest to rebellion. They are defined as collective attempts to express grievances and discontent and/or to promote or resist change. They vary in goals from those aimed at melioristic change of specific practices or rights to those aimed at redefining the distribution of power, constitution of rules, and norms of society. By analogy, mass movements in organizations range from the expression of minor grievances (not previously acted upon by authorities) to major attempts to seize control of the organization.

Mass movements differ from insurgencies and coups in number of participants and the visibility of their actions. Coups and insurgencies may be restricted to face-to-face groups and become visible only toward the end of their histories. Mass movements, on the other hand, may be initiated by a

small group, but larger numbers must be mobilized if the initiators are to gain their ends. Examples of mass movements are work slowdowns, wildcat strikes, mutinies and secessions, mass desertions, and prison riots.

It is important to note that, while the proximal goal of the movement is to change the behavior and goals of organizational authorities and the structure of organizations, the real goal may be changes in the larger society. Thus the student movement disrupted the University of California at Berkeley, Columbia, Harvard, Michigan, and Wisconsin and had as proximal targets changes in administrative behavior. But the real goals were to change the behavior of Lyndon Johnson and Richard Nixon or, in more radical form, the structure of American society. Similarly, a plant seizure or strike may occur to achieve concrete gains, but it also may be a weapon for changing governmental policies or the government itself.

A paradox confronts us in thinking about mass movements in organizations. In discussing coups and insurgency, we drew on sparse evidence; on the other hand, when one turns to the study of mass movements, the literature appears rich, systematic, and quantitative. After all, economists and sociologists have been studying the factors related to strikes, industrial conflict, and unionization for two generations. And more recently quite sophisticated quantitative analyses have decomposed the determinants of the numbers, breadth, and duration of strike activity (Britt and Galle 1972). Yet the first glance is deceiving. For strikes need not represent the social movement-like phenomenon of unconventional politics. They may represent a fully institutionalized aspect of the collective-bargaining process. As such they are part of the normal and institutionalized political system of organizations.

Winning union recognition and the legitimation of strikes is part of a social movement process in society and in specific organizations. Yet the union has now become one of the political mechanisms for aggregating preferences and resolving conflicts in the organization. Snyder (1975) has shown that one can predict aggregate strike behavior from aggregate economic variables *after* a collective bargaining system has been institutionalized. From our point of view, institutionalized unions that engage in strike action are part of normal politics.

Of course there are some parallels between industrial strikes and mass movements in organizations. Both involve mobilization of workers and the calculation of costs and benefits. Yet the mechanisms for mobilization, the costs of organizing for collective action, and the extent of organization and societal support are different enough so that we cannot incorporate wholesale the literature on industrial conflict in our explanation of organizational mass movements.[5]

[5] The problem here parallels an analytic gap in the study of social movements and politics in society. Party politics and the aggregation of preferences through the institutionalized

Determinants of mass movements in organizations.—Olson (1965) has taught us that mass movements aim to provide collective goods. The provision of collective goods entails free-rider effects; since the goods will be provided to all, individuals have little incentive to work for their provision. Free-rider problems are overcome in coups and insurgencies: in both forms the benefits accrue to a small group of participants, and in insurgency social control and solidary incentives, coupled with career incentives and normative commitments, suffice to sustain the insurgents.

The question becomes, What organizational conditions facilitate mass action? Our perspective on resource mobilization and the interplay between organizations and their societal environment leads us to five hypotheses about the relations of size, homogeneity, vertical segmentation, exit options, and associational density. All of these hypotheses assume grievances or a gap between the current situation and desired alternatives. (Since our analysis here is more detailed than our earlier discussion, we couple hypothesis and analysis rather than presenting the hypotheses in summary form.)

> *Hypothesis 7:* The larger the size of the subordinate group, the greater the probability of organizational mass movements.

Classical theory and recent studies of riots in cities lead us to expect that larger subordinate groups in organizations are more likely than smaller groups to develop into social movements. There are several reasons for this. Larger groups are more likely to include some members who develop grievances. Larger groups are more likely to be cut off from superordinates (see below). Larger groups can hide dissidents from authorities; they are more impervious to social control. Moreover, larger subordinate groups are more likely to generate associations within them. In a study of prison riots and confrontations in the largest prisons, Wilsnack (in press) finds that the very largest (over 1,000 inmates) were more likely than the smaller ones in his sample to have experienced nonriot resistance (confrontation, refusal to work). Similarly, Peterson (1968) finds that large campuses were more likely than small ones to have been involved in antiwar protests. For most industries we would expect that large establishments are more easily organized than smaller ones.

> *Hypothesis 8:* The greater the homogeneity within the subordinate group, the more likely that subordinates will challenge superordinates.

On one hand, group consciousness is facilitated by homogeneity and shared values. On the other hand, heterogeneity may facilitate conflict

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political process are treated separately from social movement emergence, growth, and change. Yet social movements with political goals influence the institutionalized system and may become part of it. Similarly, the institutionalized political process uses many of the same forms of mobilization as social movements.

within a subordinate group and the possibility of alliances between subclasses of subordinates and superordinates. This hypothesis follows Kerr and Siegel (1954). Thus we would expect that organizations with little occupational differentiation would be more likely to have superordinate-subordinate conflicts.

> *Hypothesis 9:* The greater the vertical segmentation, the more likely that grievance channels, mobility channels, and communication channels in general are blocked.

This hypothesis has been proposed in the social movement literature (see Oberschall 1973) and offered as an explanation for strike proneness (see Kerr and Seigel 1954). Of course it dates back at least to Pareto. In organizations in which occupational status and ethnic class status are overlaid, we would expect segmentation to be especially strong and both within-group solidarity and the chances for resentment to be high.

> *Hypothesis 10:* The more difficult or costly it is to exit and the greater the commitment to the incentives of the organization, the more likely social movements are to form.

In some ways this hypothesis, which just restates Hirschman's (1970) exit-voice thesis, parallels Thibaut and Kelley's (1959) analysis of "comparison-level alternatives." Thibaut and Kelley, of course, were interested in whether people continued their line of action within a social interchange or moved outside that interchange. It is apparent that different personnel in organizations and positions are confronted with differences in comparison-level alternatives. The value of the comparison-level alternatives can be broken into two components—the cost of exit and the value, negative or positive, of the perceived option once attained. Comparison level is the net value of the current situation or line of action minus the cost of exit and the value of the goal, once obtained. Some organizations have extremely costly exits. Military institutions raise the costs of exit very high. Nevertheless, it is apparent that exit is less costly in the army than in the navy at sea. Under relatively similar levels of disaffection and grievance, we would expect soldiers to mix use of desertion and protest, whereas sailors at sea use only mutiny. Blake's (1976) study of military resistance to the Vietnam war reports cases of mutinies in military prisons and among troops on the line (refusals to move into combat), but it is our impression that the number of actual mutinies on the line is much lower than in the more contained prison situation. All of the mutinies that Lammers (1969) studied were naval ones. There are, of course, cases of whole units of armies deserting; these should probably be seen as secessions. They are not protests of the bottom against the top; instead, they are led by dissident officers who disagree with either the treatment of their units by the authorities or the policies of

authorities in prosecuting the war and in relation to subject populations (see Solzhenitsyn 1973).

Earlier we noted that strike rates do not necessarily reflect social movements in organizations; they do, however, reflect disagreements with management offers or existing conditions. With regard to the hypothesis under consideration, Stern (n.d.) finds a negative correlation, over communities, between quit rates and strike rates.

A final derivation from the exit-voice hypothesis can be suggested. The classical situation for exits is free markets with low costs for information and transportation. There buyers or sellers easily compare options, and consumers switch from seller to seller with no protest. However, our hypothesis suggests that consumer protests increase as monopoly increases—a relationship amply confirmed by American history—and monopoly can be defined as the opposite of exit choice. Monopoly exists when a single seller controls the sale of a good and there are no close substitutes for it.

The exit-voice hypothesis has a strategic role in accounting for the structural conditions of mass movements in organizations. First, it allows us to account for variation within an industry (defined as a set of organizations offering somewhat similar products) of two forms of expressing dissatisfaction, exit and voice. Second, it helps to provide an answer to Olson's question: Why collective action, when collective action is accompanied by free riders? The implication of Hirschman's argument is that, if one option for individuals, exit, is removed, an individual must weigh taking no action against taking some action that will promise a surplus of benefits. Stated differently, the removal of the exit option raises the comparative discounted benefits of the voice option. But Olson's challenge still remains. Our next hypothesis treats it more directly.

> *Hypothesis 11:* The greater the associational density and the higher the proportion of organizational participants who are members of associations, the easier it is to mobilize.

A major part of the critique of the mass-society theory of social movement has been that people who are most involved in voluntary associations and political organizations are also more likely to be involved in social movement action (see Oberschall 1973). There is supporting evidence in organizations. Although not dealing with social movement participation, Lipset, Trow, and Coleman's (1956) study of the International Typographical Union makes it clear that the associationally dense culture of the ITU facilitated political involvement. Similarly, studies of the student protests find that antiwar protest was correlated with the proportion of students in left organizations (Peterson 1968). And Von Eschen, Kirk, and Pinard (1971) found that early participants in the civil-rights-movement sit-ins were likely to be part of established, politically oriented organizations, often

student based. But if the hypothesis said only that members of ideologically committed associations were more likely to take action than members of uncommitted associations or ideologically committed individuals, it would not say much. Its real thrust is to argue that belonging to associations and networks eases the cost of information flow and mobilization. In relation to Olson's question, it suggests that, even if the associations are not formed to pursue the specific target of collective action, a dense associational field within a hierarchical organization facilitates internal social movements.

Our last hypothesis in this series relates to organizational recruitment of participants.

> *Hypothesis 12:* The more an organization recruits members critical of existing conditions, the more likely it is to generate internal mass movement.

Although obvious, Hypothesis 12 reminds us that issues and grievances within organizations are developed in the larger society. Since organizations may draw upon different segments of society for subordinates, "exactly similar" internal conditions may be responded to and redefined by recruitment of members from varying backgrounds. The issues of the multiversity and the war in Vietnam could have been defined as grievances in South Dakota and Alabama as well as at Wisconsin and Berkeley. That they were responded to differently in academic communities is probably as much a result of self-selection as the internal organization of universities. A similar argument about differential recruitment applies to the locus of social movements in prisons and in manufacturing industries.

The hypotheses about mass movements presented above deal with the factors facilitating mobilization within organizations,[6] but they do not deal with the goals of mass movements. Nor do they deal with the support and control of movements from both within and without the organization.

Goals, facilitation, and control.—Mass movements in organizations range from narrow protests against specific organizational practices (e.g., the quality of food in the cafeteria) to shopping lists of practices, calls for the restructuring of authority or changes in the relationship of the organization to other elements of the society, and, at the broadest level, the restructuring of society itself. Let us examine the interaction among the goals of the movement, indirect and direct support, and the response of authorities.

When goals are narrow, the movement and the response of authorities are usually only weakly linked to external factors, either ideologically or

[6] It should be apparent that many of these hypotheses might also be used to predict the determinants of coups and insurgency. E.g., exit options for senior and middle managers may relate to participation in coups and insurgency. We have elaborated upon them in connection with mass movements because we had more evidence and illustrations with which to support them. A general theory of social movements in organizations might well start with them.

materially. (To be sure, the standards for judging conditions internal to the organization may relate to the conception of adequate conditions in the society; e.g., what is adequate food in a prison in Turkey will not be seen as adequate in an American prison.) Indirectly, however, even narrow issues implicate the relationship of the organization to society. For example, the quality of food is related to the budget of the organization, and addressing specific grievances may be constrained by organizational resources and resource dependencies.

When the goals of the movement are broader, encompassing change in the authority structure of the organization in the larger society, we find the movement deeply dependent upon the larger society for the ideological acceptability of the goals; such dependency reshapes the response to the movement of external actors. In recent years, for instance, workers in England and France have seized plants which were about to be closed or have massive layoffs. Given the much more extensive nationalization of industry in these countries than in the United States and given ideologies of worker participation in management and socialism, such attempts at worker control have received both financial and political support. Similar moves to close plants in the United States are met with stoicism, some union bargaining, some community protest, but little governmental intervention or attempts to shift the criterion of authority or ownership.

External material and coercive resources may be mobilized by either the movement or authorities. Strikers are dependent upon several kinds of external resources—in particular, money for food and the honoring of the strike by other potential employees. Indirectly, the society supports or constrains the movement by its general provision of resources. For instance, Thieblot and Cowin (1972) have shown that the length of strikes in American communities is affected by ease of access to welfare payments. The easier the access, the longer the strikes. More directly, strikers may be funded by individuals and organizational contributions to maintain them. Coercive resources are provided or withdrawn by the police, military, and courts, who facilitate or inhibit authorities' and protestors' tactics of reprisal, boycotts, lockouts, etc. The provisional withholding of these coercive resources is based upon the general laws and attitudes governing their deployment and on the specific behavior and linkages between the movement and authorities and external groups.

The response of authorities to mass movements is affected by the goals and tactics of the movement, the autonomy of organizational authorities, and their own ideological predisposition.

Note first that authorities may have little discretion to respond. For instance, prison administrators are not autonomous; they can rarely respond positively to the demands of rioters or protesters. Indeed if the list of demands is long, the protesters recognize this dependency, and their first

moves are to ask for the involvement of political authorities. Similarly, in many countries (most Latin American countries, France, etc.) the universities are not autonomous of the ministry of education. A mass movement in the university, especially if it moves into the streets, is responded to by political administrators, not university officials. Where officials do have discretion, several strategic policies may be adapted. Lammers (in press), in a comparative study of the response of university authorities to student movements, lists four general combinations of strategies, tactics, and goals:

Strategies	*Tactics*	*Goals*
Repressive	Fight off	Elimination
Concessive	Buy off	Appeasement
Preventive	Stand off	Nonemergence and dissolution
Experimental	Join in	Copartnership

In the first, the authorities attempt to eliminate the movement and its leaders. They suspend students and use heavy sanctions. In the fourth, the authorities appear to buy in to the goals of the movement and move to align the organization with the goals of the movement. Lammers goes on to assess how different types of tactics will work in different types of universities. For instance, he is skeptical about the viability of the experimental strategy and the tactics of joining in in large universities, and he believes that preventive strategy allows universities to continue to carry out their educational and research missions with less disruption than the use of either concessive or repressive strategies. (He does not consider in detail the interaction of opposition and authority strategies.)

Outcomes.—In mass movements, immediate outcomes depend upon the ability of partisans and authorities to sustain conflict and the extent to which there are viable options for bargaining. If situations are defined in zero-sum terms, a sheer power calculus can be used. But in open-conflict situations, a variety of symbolic and partial solutions may permit both parties to win. Mass movements with specific and narrow goals may accomplish a change in the system of a specific organization, with little impact upon society. On the other hand, movements with broad goals are likely to fail unless they are part of a broader movement in the society at large.

The ability of partisans and authorities to maintain a conflict depends on their ability to mobilize resources and on the continuous availability of resources. This is especially important in understanding the difference between mass movements in such organizations as universities, with high turnovers of personnel, and those occurring in such organizations as factories, with greater continuity of personnel and organization. Thus, although in a university the movement may be at high tide in the spring, the lack of continuity of student generations and the departure of student leaders at the end of their senior year lead to a high cost of remobilizing resources. On

the other hand, workers in continuous organizations with a longer career span of leaders have fewer problems in maintaining continuity. In universities with off-campus student enclaves, however, developing a culture of activism which lasts longer than a student generation might lead to more movement continuity. Similarly, when student politics is more entwined in national politics, professional staff members from national political organizations (e.g., SDS, NAACP, CORE) may supply needed continuity.

As the continuation of the mass movement raises the costs to both members and authorities, search for settlement options takes place, much as in protracted conflict in society at large. Authorities may grant concessions, symbolic or material, or they may legitimate the movement and incorporate it in the decision structure. These would be considered successful outcomes.

The outcome of organizational rebellions may be failure. The reasons are straightforward: organizations are encapsulated in society, and an attempt to change an organization radically may strike at the heart of the authority relations of the larger society as well as in the organization. Such broad-ranging movements can succeed only where they are joined with existing conflict and rebellion throughout the society. However, the visibility of mass rebellions in organizations gives them a life beyond their immediate outcome. For instance, the Soledad Brothers and the events at Kent State live in the symbolic history of American social movements. They recall an ideology and conflict which reminds all adherents of the defined inequities and injustices in society. Therefore, to the extent that mass movements occur and have some continuity, they can lead to the enshrinement and reliving of dramatic events in the future.

Three hypotheses summarize our discussion of mass movements in hierarchical organizations:

> *Hypothesis 13:* Exit-voice options, subordinate homogeneity, vertical segmentation, selective recruitment, size, and associational density affect the costs and extent of solidarity, cleavage, and mobilization.
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> *Hypothesis 14:* The goals and duration of mass movements depend upon ideological and material supports in the larger society.
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> *Hypothesis 15:* Outcomes of the movement are dependent upon authority responses, continuity of movement participants, and relationship of the movement to movements in the larger society.

Comparative Summary

Earlier we mentioned several dimensions of social movements: breadth of support, goals, tactics, location in the social structure, relation to external support, and duration. Let us compare coups, insurgencies, and mass movements in corporate organizations along these dimensions (table 1 presents the comparison in brief form).

TABLE 1

DIMENSIONS OF SOCIAL MOVEMENTS IN ORGANIZATIONS

Dimensions	Movement Type		
	Organization Coups	Bureaucratic Insurgency	Mass Movements
Breadth	Small conspiratorial group	Medium-sized enclave or one whistle blower	Large group
Goal	Succession which may or may not lead to future change	Challenging the efficacy of existing norms to effect moderate organizational change	Expressing discontent and promoting or resisting narrow or broad goals
Main tactics	Infiltration and persuasion (e.g., using CEO's own record against him)	Violating rules and procedures without violence	Direct confrontation and possible violence
Activists' location in the organization's social structure	Organization elite	Middle managers and professionals	Lower-level participants
Linkage with external elements	A few key supporters, usually banking interests or key board members	Several important supporters beyond financial interests alone	Elaborate linkages of ideological and material support from society
Duration of overt conflict	Conspiracy may brew over long period; very brief actual coup	Varies; can last several years, depending on how long the organization can stand the nonconformity	Varies, from a day to several months, depending on how long the organization can stand the disruption and on the extent to which movement's members can mobilize for the conflict

Breadth of support.—Social movements may vary in number of adherents (believers) and number of constituents (individuals or organizations who act in support). As the previous analysis suggests, an organizational coup d'etat involves a small group (the palace guard). Bureaucratic insurgency and mass movement, on the other hand, may include a department in the case of the former and/or significant segments of the organizational population in case of the latter. Breadth is important, for it implies the amount of mobilization costs. In a coup, mobilization is restricted to a small cabal; therefore organization costs may be low. In contrast, insurgency may require the cooperation of a larger group of organizational participants, some of whom might not evaluate benefits to be worth potential risks. In mass movements, increasing breadth heightens the mobilization problem considerably, thereby leading to increased costs in organizing for collective action.

Goals.—Goals can be evaluated in terms of the amount of change desired, the extent to which it involves personnel or distribution of goods, and the extent to which it is a change in the system of authority and relationship to the society. In a coup d'etat, personnel changes are usually the only goal. Structural or policy changes may follow the unexpected succession, but these are not the primary aim of the movement. Furthermore, the goal of personnel replacement is concrete. Bureaucratic insurgency and mass movements, on the other hand, may involve objectives aimed at transforming the basic structure, policy, and resource distribution of the organization itself. As insurgency or a mass movement becomes more involved in the structure and policy of the organization, goals tend to reflect larger abstract values such as mission, justice, and equitable treatment.

Choice of tactics.—Choice of tactics (e.g., violence or nonviolence) depends on a calculus which includes legitimate resources, degree of access to or exclusion from legitimate channels, the cost of using violent tactics, and the perceived utility of tactics in accomplishing strategic goals. In coups the palace guard is likely to have considerable legitimate resources available in terms of their knowledge of internal affairs and their authority to direct large segments of the organization. In addition, their access to board members, years of experience, and cohesion lead one to hypothesize a relatively peaceful choice of tactics, though they may include the threat of resignations.

Bureaucratic insurgents possess fewer legitimate resources, are more excluded from authority, and experience greater risks (e.g., being fired) in the use of violent tactics. In this case, tactics are also likely to be secret and nonviolent. Mass rebellions, on the other hand, represent the farthest extreme. With few legitimate resources for bargaining and limited or no access to authority, tactics may include the use of violence and disruption of the normal functioning of the organization.

Location in the social structure.—Social movements differ in their location along vertical, horizontal, and sociodemographic dimensions. The vertical dimension refers to the location of adherents in the hierarchical system of the organization. The horizontal dimension includes both the spatial and functional differentiation of the organization. These differentiations provide the basis for the development of within-group solidarity and differentiated concepts of organizational mission, problems, and priorities. The sociodemographic dimension refers to the age-sex-social background of groups that combine to shape orientations and perspectives within the organization.

In an organizational coup, the palace guard is located close to the authorities. That is, the guard actually will be the elite in the status hierarchy and therefore experience very little spatial or functional differentiation. This lack of differentiation should lead to high within-group solidarity and congruence with the board concerning organization mission, problems, and priorities. Such congruence is bolstered by similar social backgrounds, age, and sex. In contrast, the orientation of the insurgent department is likely to diverge from that of the authorities and be predicated on the distance from them, on spatial and functional differentiation vis-à-vis other parts of the system, and on wider variations in age, sex, and social background. Finally, rebels will probably be most detached in their orientation from authorities due to high vertical separation, extreme spatial and functional differentiation, and wide variations in age, sex, and social background.

Linkage with external elements.—Coups require linkages with key stockholders, outside board members, or key financial supporters. Insurgencies, on the other hand, are fueled by ideological and professional support. Mass movements with broad goals are heavily dependent upon support from outside the organization. These include the cooperation of agents of control (police), material resources, and political and ideological support.

Duration.—Finally, the duration of conflicts varies considerably. The coup, though possibly brewing for a long period, is over in a few short hours; mass movement and insurgency, on the other hand, can continue for long periods of time.

In every case we have been considering in this section, there is a central legitimate authority and a relatively clear hierarchic distribution of power. We now describe social movements in organizations in which the central authority is usually weaker vis-à-vis the units or members.

SOCIAL MOVEMENTS IN FEDERATED AND OTHER ORGANIZATIONS

A complete analysis of social movements in organizations will include descriptions of the variety in social movements across the wide range of organizations that diverge on major constitutional dimensions, such as voluntary associations of different types (e.g., organizations with purposive

goals such as political parties, social movement organizations, special-purpose interest groups, professional associations, associations conveying solidary incentives). These organizations differ from corporate hierarchical organizations in several regards relevant to social movement and political processes. First, members and units typically stand in a different relation to the organization and to the center, the titular authorities.

The vertical stratification system does not dominate the system of rule. Most voluntary associations do not control major stocks of incentives of the members; thus exit is easier. Time commitments of members are low, and the authorities are less in a position to impose strong sanctions. Second, the constitution of authority in these organizations restricts the rights of the center vis-à-vis members or units. In federations, ranging from business partnerships to labor unions to Protestant churches, the units are basic elements of the organization, retain rights of operation in their own domains, and share in decisions about the direction of the total organization. These organizations possess political systems which, if not democratic, at least establish a flow of authority and legitimation from the members and units to the central organization. They establish procedures for succession to major office that involve the unit or members; they establish procedures for the members or units to affect major policies and directions for the organization. What effect do these differences in polities have on the kinds of social movements that emerge?

As noted earlier, professional associations, voluntary associations, and federated organizations have more open politics than do hierarchical organizations. At least dissent, factionalism, and conflict are less likely to be repressed. In many ways, then, one can move the whole apparatus of political sociology analysis into these polities with even less modification. Moreover, theoretical and empirical works have examined phenomena resembling social movements in these organizations before, though often under different guise. Thus the literature on factionalism and democracy in unions (see Edelstein and Warner 1975), on the conflict in the Methodist church (Wood and Zald 1966), on factionalism in political parties (Wilson 1966), and on change in professions (Ross 1975; Epstein 1970) begins to describe the phenomenon in question.

In general we expect that coups and insurgencies are less likely to occur in these organizations, and factional fights and secessions are more probable.

Federations

A federated organization is defined constitutionally by the rights reserved to the units. Both constitutionally and in practice, there is variation in the relationship of federated units to their centers and to each other (Sills 1957). For instance, a federated union may have weak and apathetic units,

so that the central organization essentially dictates policy and makes it extraordinarily difficult for any local initiative to materialize. In contrast, other units may command resources, loyalty, and a spirit of independence that make them a perennial thorn in the side of both management and central union offices (Kuhn 1962). Similarly, church organizations may range from episcopal structures in which the living constitution gives great power and command to the bishop to congregational structures in which the local congregation controls and appoints the minister, owns the church, and does not need to accept any policy directives from the center.

Social movements in federated organizations usually take the form of rebellions of single units or coalitions of units against the center. Conspiracies may occur if the dissident faction's composition or goals are abhorrent to the dominant coalition in the organization. For example, the establishment of front organizations by the Communist party is not such ancient history, nor is the conspiratorial takeover of the Social Democratic party by Lenin-led Bolsheviks.

The greater the degree to which local units control (own) the resources of the units (the physical facilities and/or the loyalties and identification of the members), the more likely it is that schisms and secession will occur. Thus, as compared with hierarchic corporate organizations, social movements in federated organizations may lead to a breakup or splintering off of various segments of the organization. The history of American Protestantism is rife with such schisms and secessions. Schisms also occur in political party systems where proportional representation rules exist. In contrast, when voting rules lead to winner take all, political parties retain their factions.

There are essentially two types of schisms. In one, whole units secede; in the other, groups of members within units find the program or policy of the organization unacceptable and secede with the intent of setting up a new organization. The former is probably more easily accomplished, since the whole unit secedes with an organization and facilities already in place.[7]

Less dramatic than schisms or secessions, members or units may lead the battle for changes in policy. For instance, the Episcopal church, which combines hierarchical and federated features, has had a long, drawn-out battle over the adoption of a new translation of its book of prayer. This movement has been led by college English professors and has brought about a reversal in church policy. By the same token, trade union locals may hold out against the settlement negotiated by the international union. This is not to suggest the center is without power in federated organizations. For instance, a recalcitrant local can be put in trusteeship and its officers,

[7] In episcopal structures the church building is often owned by the central church, even though possibly paid for by local donations. And in court battles over expulsion, the building may revert to the overall association. On the other hand, in congregational structures the local denomination may own the building.

stewards, and business agents suspended. Since putting a local into trusteeship also leads to cutting off funds and abrogating votes, it is a potent weapon for the center. In Protestant denominations, the center's power over the dissident congregation ranges from censure to lockouts and even to heresy trials.

Voluntary Associations

Social movement organizations, professional associations, social clubs, and local community-betterment groups are characterized by heavy dependence upon their membership and membership commitment for their maintenance. As noted above, because the organization is not a purveyor of major stocks of incentives, the center must curry favor; and executives are heavily dependent upon persuasion, symbolic rewards, and providing services and friendship to maintain commitment.

In some voluntary associations, open conflict and politics are muted precisely because the conflict would lower the commitment of members and lead to high rates of exit. Organizations that depend upon solidary incentives attempt to avoid conflict; they develop a politics of accommodation. If conflicts do emerge, they may lead quickly to individual or group secession. Even voluntary associations with purposive goals must avoid conflict, for the losing factions are likely to secede.

Professional segments.—One more type of social movement in organizations deserves mention: a movement in a profession that infiltrates both corporate hierarchical and professional associations. Bucher and Strauss (1961) describe the growth of segments in social movement terms. The members develop a sense of group identity and difference from the rest of the profession; they develop a program of action. In professional associations, such segments operate loosely either through caucuses or through attempts to change the policies and structure of the association. In hierarchic organizations, they can be described as bureaucratic insurgents.

CONCLUSION: THE UTILITY OF THE MODEL AND RESEARCH PROSPECTS

This analysis has suggested that the similarities of nations to organizations permit us to utilize concepts of social movements drawn from the former to examine similar processes in the latter. Much more than a metaphor is intended. Organizations and societies are similar enough so that a strong analogical model can be and has been employed. There are indeed strong similarities. Both organizations and nation-states, for example, are composed of groups located in a stratified system of statuses and with differential access to resources. They are both webbed by ideologies which often cause conflict in terms of which perspective will lead more efficiently to goal

accomplishment and whose perspectives on goals ought to be adhered to. They both have group cleavages. They both have systems of rules and of social control, and they both have group and individual resistance to their rules and control.

On the other hand, there are differences that weaken the analogy. A state has a legitimate monopoly over coercion, whereas an organization does not. Moreover, a state has a pluralistic set of goals. It is not even clear how one can conceive of the goals of a state; whereas the goals of an organization seem at least more concrete. Nation-states are relatively large in territory, whereas organizations are more concentrated geographically. Nation-states can present great obstacles to mobility (i.e., entry and exit), whereas in most organizations, except for coercive types, entry and exit are comparatively easy. Finally, the cleavages and structure of nation-states endure for generations and are transmitted through families and the class system, while the cleavages of organizations have a less enduring base.

The question then becomes, What are the consequences of these differences for the strength of the analogical model? First, the claim that the state has legal powers of coercion, whereas the organization does not, suggests that organizations will not have a full range of strategies with which to deal with conflict groups. Yet it can be argued that the organization's ability to expel, isolate, or apply sanctions to its members represents similarities to legitimate coercion and that both nation-state and organization use coercion which is circumscribed by legitimate norms.

Second, the claim that goals in a nation-state are more abstract and organizational goals more concrete implies that movements may be easier to mobilize in an organization. Yet it can be argued that in both cases there is a duality of goals. That is, there is a distinction between official and operative goals. Official goals are the entities' general purposes, as set forth in its charter, whereas operative goals are the actual goals sought which are determined by group interests (Perrow 1961). Furthermore, it can be asserted that the recent civil rights, end-the-war, women's, and environmental-protection movements have all had very precise goals.

Third, the argument that states are larger and organizations smaller implies that mobilization costs may vary as a function of size. In some sense this may be true, yet it can also be argued that the processes are the same and only the scale is different. If both entities were standardized to control for size, the results might be very similar. Fourth, the assertion that the two entities differ in mobility potential implies that the exit option may be readily available in organizations, hence leading to fewer movements, whereas exit from one's nation-state is more difficult, suggesting a greater reliance on voice (i.e., movements). Yet this reasoning ignores the facts that (1) changing one's job represents a serious disruption; that is, while job mobility may be theoretically possible, on a practical level it is not

always desirable; and (2) apathy and passivity are just as prevalent at the nation-state level as at the organization level, suggesting that the formation and execution of movements in both are equally problematic. Finally, even generational turnover in organizations is a variable: although class ideology may have to be transmitted more rapidly in organizations, people are not so transient that ideology and alternative accounts may not be transmitted within organizational generations (see McNeil and Thompson 1971).

The differences between nations and organizations do imply differences in the social movements that will be generated. In organizations they are likely to be smaller, less likely to use violence, less likely to have well-developed ideological systems, less likely to have long-range continuity. These, however, are matters of degree, not qualitative differences. These "little" social movements are similar in enough regards that parallel processes and dynamics can be seen, and sometimes they are not so little.

A more serious difficulty with the approach has to do with its utility for research. How can we gather data on these processes; how can we study these events? For example, the propositions imply a sample of organizations to evaluate structural and dimensional determinants of the occurrence and form of a movement. Often, however, published accounts of occurrence and form are sketchy or nonexistent. Further, it is very difficult to learn about movements which were aborted or suppressed. For nations, some enterprising historian will describe such movements. Who will describe them for organizations? While research in this area is difficult, it is by no means impossible. Compared with research on some aspects of social movements in nations, it is actually easier. The difficulties come largely in the quantitative study of coups and insurgency. We typically learn of them after the fact and only if they hit the "news." Yet both can be examined by ethnographic case studies. And even systematic sampling of informants in organizations can provide us with a sense of the range of insurgencies and coups in organizations. For instance, using a sample of organizations, an investigator could gather a record of all successions to top positions over a number of years. He then could use informants and organizational histories to examine the processes of succession. Or, following Wilsnack's (in press) lead, a survey could be designed tapping the range of social movement phenomena in an organization, from protest to petitions to riots, over a defined period.

The advantages of studying social movements in organizations are obvious: there are only 100 or so nations, there are thousands of organizations, and there is probably more variation in the polity of organizations than in nations. Even the study of outcomes of social movements is easier within organizations than within nations. Analyses of social movements in nations have been notorious for the looseness with which outcomes—success, failure, goal displacement, movement becalmed—are discussed. Given the range of processes in a society, attributing change to a social

movement is like looking for a needle in a haystack. (See the critique and programmatic statement on this problem by Snyder and Kelly, in press.) At least in organizations the boundaries of effects have clearer space-time limitations. Specific policies, authority relations, and changes in rights and prerogatives can be pinpointed.

This article has identified a new agenda for organizational analysis. Instead of following the traditional approach to conflict, power, and exchange, the essay conceives of many such processes as unconventional politics that give rise to movement-like phenomena. More specifically, the essay distinguishes organizational coups, bureaucratic insurgencies, and mass movements to argue that a combined approach of analogical theorizing and resource mobilization may explain the form, occurrence, and outcomes of such phenomena. After offering a number of propositions relating to a resource-mobilization perspective, we evaluated the strength of the analogy and implications for research.

The model we have presented here has, we believe, several uses. First, it brings together under one conceptual structure partial analyses of social movement phenomena employed by others. That is, we are able to link a number of analyses of insurgency, strikes, enclave formation, and other phenomena that have been conducted without reference to each other. Second, our approach suggests a number of possible research strategies to illuminate a phase of organizational life that has been hidden in darkness. Third, it suggests a new paradigm in the study of organizations: the study of mobilization processes (see Benson 1977). Finally and most provocatively, it opens up once more a host of questions that have seemed settled. Can we, for example, look at the Protestant Reformation not as an issue in theology, or as an issue in the relationship of societal constitution to collective ideation (Swanson 1967), or an issue in state formation but as an organizational rebellion? Seen from Rome, the Protestant reformers must have looked like bureaucratic insurgents, and the insurgents had to mobilize internal and external support.

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Arts and Crafts

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"Art" and "craft" are two contrasting kinds of aesthetic, work organization, and work ideology, differing in their emphases on the standards of utility, virtuoso skill, and beauty. Activities organized as craft can become art when members of established art worlds take over their media, techniques, and organizations. Conversely, through increased academicism or subordination of traditional art concerns to exigencies that arise outside an art world, activities organized as art can become craft.

People often use commonsense folk classifications to categorize their occupations, the organized forms of work they participate in (see Hughes 1971, pp. 360–63). They may speak of the medical "profession," the music "business," the "science" or "discipline" of sociology, the garment "trade," or the numbers "racket." Each term conveys in shorthand form a conception of a distinctive way of organizing work: the characteristic activities that make up the work, the typical settings in which it is done, and the cast of characters with whom one usually associates while doing it, the kinds of people who do it, their typical careers, the problems that ordinarily arise, and the moral evaluations those inside and outside the occupation make of the people and activities which compose it.

The shorthand folk term suggests that all these matters cohere in a neat, interrelated pattern. Thus, when we call some line of work a "profession," we imply that its highly trained members perform a vital social function by assisting people who do not understand what is being done for them and police themselves according to a high ethical standard, and we imply all the other things included in the conventional definition. Conversely, when we speak of a "business," we imply that it is carried on for a profit, that the applicable code of ethics involves only living up to contracts one has entered into, that the operating logic is that of efficiency, that the principal of caveat emptor applies, and so on.

Since the world is less neat than that, the elements of work organization do not fall into such tidy patterns in real life. Some features of an occupation may resemble those of a profession, while others suggest a business or a racket. The folk categories predict what elements will be found in what arrangements, but they often predict wrongly. They are not so much accu-

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rate descriptions of reality as ideological descriptions of some preferred arrangement of elements.

The members of art worlds (Becker 1974, 1975, 1976; Levine 1972; White and White 1965) usually describe the work of those who produce their characteristic products with such shorthand folk terms as "art" or "craft."[1] The person who does the work that gives the product its unique and expressive character is called an "artist" and the product itself "art." Other people whose skills contribute in a supporting way are called "craftsmen." The work they do is called a "craft." The same activity, using the same materials and skills in what appear to be similar ways, may be called by either title, as may the people who engage in it. The histories of various art forms include typical sequences in which what has been commonly understood by practitioners and public to be a craft becomes redefined as an art or, conversely, an art becomes redefined as a craft. This paper considers such shifts, with an eye to understanding how they occur and arriving at a better understanding of the meaning of both "art" and "craft" as those terms are conventionally understood and applied.

I have no preferred meaning for either term and no intention of legislating definitions for them: quite the opposite. As folk terms, "art" and "craft" refer to ambiguous conglomerations of organizational and stylistic traits and thus cannot be used as unequivocally as we would want to use them if they were scientific or critical concepts. Since I will nevertheless have occasion to speak of art and craft worlds, organizations, and styles of work, it should be understood that in doing so I am referring to one or another aspect of some folk definition. I often refer to particular organizations that come close to realizing the ideal combinations implied by the folk terms, but even these do not live up to the expectations embodied in the ideal, nor does it matter analytically that they do not.

In fact the ambiguities of the terms and the contradictions between what they predict and what the world exhibits will be most useful in the analysis, as those ambiguities and contradictions occur in particular fields of activity undergoing change. When change occurs, the people involved argue over the meaning of the activity; therefore examining cases of change from one definition to another will help us understand better the social meaning of our basic terms.

I have made indiscriminate use of materials from a variety of sources—my own experience in a number of worlds of art and craft as well as sociological and historical studies of such worlds—but I have not examined any systematic body of data in a systematic way. For my major examples I have used the worlds of the conventional handicrafts (especially ceramics), which

[1] The concept of an art world is made the basis of a system of aesthetics in Blizek (1974), Danto (1964), and Dickie (1971).

produce objects capable of visual appreciation and thus tend to be linked to such high art worlds as painting and sculpture. But the analysis is intended to be more general than that, and, though I speak largely of such crafts, I will occasionally indicate applications to other kinds of media and to performing as well as object-producing arts and crafts.

CRAFT AND ART

As a work ideology, an aesthetic, and a form of work organization, craft can and does exist independent of art worlds, their practitioners, and their definitions. In the pure folk definition, a craft consists of a body of knowledge and skill which can be used to produce useful objects: dishes one can eat from, chairs one can sit in, cloth that makes serviceable clothing, plumbing that works, electrical wiring that carries current. From a slightly different point of view, it consists of the ability to perform in a useful way: to play music that can be danced to, serve a meal to guests efficiently, arrest a criminal with a minimum of fuss, clean a house to the satisfaction of those who live in it.

To speak of usefulness implies the existence of a person whose purposes define the ends for which the objects or activities will be useful. Those purposes arise in some world of collective action in which they are characteristic, part of the definition of what kind of world it is. Serving a meal to guests efficiently might, for instance, be part of the world of commercial catering, in which the development of a stable clientele who can be fed at a profit is the end in view. Or it might be part of one's domestic world, in which case the object is to satisfy the appetites for food and graceful social intercourse of one's family, friends, and acquaintances. In both cases, utility is measured by a standard which lies outside the world that is or might have been constructed around the activity itself. For there is a world of *haute cuisine* and etiquette which treats the enjoyment of food and its service as ends in themselves, the measurement of utility referring to standards developed and accepted by knowledgeable participants in that world. (The distinction between utilities which are part of the world constructed around the activity itself and those measured by standards imported from other worlds—call them "intrinsic" and "extrinsic" or "practical" utilities—will occupy us throughout.)

Defining craft as the knowledge and skill which produce useful objects and activities implies both an aesthetic, standards on which judgments of particular items of work can be based, and an organizational form in which the evaluative standards find their origin and logical justification. The organizational form is one in which the worker does his work for someone else—usually a client, customer, or employer—who defines what is to be done and what the result should be. The employer understands that the worker

possesses special skills and knowledge but regards it as appropriate to have the final say himself as to the suitability of the result. The worker may know better ways of doing things, not known to someone outside the craft, but recognizes the employer's right to the final word. Both recognize that the object of the activity is to make something the employer can use for his purposes, whatever they may be. Although a worker sometimes makes things for his own use, that does not alter the point I want to emphasize, that the object is made to serve someone's need for a useful object.

If a person defines his work as done to meet someone else's practical needs, then function, defined externally to the intrinsic character of the work, is an important ideological and aesthetic consideration. If the piece that is made has no evident or possible practical use or if it is totally unsuited to its ostensible use, the craftsman who made it (a craftsman being someone who accepts the craft ideology I am describing) will probably receive and feel vulnerable to severe criticism from his colleagues. I will give some examples later.

In addition to function, craftsmen accept a second aesthetic standard: virtuoso skill. Most crafts are quite difficult, with many years required to master the physical skills and mental disciplines of a first-class practitioner. One who has mastered the skills—an expert—has great control over the craft's materials, can do anything with them, can work with speed and agility, can do with ease things that ordinary, less expert craftsmen find difficult or impossible. A potter, for instance, may be able to throw pots with walls so thin that other potters would be unable to prevent them from collapsing. Conversely, he may be able to throw great masses of clay other potters would find impossible to control. The specific object of virtuosity varies from field to field, but it always involves an extraordinary control of materials and techniques. Sometimes virtuosity also includes mastering a wide variety of techniques, being able not only to do things better than most others but also to do more things. Virtuoso craftsmen take pride in their skill and are honored for it in the craft and sometimes by outsiders.

That an object is useful, that it required virtuoso skill to make—neither of these precludes it from also being thought beautiful. Some crafts in fact generate from within their own tradition a feeling for beauty and with it appropriate aesthetic standards and canons of taste. Both makers and users think that some furniture is beautiful in addition to being useful and that they can tell the difference. Not many people care to make these fine distinctions with respect to household craft objects, but those who do add beauty to utility and virtuosity as a third criterion of judgment which they embody and maintain in their daily activities. Beauty becomes an additional criterion which connoisseurs use in forming judgments and workers try to satisfy.

By accepting beauty as a criterion, participants in craft activities take on

a concern characteristic of the folk definition of art. I will not try to enumerate all the elements of that folk definition here but will simply indicate that it includes an emphasis on beauty as typified in the tradition of some particular art, on the traditions and concerns of the art world itself as the source of value, on expression of someone's thoughts and feelings, and on the relative freedom of the artist from outside interference with his work. Concerning the last element, the folk definition acknowledges, implicitly though usually not explicitly, that other participants in the art world—patrons, dealers, curators, and critics, for instance—will in fact if not in theory constrain the artist's expressive freedom substantially.

Because some craftsmen accept beauty as a criterion, the organizational form of craft worlds becomes more complicated and differentiated than it might otherwise be. Crafts ordinarily divide along the line between the ordinary craftsman trying to do decent work and make a living and the artist-craftsman with more ambitious goals and ideologies. Ordinary craftsmen usually respect artist-craftsmen and see them as the source of innovation and original ideas. The two types not only have distinct ways of carrying on the craft but also constitute distinct groups of people, since workers tend to identify themselves as one or the other and to adopt fairly exclusively one or the other mode of activity.

The ordinary craftsman, I think, does not take the criterion of beauty very seriously. Busy satisfying the demands of a variety of jobs and customers, he contents himself that the pipes he installs carry water, that the bookcase he builds is sturdy and fits in the space he measured for it, that the meal is served expeditiously. I have deliberately, of course, chosen examples from crafts in which the idea of beauty seldom enters anyone's calculations, at least in the conventional sense connected with such high arts as painting and sculpture.

Some craftsmen (a current list would include potters, weavers, glassblowers, and furniture makers, to cite the most obvious cases) speak of themselves not just as craftsmen but as artist-craftsmen. The distinction means something in these craft worlds. The American Crafts Council identifies itself as the organized voice of the artist-craftsman. Its influential magazine, *Craft Horizons*, emphasizes questions of beauty and artistic merit in contrast with a more purely craft-oriented magazine like *Ceramics Monthly.* Similar purely craft-oriented magazines may be found for most crafts.

Work by artist-craftsmen, with some claim to be considered "art" by the custodians of conventional art—collectors, curators, and gallery owners—opens up new organizational settings in which to work and gain support for one's work. This frees the artist-craftsman in some measure from the constraints embodied in the employer-employee relationship characteristic of the pure craftsman's position. Under the heading of "minor arts," beautiful craft objects are displayed in shows and museums, win prizes for their beauty,

contribute to the reputations of the craftsmen who make them, become the subject of books and the occasion for demonstrations of "how to do it," and even furnish the basis on which teaching jobs are given and held. In short, not only do some people care to make the distinction between beautiful and ordinary craft objects, but there are substantial rewards for making more beautiful objects while adhering to craft standards.

Artist-craftsmen have higher ambitions than ordinary craftsmen. While they may share the same audiences, institutions, and rewards, they also feel some kinship with fine art institutions. They see a continuity between what they do and what fine artists do, even though they recognize that they have chosen to pursue the ideal of beauty they share with fine artists in a more limited arena. What constitutes beauty can of course be the subject of considerable controversy, but it is the third major criterion according to which people judge work and to which they orient their own activity.

We might imagine the differentiation of craftsmen and artist-craftsmen as a typical historical sequence. A craft world, whose aesthetic emphasizes utility and virtuoso skill and whose members produce works according to the dictates of clients or employers operating in some extracraft world, develops a new segment.[2] The new segment's members add to the basic aesthetic an emphasis on beauty and develop some additional organizational elements which in part free them from the need to satisfy employers so completely. These artist-craftsmen develop a kind of art world around their activities; we might reasonably call it a "minor art" world. The world contains much of the apparatus of such full-fledged "major arts" as painting or sculpture: shows, prizes, sales to collectors, teaching positions, and the rest. Not all craft worlds develop such an artistic beauty-oriented segment (plumbing, e.g., has not). But where an art segment develops, it usually coexists peacefully with the more purely utilitarian craft segment.

ART INVADES CRAFT

Another typical sequence of change occurs when members of an established world already generally defined as "art," people involved in the typical activities and ideologies of a contemporary art world, invade (and the military metaphor is appropriate) an established craft world and especially its art segment. The sequence begins when some fine artists look for new media in which to explore a current expressive problem. These artists happen on one of the crafts and see in its materials and techniques a potential for artistic exploitation. They see a way to do something that will interest the art world to which they are oriented and to which they respond. They have no interest in the conventional standard of practical utility; their notion of

[2] The notion of an occupational segment is analyzed and illustrated in Bucher (1962) and Bucher and Strauss (1961).

beauty is likely to be very different from and "more advanced" than that of the craft they are invading and the kind of skill and control they are interested in quite different from that prized by the more traditional practitioner.

The new breed of artists in this craft produce altogether new standards, standards that are aggressively nonutilitarian. That is, they are interested only in the utilities defined by the art world in which they participate. Art utilities typically include usefulness as objects of aesthetic contemplation, of collection and ostentatious display, and as items of investment and pecuniary gain. They do not include practical utilities defined by the purposes and organization of other worlds. Artists invading a craft want to make sure that the works they produce cannot be used as people have been accustomed to using them. Robert Arneson, for example, one of the leading spirits in the movement which claimed pottery as a fine art field (Zack 1970), made a series of large plates, technically quite competent, whose utility was destroyed by the large brick which sat in the middle of each one, slowly sinking into the surface as the series progressed (fig. 1). In another instance, a group of artists gained control of a ceramic department in an art school. The new chairman announced decisively that from then on they would make no high-fire pottery in the department. His point was that they would no longer make clay objects that had any utility, because only high-fire pottery will hold water and thus be useful for domestic purposes: cups, glasses, dishes, vases, and so on. By insisting that only low-fire pottery be made, he in effect announced that what they would do from then on was some version of contemporary sculpture. Lest anyone miss the point, he elaborated by saying, "We are not going to make any vessels."

Just as the standard of utility is devalued, so too are old craft standards of skill. What the older artist-craftsman has spent a lifetime learning to do just so is suddenly hardly worth doing. People are doing his work in the sloppiest possible way and being thought superior to him just because of it.

Instead of adhering to the conventional craft criteria, which of course turn up in somewhat different form, the artists who enter a craft field propose, rely on, and organize their own work according to criteria characteristic of worlds conventionally defined as high art. For instance, in the art versions of any of these media, uniqueness of the object is prized. Artists and their publics think that no two objects produced by an artist should be alike. But for good craftsmen that is not a consideration; indeed it is thought a mark of the artist-craftsman's control that he can make things as much alike as he does. People who pay $200 for a small, beautifully turned bowl will not feel cheated if they find there is another more or less like it. What they bought exhibits the virtuoso craftsmanship they paid for. But if they had bought the same bowl on the assumption that it was a unique work of art, they would feel enormously cheated to find that there were two. So artists who work in these media sell their conception and its execution in that

medium and take care to be obvious about how each of their pieces differs from all the others. No one wants to buy a copy from an artist, only from a craftsman.

The new standards artists create insure that a work's only utility will be as art: to be admired, appreciated, and experienced. The artists denounce the "mere virtuosity" of the old school of craftsmen. They discover and

Fig. 1.—Robert Arneson, *Sinking Brick Plates*, 1969; courtesy of Hansen Fuller Gallery, San Francisco.

create a conscious continuity with work in other areas of art, especially in the traditional areas of painting and sculpture. They announce their independence of others' ideas of what their work should consists of and denounce any attempt to fasten on them the requirements of utility. What they do usually requires a great deal of skill and control, but the skills needed are usually of a deliberately different kind from those prized by either ordinary craftsmen or artist-craftsmen and often are hidden as well. Marilyn Levine, for instance, has achieved a considerable reputation for ceramic sculptures of shoes, boots, and other leather objects which look so much like real leather that you have to tap them and hear the ring to be convinced that they are clay; they work in part because of the contradiction between what they look like and what they are made of (fig. 2). Indeed it becomes a virtue not to display conventional craft virtuosity, and the artist may deliberately create crudities (the making of the crudities may itself involve considerable virtuosity, though not the same kind as that of the craftsman), either for their shock value or to show that he is free of that particular set of conventional constraints.

Defining their work as art, the artists who adopt craft materials and techniques create and accommodate themselves to a different social organization from that which grows up around a craft. Craft organization subordinates the craftsman to an employer, at whose insistence and for whose purposes the work is done. But the contemporary folk definition of art presumes that the artist works for no one, that the work is produced in response to prob-
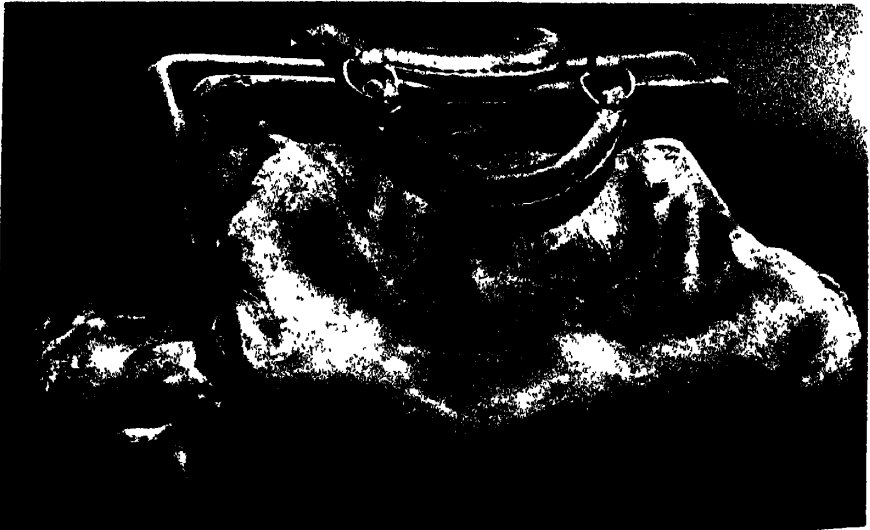
FIG. 2.—Marilyn Levine, *Brown Satchel*, 1976, ceramic, 6 × 9 × 13½ inches; courtesy of the Hansen Fuller Gallery, San Francisco.

lems intrinsic in the development of the art and freely chosen by the artist. Organizationally, of course, the artist is no such heroic individualist: he operates in a setting of institutional constraints which vary from time to time and place to place. Some art worlds operated through a system of church and royal patronage in which the artist found it expedient to take account of the tastes and desires of noted patrons. Contemporary artists, enmeshed in a world of collectors, galleries, and museums, typically produce with no particular purchaser in mind and expect their work to be marketed through the conventional apparatus of dealers and museums, the purchaser exercising control by buying or refusing to buy. Whatever the organizational form, the folk definition further presumes that these purchasers and intermediaries are as concerned as the artist with the utilities defined by the art world and therefore with problems and topics defined within rather than outside the current art world. These presumptions are often violated, but they are the model to which artists orient themselves.

Fine art photographers, for example, do a greater variety of work, less constrained by the requirements of organizations in which they work, than do those who work in such craft-oriented areas as advertising and fashion photography or photojournalism (Rosenblum 1973). Artists working in conventional craft media are similarly relatively freer than artist-craftsmen who work in the same media, both in the diversity of objects they make and in the variety and whimsicality of the ideological explanations they offer for their work. The objects typically display great continuity with current trends in such contemporary high art worlds as painting and sculpture, and the talk both calls attention to that continuity and displays at least superficial indifference to being intelligible or rational. I take this latter characteristic to express a posture of indifference to public acceptance characteristic of many contemporary artists.

Here are some examples. Arneson has made many pieces which are in fact sculpture: a typewriter, somewhat sagged out of shape and rough around the edges, whose keys represent red painted fingernails (fig. 3; see Zack [1970] for other examples); a series of self-portraits, smoking a cigar or with the skull opened to reveal various contents; an enormous table covered with dishes of food, standing in front of a life-sized portrait of the artist in a chef's hat, all glazed a pure unrelieved white. To an observer familiar with the conventions of contemporary sculpture and ceramics, these pieces look not quite like sculpture but more like ceramics. Aggressively not utilitarian pottery, they nevertheless call attention to themselves as pottery through the rough modeling of the clay and the gaudy glazes. Some of their effect lies in the ambiguity so created. Other pieces are utilitarian in principle but not quite in fact. An example is Arneson's teapot (fig. 4) whose spout is a realistically modeled penis; you can pour tea from it, but not for everyone.
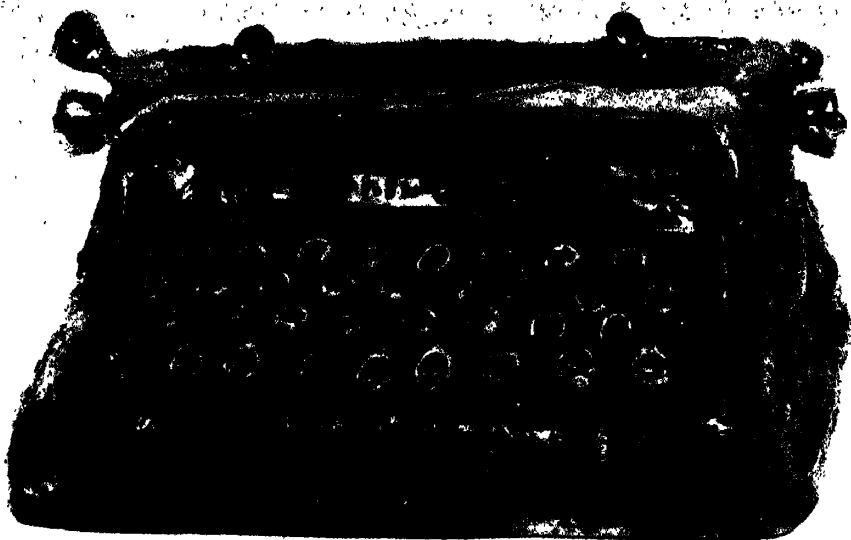
FIG. 3.—Robert Arneson, *Typewriter*, 1965, ceramic, $6\frac{1}{8} \times 11\frac{3}{8} \times 12\frac{1}{2}$ inches; Collection University Art Museum, University of California, Berkeley; gift of the artist.
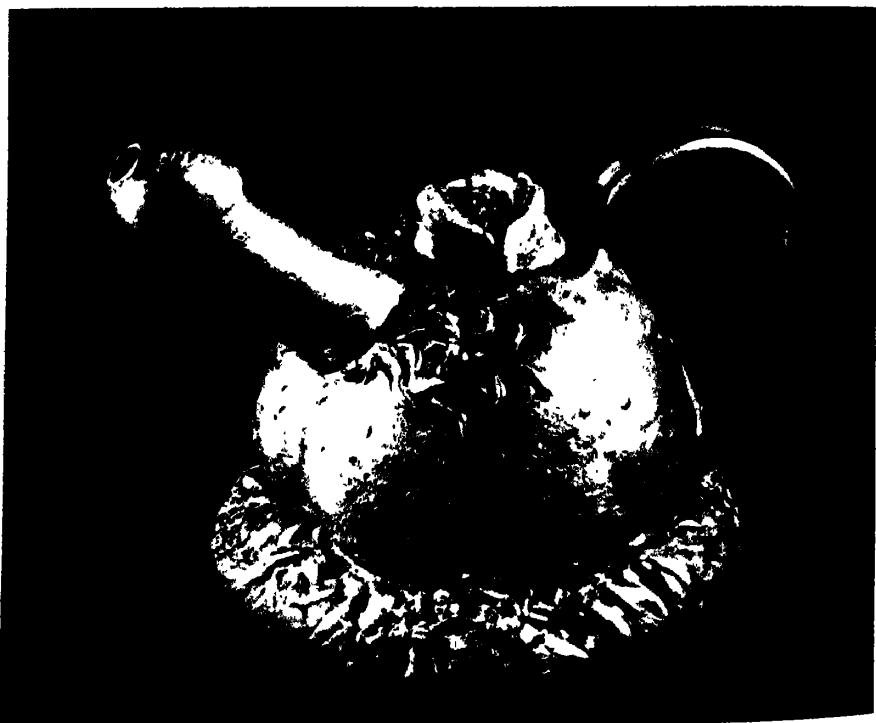
FIG. 4.—Robert Arneson, *A Tremendous Teapot*, 1969, lustered earthenware, 8 inches tall with base; courtesy of the Hansen Fuller Gallery, San Francisco.

To turn to the talk that accompanies such work, here is Arneson explaining himself:

> My recent ART WORKS IN PORCELAIN, GLAZED IN ALL THE COOLNESS OF CELADON, are like a 9th Century southern Chinese Sung Dynasty potter explaining the TRUTH of art, non-art, not-art and their significance in a precious and free sense way with added footnotes and trivia culled from folks like Steve Kaltenfront, The Duncan Mold Company, and my own astrological signs for a Virgo artist who thought his Scorpio was rising until his astro flashed the truth from the depths of the station. But that was long after the fires went out and the kilns had cooled, so I figured in light of the evidence presented, and depending on where you stood, that it all didn't seem to make too much difference anyway—just a lot of work in white mud calling itself art. [Quoted in Slivka 1971, p. 42]

Arneson ridicules the idea of art and conventional artists' explanations of their work in a way that is now common among "advanced" artists. He is not an isolated case. Roger Lang explained his sculpture of a piece of pie on a plate similarly:

> Pie was interesting to me first of all as food—then I found some triangular associations, geodesic, mathematical, sexual, using a pie wedge as a basis for plate decorations. Later, high in the sky, chicken pot pie, apple pie, cherry pie, and pie-eye thoughts pushed me into 3-dimensional usages. Fruit Pie is, after all, a very American food. Gradually, things accumulated and I came to think of pie as a vehicle for associations, things that come along for nothing, free. In addition, there are the visual changes which I impose, and I haven't even begun explorations of one-crust pies yet. Taking everything into account, pie is very rich. [Quoted in Slivka 1971, p. 43]

Similar examples, visual and verbal, could be collected for weaving, glassblowing, furniture making and clothing design (figs. 5 and 6 present visual examples for weaving and glassblowing, respectively).

I have spoken of artists "invading" and taking over a craft world, bringing new standards, criteria, and styles to an activity previously dominated by craftsmen and artist-craftsmen. All the conventional crafts have an apparatus of exhibitions and shows, usually held in specialized craft galleries and smaller museums (e.g., the biggest annual ceramics show is held in the Everson Museum in Syracuse, N.Y.) or in out-of-the-way corners of more important museums. In fact artist-craftsmen usually busy themselves fighting for more exhibition space in better museums for their craft and rejoice when "one of ours" makes it into a more prestigious space (see Christopherson 1974). Museums especially tend to be sensitive to ideological and aesthetic fashions. This sometimes results in the "invaders" being invited to serve on the juries of craft shows. They pick work that ranks high by their standards and ignore work done by the old standards. Soon the new artistically oriented works are being shown, winning prizes, being written up in the magazines, and perhaps even being sold. (Sales, of course, depend on
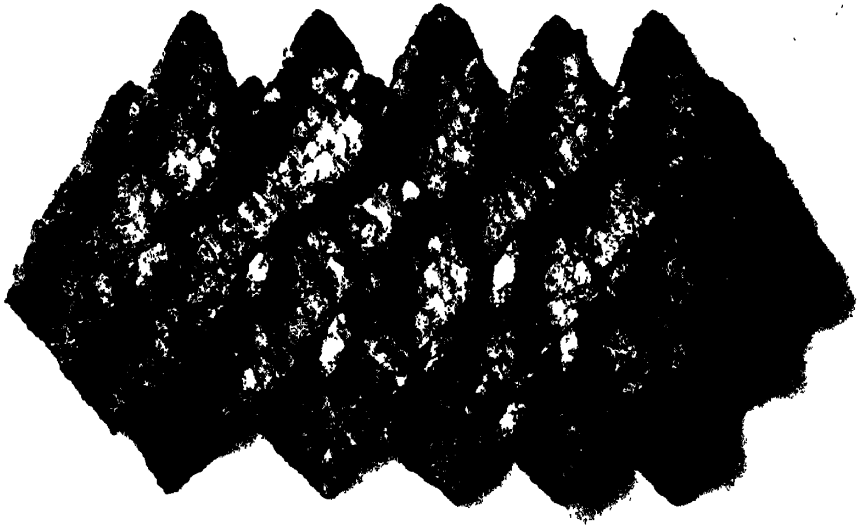
FIG. 5.—Neda Al-Hilali, untitled paper weaving, 8 × 12½ feet; courtesy of the Allrich Gallery, San Francisco.

the degree to which the new style of work muscles in on the already established craft market, as opposed to simply creating a new market or siphoning off some of the money that might have gone to the other forms of advanced art.) The older craftsmen become enraged and often are hurt economically. They lose teaching positions to the newcomers, so that a whole new generation of students and potential consumers are turned on to the movement as both producers and purchasers of work.

Of course the old group will probably retain most of their old clientele, so it is not quite an economic disaster. On the other hand, they lose some customers and patronage and lose decisively in the area of recognition.

In part, then, one group—the new artists—simply replace the older group of artist-craftsmen. As that happens, a great deal of conflict occurs. The craftsmen feel that a bunch of incompetent savages are taking over what they have no right to. The artists feel that they are getting rid of some old fuddy-duddies who stand in the way of artistic progress. Here is a pair of quotations from *Craft Horizons* that illustrates the emotional tone and ideological content of the polemic. In 1960 Mary Buskirk, a well-known traditional weaver, said,

> Too many times an object overshadows the purpose for which it is designed. Draperies, for instance, which are to serve as a background, should be just that—a backdrop to pull together all the objects in the room. As for rugs, they should invite you to step on them. Too often you find people walking around a rug and, when this happens, it is safe to say that design and func-

FIG. 6.—Robert Fritz, *Ménage à trois*, 1977, glass; courtesy of the artist

> tion have gone out of focus. This doesn't mean you can't use color and pattern, but it does mean you should use those elements to create something which is inviting to step on. [Quoted in Halverstadt 1960, p. 10]

Ten years later Virginia Hoffman, representing the artist group, spoke to the question, "When Will Weaving Be an Art Form?" and made clear the kind of conceptual transformation which occurs as the new group takes hold as well as the degree to which one such movement may serve as the model for others:

> The controversy regarding the status of weaving as an art form, efforts to justify its placement among the hallowed media, labels such as stitchery, applique, macrame, woven, nonwoven, unwoven, loom-free, built-in loom, ad infinitum, may indicate that a transition in thinking has not occurred . . . one hears echoes of the past, as when the ceramists were searching for a raison d'être for nonfunctional pots. . . .
>
> If one accepts the lack of need for minute classification of media and processes, how may one refer to different ways of causing concepts to materialize? Ron Goodman's "Genesis" in "Young Americans 69" pointed to the obvious nomenclature: soft sculpture. Such a large category could logically include any three-dimensional form made by flexible joinings, fibrous materials, modules with no fixed beginning or end, soft materials made hard and vice versa, strength through tension, counterbalancing, spacing, and forms created by use of invisible forces such as weightlessness and gravity. One thinks of the metal tubing and cable structures of Kenneth Snelson, some works by Gabo, architectural structures such as the Sports Palace in Mexico City and Fuller's Geodesic Domes, works by Eva Hesse, Alan Saret, Robert Morris, Alice Adams, and a growing group. [Hoffman 1970, p. 19]

This statement also illustrates the striving to change the standards of the craft, even to change the name, and to establish a continuity with work by serious artists in other fields.

But the change is not simply a matter of one group replacing another. They do not replace them completely, for the craftsmen continue to exist, produce, sell, make reputations, have careers, and construct and maintain a craft-oriented world. Instead, a new and more complicated world comes into being in which craft segments, artist-craftsmen segments, and art segments coexist. One can work solely in the confines of one such segment or orient oneself to some combination of them; most of the possibilities for orientations, modes of action, and careers that existed in the craft world still exist, together with a variety of new combinations. As the years pass, these worlds settle down and begin to experience their own segmentations, differentiations, and splits. Detailed research into the history of various crafts will help us learn the specific steps and characteristic sequences involved in these changes. Investigation of contemporary art and craft worlds will show us the patterns of coexistence of the various segments.

ART TURNS INTO CRAFT

An already developed world commonly defined by insiders and outsiders alike as an "art" world, complete with appropriate ideologies, aesthetics, and forms of social organization, often becomes the locus of another characteristic sequence which moves in the opposite direction. The originally expressive art works and styles become increasingly more organized, constrained, and ritualized; organizational forms subordinate the artist increasingly to partially or entirely extraneous sources of control; and the world and its activities begin to resemble conventional craft worlds. In this sense we can speak of an art turning into a craft. The process takes two forms. One leads to what is usually called "academic" art, the other to what is usually called "commercial" art.

Academic art.—Academicism is a tendency that emerges full-blown in commercial art. It consists of an increasing concern with how things are done, with the skill the artist or performer exhibits, as opposed to what is done, the ideas and emotions the work embodies and expresses. Since all arts require some substantial measure of skill, academic art is clearly an intermediate and ambiguous case. Most participants in any art world are not concerned with being enormously expressive or creative; they are content to work within quite conventionalized formats. But they and those who support their art world as patrons or customers generally orient themselves to ideas of expressiveness and creativity, recognizing these as the valuable components of art works.

We can speak of academic art as art produced in a world in which artists and others change their priorities so that their major concern shifts from these qualities to virtuosity. That concern, paralleling the craft concern with skill, is a step away from the standards conventionally accepted as developing out of the history of an art and toward the standards characteristic of crafts, but only a step, not the full trip, for the utilities toward which the work is pointed are still those of the art world: appreciation, collection, display.

Ivins has described the development of academicism in the field of engraving in the 16th century:

> Thus, in engraving there were performers who made great specialties of the rendering of glass and shiny metal, of silks and furs, and of foliage and whiskers. It is impossible to think that even so great an artist as Dürer was not tainted by this sort of virtuosity. The virtuoso engravers chose the pictures they were to make or reproduce not for their merit but as vehicles for the exhibition of their particular skills. The laying of lines, swelling and diminishing, the creation of webs of crossed lines, of lozenges with little flicks and dots in their middles, the making of prints in lines that all ran parallel or around and around—one engraver made a great reputation by the way he rendered the fur of a pussy cat, and another made a famous head of Christ that contained but one line, which beginning at the point of the

> nose, ran around and around itself until it finally got lost in the outer margin—stunts such as these became for these exhibitionists not a way of saying something of interest or importance but a method of posturing in public. Naturally the great show men became the models of the less gifted but equally stupid routine performers, for all these trick performances contained far more of laborious method than of eyesight or of draughtsmanship. [1953, pp. 69–70]

Classical ballet and the virtuoso playing of concert instrumentalists also furnish examples: in both, there have been long periods in which criticism dealt in some large part with whether any mistakes had been made, whether the performer had been faster or surer than others, and other craft concerns.

The conventional style which marks the turning of art into craft is what is meant precisely by "academic art" (Pevsner 1940). That it is academic does not mean that it cannot be beautiful or effective, but effectiveness and beauty become harder to achieve because there are so many rules of good procedure and form to follow. Every high art shows examples of this tendency. In certain periods poets must know a great variety of forms, many of them as demanding or constraining as the sonnet, just as composers have sometimes had to know and use such constraining forms as the fugue and canon. At the extreme, there is a right way to do everything: to draw a tree, to harmonize a theme, to portray Lear. The subject becomes more and more the skill and craft of the artist, whatever the work is ostensibly about. That limits its appeal to those who understand the conventions, rules, and skills almost as well as the artist does.

Innovations themselves are quickly assimilated into the conventional vocabulary and become the basis for lay criticism and complaint, even when directed against those who pioneered the innovations. Stravinsky suffered from just this when he premiered his comic opera *Mavra*. It used much simpler musical language than the earlier ballets—*Petrushka* and *Rite of Spring*—which had made him famous. According to his son, who witnessed the premiere of *Mavra*,

> The modest and intimate character of *Mavra*, together with its melodic idiom, which is related both to gypsy songs and to Italian *bel canto*, was bound to upset a public which over a period of years had become accustomed to look on Stravinsky as a rebel; it could not, and would not, expect each work to be other than "sensational." Such a public was bound to feel frustrated, and to look on Stravinsky as having failed his duty. The disappointment was great; and the most annoying part of it was that *Mavra* contained absolutely nothing to justify such an uproar. [Quoted in White 1966, pp. 59–60]

Similarly Edward Weston, the American photographer, suffered from complaints that he had failed to meet his own compositional and technical standards, as embodied in his earlier photographs. Those who admired his still lifes (e.g., fig. 7) and landscapes were scandalized when he produced a series

of bitter and unusual pictures during World War II, typified by a nude woman lying on a sofa wearing a gas mask (fig. 8). He wrote to a friend who had complained about these pictures,

> Your reaction follows a pattern which I should be used to by now. Everytime that I change subject matter, or viewpoint, a howl goes up from some Weston fans. An example: in the E. W. book [Weyhe] is a reproduction of "Shell and Rock—Arrangement"; my *closest* friend, Ramiel, never forgave me for putting it in the book because it was "not a Weston." Another example: when I sent some of my then new shell and vegetable photographs to Mexico, Diego Rivera asked if "Edward was sick." And finally (I could go

Fig. 7.—Edward Weston, *Shell*, 1927, black and white photograph; courtesy of the Art Institute of Chicago.

> on for pages) when I turned from shells, peppers, rocks—so called abstract forms—Merle Armitage called my new direction the "hearts & flowers" series.
>
> So I am not exactly surprised to have you condemn . . . work which will go down in history. [Quoted in Maddow 1973, p. 269]

Commercial art.—Subordination to the requirements of audiences and employers occurs in a more coercive and complete way in commercialized arts (see Becker 1963, pp. 79–119; Griff 1960; Lyon 1975). The employer chooses a use, just as in craft worlds, and the artist uses his virtuoso skill to meet the employer's requirements. An artist who has more interest in the display of virtuosity than the expression of personal ideas or emotions is more open to suggestion, influence, or coercion and more prepared to take on any of a variety of assignments proposed by others. The craft interest in utility appears in a somewhat different form, as the artist begins to pride himself on being able to do whatever he might be asked to do. Thus a commercial actor might be proud of his ability to play a variety of roles: people of different ages, classes, nationalities, and character types. A musician might be proud of his ability to play a great variety of kinds of music, from ethnic specialties like the polka to jazz, symphonic, and avant-garde music and perhaps even

Fig. 8.—Edward Weston, *Civilian Defense, 1942*, 1942, black and white photograph; courtesy of the Art Institute of Chicago.

music from cultures using unfamiliar instruments. These highly developed skills make them attractive to a variety of employers who can find use for those varied abilities.

Artists who master such technical skills usually begin to think, talk, and act like craftsmen. Proud of their virtuosity and control, much more than of the content of the art they happen to be producing, they boast of their ability to handle "whatever comes up." One can imagine that a poet laureate might be proud of his ability to compose a proper sort of poem for whatever occasion came up. Or that a court painter might have been similarly proud of his ability to produce a picture appropriate to whatever occasion it was needed for. An extended study of performing musicians who epitomize this attitude toward artistic work is found in Faulkner's (1971) book on the musicians who record the sound tracks for films and television programs. The work is well paid and requires a high degree of skill. Most of what these musicians play is very simple. But they must be prepared to play at a moment's notice material of tremendous difficulty. Faulkner quotes a cellist:

> Ninety-eight per cent of the time is just simple and dull. But one or two per cent . . . it's demanding and you have to do it. . . . Now tomorrow at 9:00 I have a call I don't have the vaguest idea who I'm working for, or what it is, how big the orchestra is, or who else is with me [in the section]. It may be X, Y or Z studio . . . 9:00. Now a cue might come up from a cello concerto, which if Leonard Rose or Pablo Casals had for a goddam concert, they'd have to study it for two months. And we have to knock it off . . . just like that on the spot--two runs and then a take. . . . That's why they're paying me more, and that's why you are known as a soloist in the business; that's why you're in demand. And we better do it. So those kinds, like I say, two per cent of the time . . . you get 'em. . . . Those moments are rare, just here and there . . . a couple of weeks that you have to use every bit of talent and tricks and whatever learning from past experience has taught you. [P. 120]

Not only is the music these men have to play difficult; they have to play it under the most trying conditions. They have to play it without benefit of prior study, according to a rigid rhythm already recorded on a previous sound track, and have no more than two run-throughs before they make the final recording, all because of economic pressures. The musicians who play and record these scores are among the most technically competent there are. They know it and feel a great pride. Faulkner quotes one "elder statesman": "I believe it is necessary to play commercial music, no matter how poor, no matter how poorly written, or how poorly starved a film score is for good ideas, I feel that for my own private pride of performance, it deserves the best I can give it. I never compromise on that" (p. 129). A few musicians bragged about their versatility:

> Many reed players don't bend, they're not flexible. Some don't even get the right sound in the studio or they refuse to play different. So [a composer] wants a light, French sound on the oboe for example, and [another composer]

> prefers a dark, flat German sound and you have to bend, to be able to play them all. . . .
>
> I doubt whether symphony players or other guys really have all the things down, all the experience that comes with knowing every style, of having gone through the mill. . . . I have to be funny, be a clown, be serious, play jazz, there's all types of music and all types of challenges. You have to improvise, the composer will even tell you to do anything you want. . . .
>
> You're pounding on high notes like we were last week for a couple of hours and all your blood has gone out of your lips and then they have you turn right around and play something soft and delicate, in the upper register, or play a little jazz, or a bugle call with finesse. Not many guys can make it come on. . . . [P. 140]

People usually refer to this mode of working in the visual arts, music, or theater as "commercial." Commercial arts use more or less the same skills and materials as fine arts but deliberately put them to uses no one regards as artistic, because those uses find their meaning and justification in a world organized around some activity other than art. When visual artists make drawings for an advertisement or an instruction manual, they serve ends defined by business or industry, as do musicians when they record the background for a television jingle. Musicians work to serve ends defined by ethnic cultures and family display when they perform at wedding ceremonies. Workers and consumers judge the product by its utility as that is defined by some world other than the art world, in relation to some other form of collective action than that defined by the art world. Art academies (see Pevsner 1940) teach the range of techniques necessary to be able to do that well.

Of course whole art worlds do not turn in this way to a craft orientation. Instead, they gradually spin off a segment composed of people who devote themselves primarily to craft-commercial activities. Because these people are skilled craftsmen, they can (and often do) also devote themselves to creating fine art on occasion. The players Faulkner interviewed furnish personnel for avant-garde concerts and jazz groups in Los Angeles when they are not recording.

CRAFT TO ART AGAIN

As an art becomes more conventionalized, standards become more and more rigorous. Most artists accept that rigor, satisfied with the expressive possibilities of conventionally acceptable forms. They are well-integrated professionals (Becker 1976), analogous to the scientists who produce "normal science" in nonrevolutionary periods (Kuhn 1962). But others find the rigor constraining and oppressive. They feel that, in order to demonstrate their competence, they have to spend so much time acquiring the conventional

wisdom and skills that they can never get to the production of the art which most interests them. They sometimes also feel that they will never be able to outdistance the traditional knowledge and technique of those who are now acclaimed.

In addition, they find that every innovation they make provokes the criticism that it fails to meet current standards of competence. Artists who give no evidence of knowing any of the "right" ways of doing things are thought by critics, audiences, and other artists to be bunglers and incompetents, even though their deviations from standard form are deliberate choices. The tyranny of such "proper" models of artistic work can be found in every field. A photographer who experiments with such an outmoded device as a soft-focus lens may be berated for his apparent inability to get a picture in focus. Many dance and jazz musicians found the Beatles incompetent because of their apparent inability to compose and play songs in the strict forms of contemporary popular song, the eight-bar sections to which such musicians are accustomed; it did not occur to them that composers like Lennon and McCartney were creating nine-bar phrases deliberately. (This is a particularly painful example for me, since I was one of the reactionaries who objected to the Beatles' inability to count to eight.)

Critics, patrons, and artists who find the constraints of convention intolerable fight back, using a variety of invidious descriptive phrases. They call work which accepts the constraints academic or speak of "mere" technical virtuosity or "mere" craft.

So the end point of the sequence in which an art turns into a craft consists of younger, newer, rebellious artists refusing to play the old game and breaking out of its confines. They propose a new game, with different goals, played by different rules, in which the old knowledge and techniques are irrelevant and superfluous, no help at all in doing what is to be done in the new enterprise. That is, they produce or discover somewhere new paradigms, new great works that are to furnish from now on the standard of beauty and excellence, works which require a different set of skills and a different kind of vision. Photography has gone through many such revolutions in its short history (see Becker 1975; Corn 1972; Newhall 1964; Taft 1938). At one time the object seemed to be to make sharp, clear renditions of whatever was there to be photographed and documented; perhaps at that point photography was a craft whose end was to serve the purposes of those who needed to have information reproduced in pictorial form. Later on, more "artistically" oriented photographers attempted to make romantic, painterly photographs (fig. 9); Edward Steichen, in the early part of his career, Alvin Coburn, and Clarence White took this tack. They were replaced by the f64 group, who insisted on a standard of sharp focus and clear light (fig. 10) and who were replaced in turn by people concerned with cap-

turing moments of real life (fig. 11), a variety of photographers including Cartier-Bresson and Robert Frank. But Frank's notion of real life was much more symbolic than most and in some ways might even be seen as a return to the painterly concerns of the early Tonalists. In short, the game of solidifying technical standards and rebelling against them goes on endlessly.

FIG. 9.—Painterly photograph, Gertrude Käsebier (seated woman with hat), undated, gum print; courtesy of the Art Institute of Chicago.

In discussing this oscillation between the solidification of standards and the development of new approaches, I do not intend to present a picture of lonely, inventive geniuses fighting against smug artistic establishments (although that happens, too). The shift from art to craft and back is not carried out by individuals acting independently; such shifts are successful only insofar as they involve enough people to take over an established art world or create a new one. For most of the people involved in such transformations, the experience is much more one of choice among alternative institutional arrangements and working companions than of inventive and creative expressive leaps.
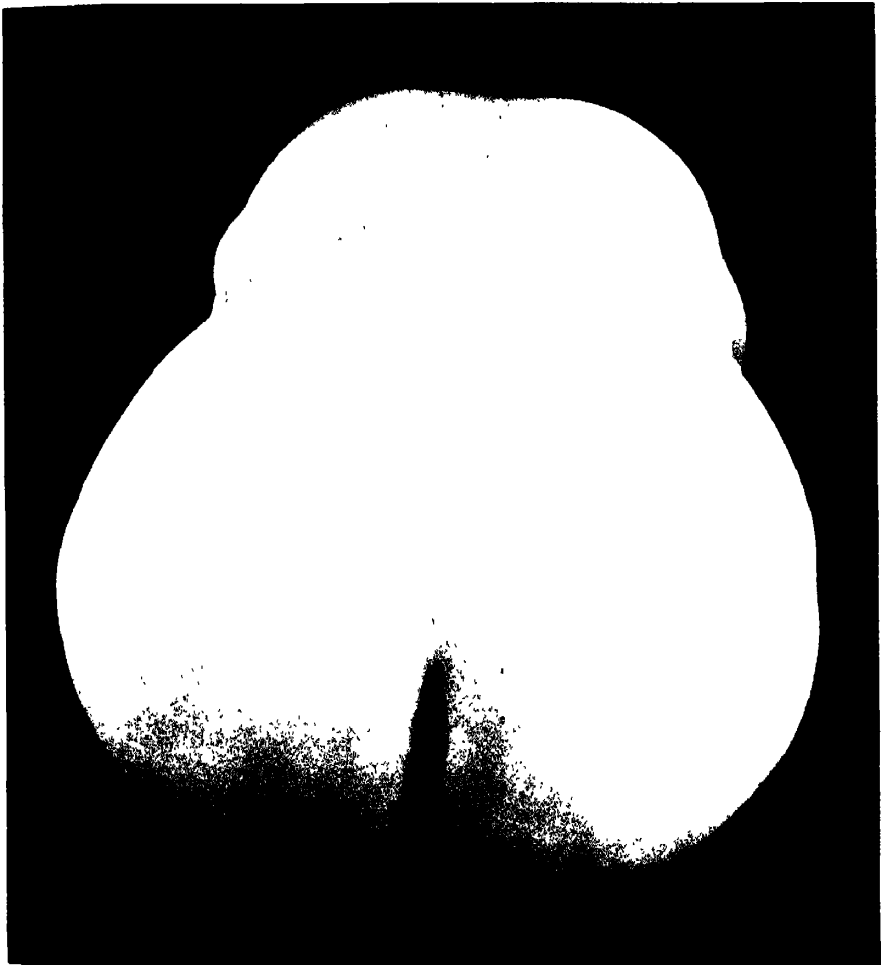
Fig. 10.—The f64 group: Edward Weston, *Nude, 1926*, 1926, black and white photograph; courtesy of the Art Institute of Chicago.

FIG. 11.—Robert Frank, *Covered Car, Long Beach, California*, from *The Americans* (1959; reprint ed., New York: Aperture, 1969), undated, black and white photograph; courtesy of the Art Institute of Chicago.

ART AND CRAFT

I have described sequences in which, in one or another commonsense meaning of the terms, craft generates art, is invaded by it, or is something that art becomes. Two points merit further discussion. It is very likely that most contemporary high art started out as some kind of craft (see, e.g., Baxandall 1972; Harris 1966; Martindale 1972). The composition and performance of European art music undoubtedly started as an activity subservient either to the requirements of the church (as in the composition and performance of the Mass and plainsong) or to the desires of a ruler and his court for entertainment and music for dancing or of the ordinary members of a community for some kind of entertainment. All the fine arts we now enjoy may have begun in just this way, going through some such change as I have described in the case of weaving and ceramics.

More generally, the specific changes whose details we can study in contemporary events—all three of the sequences described—have their historical counterparts in what are now regarded without question as high arts. The problem of how these sequences and others still to be analyzed interact with one another and how those interactions produce distinct segments of an art or craft world requires more research and more use of available historical materials. What are the typical arrangements of sequences, and under what conditions does one or another occur? How do artists and craftsmen with different orientations coexist? To what degree are different orientations toward work embodied in specific people or groups, and to what degree are they simply different orientations which anyone in that art or craft world may adopt from time to time?

A second point concerns the meaning of "art" and "craft." These folk terms have a life of their own and consequently built-in ambiguities arising from the complications of everyday life. But we can see from the sequences we have considered what considerations come into play when one or another way of talking about work is used. Craft implies practical utility, art does not; and when considerations of practical utility begin to be voiced, members of art worlds worry about art being subordinated to "mere craft." Craftsmen consider utility an important consideration, because what they do is done for someone else's use. The ideological point expresses an organizational reality, just as the artists mean their insistence on nonutility to indicate liberation from such constraint as a contrasting organizational fact.

Both viewpoints can use beauty as a key consideration. Art presumably always does or at least uses some quality with an analogous purpose of divorcing judgment of the work from any extrinsic considerations. Thus artists who would not use the term "beauty" might speak of "expressiveness" or "effectiveness" or some similar quality. Craft need not refer itself to a standard of beauty but sometimes does. When it does, it signals the

emergence of an artist-craftsman segment, a corresponding differentiation of the craft world's organizational apparatus and a complication and proliferation of career possibilities.

Virtuosity appears in both art and craft worlds as a standard of judgment. It implies an ability to handle a wide variety of techniques and materials with ease and efficiency and an ability to do what few others can do. That view is most typical of craft worlds, but the emphasis on sheer skill also appears in mature art worlds, ones in which what is to be done is more or less settled and how one does it is the major remaining question. The organizational basis of such a concern lies in the relative importance of the workers in the craft or art vis-à-vis their employers, patrons, or clients, for the standard of virtuosity involves esoteric knowledge of the inner workings of the art or craft; it is essentially a set of standards created and enforced from inside. Employers can judge for themselves whether what has been made or done suits their purposes, according to their own standards; they may feel equally able to decide whether something is beautiful. But no one can tell whether an object or performance displays virtuosity without learning and accepting the standards of the workers responsible for them. That acceptance indicates a limited degree of organizational autonomy for members of the occupation. But in the arts it also means that more established members of the occupation as well as knowledgeable critics and patrons, who also participate in setting those standards, have greater power and control than younger and more innovative workers.

"Art" and "craft," then, refer simultaneously to contrasting ways of bringing to bear in particular work situations the standards of utility, virtuosity, and beauty and contrasting situations of work. In the real world, these appear in all kinds of combinations, depending on historical circumstances.

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Travels through Inner Space: Family Structure and Openness to Absorbing Experiences[1]

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The tendency to have certain kinds of absorbing and self-altering experiences is found in samples of college students to be related to the patterns of decision making and to the size of the families in which the students were reared. It is also related to the development of primary relations in those families and to the sex and birth rank of the students themselves.

This is a study of relations between certain absorbing and self-altering experiences and some of their social correlates. The experiences are among the ones that correlate significantly with hypnotizability. They have, however, a wider significance and that will be the focus of my interest.

People who, in ordinary circumstances, *behave* in the manner of hypnotized subjects are themselves more susceptible to being hypnotized than are others. So are people who report having *experiences* that resemble those of hypnotized subjects (Hilgard 1965, pp. 296–312). It happens that most people are hypnotizable and most people also have some of these related experiences (Hilgard 1965, pp. 71–76; Hilgard 1970; Tellegen and Atkinson 1974). There are, however, wide differences among individuals in the frequency and intensity with which such experiences occur.

Tellegen and Atkinson (1974) provide a convenient classification of the relevant experiences. They speak first of experiences that involve "reality absorption": a "tendency to become immersed in movies, acting, nature, voices, past events, etc.": a person becomes so involved that he feels "swept away" and then experiences his whole consciousness as temporarily altered or he feels that he is fundamentally a different person in one role than he is in another. They refer, second, to "fantasy absorption," a tendency to treat imagined objects as if they were "real": for example, a person finds that the elaborations he gives to a story to make it sound better come later to seem as real to him as the actual events recounted. Third, there are dissociations (Hilgard 1974), activities in which the body seems split off from the personality, or in which some part of the personality seems split off from the main consciousness, the part that is separated

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appearing to operate with relative autonomy. Fourth, there is "openness to experience," more specifically an eagerness to get beyond the "world of logic and reason."

These are not all of the absorbing and self-altering experiences that have been found to be related to hypnotizability, but they are the ones that have the strongest relations (the correlations range from .3 to .5). Individuals tend to be quite stable in the frequency with which and the degree to which they have these experiences—even as they are quite stable in their degree of susceptibility to hypnosis.

Individuals tend to differ, however, in the kind of absorbing and self-altering experiences that they have in everyday life. Thus Josephine Hilgard describes two distinct kinds of absorptive experiences, distinct in that people who have one are unlikely to have the other. She finds that some people like to enter an imaginary world that is already created. They receive that world. They work within its terms and encounter what such a world provides them. This often seems the experience of people who lose themselves in movies, novels, music, religion, or nature. Other people—the ones she calls "adventurers" (Hilgard 1970, pp. 126–41)—like to take initiatives or risks and to try new activities that bring them into an imagined world: activities such as skiing, scuba diving, meditating, speculating, taking drugs, and playing new roles and trying out new selves. These people do more than receive and enter a world. They assemble materials that they find available—ideas, props, interpersonal relations, and so on—and, by their own continuing efforts, they partially create and sustain the imagined worlds in which they move.

Whatever these and other absorptive experiences involve, they entail puzzling changes in the relations of people to themselves and to others, and these changes occur frequently in the behavior of normal people in everyday life. The studies I am going to describe will show that in populations of college students such experiences are related to the experiences of the students in the families in which they were reared.

To understand these changes, I shall focus on one aspect of the varieties of absorbing experience that correlate with hypnosis: on a person's being moved from within by purposes that are not simply his own. I say "purposes" because the forces at work are like those that are distinctive of relations between people in their capacities as persons or between a person and a group. An individual encountering these forces views them as operating in a meaningful way, as being expressions or aspects of intentions (or of desires, orientations, and the like) which lie outside himself but which he can come to make his own. Take, for example, the reports of being "swept up" into a movie or novel or play. The person reporting this experience has imaginatively entered the plot and feels himself immersed in, and shaped by, the relations among the characters; a

participant in the interplay of their hopes and efforts, their successes and failures. Or consider the experience of being a different person as one moves to a new role: an experience of personal continuity and, simultaneously, of dissociation—of being created in a new form by one's special relevance for certain individuals or for a particular group. Or the implicitly purposeful and social qualities that people sometimes read into their encounters with nature, music, or art when they see these objects as worlds of meaning and understand them as having a "life" into which an individual can find his way and in which he can participate.

It is also characteristic of these absorbing experiences that a person sees his participation as, in part, voluntary. He is attracted by a story or by the sea or by a social role because of his own desires and not merely by the character of the object itself. He may be "overwhelmed" by nature or by music, but this is possible because he is receptive to them.

I find suggestive some similarities between absorbing experiences of this sort and certain experiences that we have as participants in groups. In one sense, we are the *users* of groups, participating in them in order to get what they offer for the satisfaction of our personal interests. Usership is what is stressed in introductory textbooks when the authors tell us that groups are created by people, that groups are facilities by means of which individuals satisfy their needs, and that, other things being equal, people stay together in a group only so long as they get enough from this association to make it worth their while. But people are also, and simultaneously, the *agents* of groups. They discover that to have a group, and receive its benefits, they must preserve and maintain that group; they must identify the group's interests and they must serve as the group's hands and feet in pursuing these interests. In this way collective interests come to be those of participants in a group—and not simply because participants are pressed to make them so but because participants, if only for their own sakes, want the group to prosper.

It is especially as agents that we are likely to have absorbing experiences of the sort I have been describing. As the users and creators of a group, we have the experience of being separate from one another, of being moved by our own personal impulses and interests, and of uniting only as it serves our concerns as individuals. As agents, and in many absorbing experiences, we are both motivated and authorized to serve interests that exist outside ourselves rather than being merely our own.

Our position as agents is easiest to see if it is formalized, as when we are the officers of a group, but all the members, and not only the officers, are agents of the group. In families, for example, the parents and the oldest children are likely to have at least an informal office as agents, but all members act as agents whenever they serve collective purposes, when their personal characteristics and powers and round of life are the ones

authorized by the corporate order in which they participate (Barbu 1960; FitzGerald 1972; Mauss 1938; Morris 1972; Swanson 1973; Tillich 1952, pp. 86–90; Ullman 1966; Zaehner 1957). Thus, for all the children in a family, there may be experiences of warm unity with parents and siblings, of the care and use of common property, of having to represent the family and embody its standards in school and in other public situations, of pulling one's weight in a collective effort, of roles, formal and informal, that one has in this group and not in others (the roles, perhaps, of oldest child or scapegoat or family clown), of being authorized to act for the group or of being made responsible for its welfare. There may be a sense of common purposes, of the family's traditions or rituals, of formative events in past generations that give the family its present course and its vision of the future. There may be the experience of being recognized as increasingly capable of sharing in the family's management, of exercising discretion on its behalf, of socializing newcomers to its ways, of supervising other members in the family's interest, and of becoming a part of a cosmos—of the family's special world as carved out by its collective concerns[2] (Erikson 1959, p. 97; Jaeger 1945, pp. 150–84; Kroner 1956, pp. 46–47). In their capacity as agents, the children have the experience of entering a meaningful order that they as individuals did not create but in whose purposes they come to share.

Experiences of agency are experiences with purposes not our own but of purposes which we are encouraged to make our own. Are they also experiences of imagined worlds?

It is helpful to remember that all action takes place in an imagined world. The usual distinction between action and other forms of behavior is that, in action, people relate to an environment, and employ their knowledge about that environment, according to its relevance for their "motives": their impulses, desires, wishes, interests, purposes, and the like. To act is therefore to imagine: to conceive the environment from a point of view afforded by some motive. In acting, we relate to the environment not as it "is" apart from our behavior, but in its anticipated—its imagined—import for our objectives.

Families and other groups train and reward us for competent participation in the imagined worlds fashioned by collective or corporate interests. Indeed it is a requirement for full participation in most groups that we do not merely conform to one or another of the group's rules but also acquire some understanding of the group's needs and purposes: that we come to take *whatever* action is appropriate to serve the group's con-

[2] For extended treatments of the formal properties of such worlds, see Lewin (1939) and Sorokin (1943). For a treatment of personal and social space from the view of literary criticism, see Poulet (1964). And, from the view of the interpretation of cosmologies, see Baldry (1965), Kirk and Raven (1957).

cerns (Swanson 1973, 1974*b*, 1976). It is only by such fluent adaptivity that we are able as agents to cope with the contingencies in which real situations abound.

It remains for future research to disclose the processes through which these abilities in and commitments to the serving of collective interests are learned and activated (Hartup 1970; Turiel, in press). We can see that there are stages in their development: young children act from their own point of view and come only later to appreciate that other individuals have objectives and, later still, to grasp the needs and purposes of collectivities. We can also see that young children act at first to serve their own interests, that they come later to serve the interests of other individuals, and that they come still later to act on behalf of collective interests. For present purposes, I shall simply assume that families and other groups generally have the means to bring their members to serve the group as its agents. The problem of just how that is done will be put to one side.

To summarize the argument to this point, the types of absorbing experiences I have described have similarities with the experiences of the agents of groups. In both cases, people (1) make their own some system of meanings and purposes which has an existence independent of theirs, and they (2) do so voluntarily, with the result that they (3) are moved by purposes and directed by perspectives that are not simply their own and thus involve them in a kind of dissociation.[3]

All of this can be contrasted with people's experiences as the users of groups. As users, people find themselves pursuing purposes that are their own and acting in terms of the perspectives that such purposes afford. Experiences as users are thus at variance with the imaginative involvements I am considering.

My first proposal, then, is that experiences as an agent of a group may provide the source, the occasion, or the encouragement for these imaginative involvements and that experiences as a user of a group may work in the opposite direction. More specifically, and with regard to children in families, I am proposing that firstborn children, because they are likely to be given special responsibilities as agents, will be more likely than others to have these absorbing experiences in imagined worlds. I want also to explore the possibility that, other things being equal, firstborn children reared in large families are more likely to have those absorbing experiences than are similar children reared in small families. The thought here is that the role of agent may become more sharply differentiated in large groups than in small (Blau and Schoenherr 1971).

Before investigating the truth of these proposals, we need to take ac-

[3] Life as an agent embodies the characteristics of primary process as well. For a treatment of primary process as a property of social relations, see Swanson (1974*b*).

count of another consideration. Families may differ, as may all other groups, in the relative value that they place upon collective interests as against the special, personal interests of their members. This factor should affect the imaginative involvements of their members as agents. Some families are so organized as to give primacy to personal interests, the group being regarded as primarily a means—however necessary and valued—to the ends of individuals. This is true, for example, in families organized as federations. The members are conceived as having special interests and the purpose of the group is taken to be the facilitation and the fostering of the members' personal careers. To be an agent in a federation is to be responsible for the attainment of the ends of the individual members and this requires that one not permit collective interests, important though they are, to be treated as the goals of collective action rather than as means.

Other groups, many families being among them, are organized to give primacy to collective interests and to afford those interests the position of corporate goals. Thus, in families organized as pure and consensual democracies, each of the parents, and, as they grow older, each of the children, is given equal status in the making of decisions. But decisions are defined as requiring everyone's agreement rather than the support of a majority. In this way, the decisions and goals arrived at in the family come to be those that represent the common and collective interest and not divergent or special interests. An agent of such a group is obligated to take collective rather than special interests as the goals of collective action.

Following an earlier usage (Swanson 1971), I will call groups "associations" to the extent that they give primacy to special interests, and "social systems" to the extent that they give primacy to collective interests. My proposal, then, is that agents in social systems will be more likely than other persons to have the experiences which engender imaginative involvements of the sort that are under discussion and that agents in associations will be less likely than other participants in either kind of group to have such experiences.

PROCEDURES

Indicators of absorbing experiences.—As we have seen, a large number of these indicators have been developed in connection with studies of hypnotizability. I have worked from the indicators, old and new, that Josephine Hilgard (1970, 1973) drew together for her recent research on susceptibility to hypnosis (Ernest Hilgard 1964, 1965, pp. 343–74). In her own investigations, Hilgard constructed a clinical interview to get at the "imaginative involvements" of normal people during their waking hours. An as-

sistant and I transformed Hilgard's questions and codes into a questionnaire that could be administered to undergraduates in college.

Hilgard had uncovered more indicators than one can conveniently employ in a questionnaire to be administered in a college classroom. In order to obtain a workable number of indices, we chose the ones that Hilgard found to have a significant relationship to hypnotizability (as measured on Form C of the Stanford Hypnotic Susceptibility Scale). Questions based upon these indicators and used in this study are given in the Appendix.

An indicator of a person's being an agent in a family.—As suggested earlier, I will use birth order (in conjunction with family size) for this purpose and will work from the assumption that firstborn children, especially those in large families, are more likely than others to have a differentiated status as agents. In these samples, a family will be called "large" if it has three or more children.

Birth order is probably not as satisfactory an indicator of agency as could be had from direct observations of a child's acting on his family's behalf. It seems, however, to provide a reasonable if rough approximation. The idea that firstborn children are morely likely than others to have a share in the family's management, and so to be quasi-formal agents, is in agreement with commonsense views and with the findings from research on personality and birth order. Those findings show that firstborn children are more likely than others to be responsible, to take initiative in establishing social relations or in setting them right, to try to meet standards of excellence set by others, to inhibit their own overt expression of aggression and other socially disruptive feelings, to be anxious about the rectitude of their actions and impulses, to be popular with adults and with their peers, and to be planful (Clausen et al. 1965; Pryor 1970; Sampson 1965; Sutton-Smith and Rosenberg 1970). Pryor (1970, pp. 43, 88–92) finds that (contrary to certain doubts expressed later by Schooler [1972]) these associations appear when family size and patterns of familial decision making are held constant.

An indicator of a family's organization as an association or as a social system.—Figure 1 contains descriptions of nine patterns of decision making in families that have been shown to distinguish between a group's organization as an association or as a social system (Swanson 1971). In this study, I asked college students who were still living at home with their parents to indicate the pattern in this list that came closest to the one by which their own parents made decisions and the pattern that had the next best fit.[4] (Evidence for the reliability and validity of this ty-

[4] The labels given these models in previous studies are: 1 = simple centralism, 2 = feudalism, 3 = unitary centralism, 4 = limited centralism, 5 = commensalism, 6 = commensal heteronomy, 7 = heterarchy, 8 = balanced centralism, 9 = individuated heteronomy (Swanson 1971).

Below are nine models of ways families make decisions. We would like your impression about how your family makes its decisions. Read through all nine of the models and choose the <u>one</u> that <u>best</u> describes your family. Put a <u>1</u> in the blank beside that model. Then put a <u>2</u> beside the model that is the <u>next best</u> description of the family.

Note that each model focuses on the parts the <u>parents</u> play in making family decisions. Models 1-4 describe families in which <u>one</u> parent has final decision making responsibility, if he wants, and models 5-9 describe families in which the parents have <u>joint responsibility</u> for most decisions.

___1. In this family, one parent is seen by all as being the "head" of the family. He makes the final decisions. However, the other parent has the right to give advice and to expect that his advice will be sought and taken into account.

___2. This family is identical with the one just described except that the "head" of the family makes the final decisions on the most <u>important</u> issues the family has to decide. In less important family matters, each parent has his own areas of responsibility and competence and has considerable freedom to decide what will be done.

___3. In this family, one parent is without question the head of the family. He can, if he chooses, have the final word in deciding what the group will do, and how it will do it. The other parent does not usually expect to be consulted for advice, even though he sometimes is.

___4. This family is identical with the one just described except that the decisions of the head of the family usually apply to the <u>most important</u> issues the family has to decide. In less important family matters, each parent has his own areas of responsibility and competence and has considerable freedom to decide what will be done.

___5. In this family, both parents discuss a problem together and agree on a solution <u>before</u> they announce their decision to the family or begin to carry it out.

___6. This family is identical with the one just described, except that "decision" is too strong a word for the parents' standard way of doing business. They talk over problems, or talk around them, until a sort of <u>common view</u> emerges. This view is then the basis for action.

___7. In this family, the parents often consult together about what the family will do, and agree on a solution before they announce their decision to the family. However, each parent also has the right to make decisions and act on his own. If the other parent wishes, he then can review this decision, and question and even delay the action, until both parents have consulted together and reached an agreement about the final course of action.

___8. This family is identical with the one just described except that, should the parents disagree on something that is urgent and important, one in particular has the right to break the tie and make the decision.

___9. In this family it is difficult to identify a standard procedure of decision making. Things are often done, or not done, pretty much as the moment dictates. The parents may or may not consult together about decisions. If either dislikes what the other is doing, about all he can do is to withhold resources (like money, time, encouragement) that are necessary for his partner's activities. Similarly, he often finds that the best way to get his partner moving, is to supply some of these resources specifically for the purpose he intends.

Figure 1. PATTERNS OF DECISION-MAKING IN FAMILIES

pology, and of variant forms, is given in Pryor [1970] and in Swanson [1971, 1973, 1974*a*, and 1976].)

Models 7, 8, and 9 are patterns of organization that have been shown in previous research (Swanson 1971, 1973, 1974*a*) to give high legitimation to the pursuit of special, personal interests and hence to fit what is meant by organization as an association. (Model 7, e.g., is that of the federal system which I have already described.) Model 9 is more an aggregate of individuals than it is a group or corporate body, but it clearly gives primacy to special as against collective goals.

Models 1, 3, and 5 have been shown in earlier studies (Swanson 1971, 1973, 1974*a*) to legitimate collective purposes as determining the goals of collective action and thus to meet the criterion for organization as a social system. (Model 5 has been found to operationalize a pure, consensual democratic system of the sort I have already discussed. In models 1 and 3, one parent speaks for the family in making the final decision. In most of the families in which model 1 or model 3 appears, the mode of decision making is justified on the grounds that the head of the family is indeed an agent, representing the collective interest rather than merely his own. The actions of other members of these families are legitimate insofar as they also embody collective interests; insofar as they implement decisions made by the family's head. There is no formal provision in these patterns of decision making for the pursuit of personal or special interests as the goals of collective action.)

Models 2 and 4 have been found in previous research (Swanson 1967, 1969, 1971) to give priority to the common interest. In that, they resemble models 1, 3, and 5. But they resemble models 7 and 8 in that the common interest is organizationally separated from the members' special interests, the pursuit of the latter also being legitimated and taken formally into account in the formulation and implementation of collective action. It seemed to me that in small informal groups, most families being among them, this provision for the pursuit of special interests was likely to be honored and to have a principal place in collective action. For that reason, I have grouped models 2 and 4 with models 7, 8, and 9 in this study as exhibiting organization as an association.

The remaining model, model 6, embodies an emphasis upon the common interest and upon personal interests, but it approaches model 9 in its lack of corporate structure (Swanson 1973, 1976). This combination of characteristics suggests that it is organized both as an association and as a social system but in only modest degree. On these considerations, I shall regard it as an intermediate case.

Table 1 contains a summary of the interpretations just stated and their relation to my orienting hypothesis. In terms of the ratings available in figure 1, the respondents thought most likely to have experiences in their

TABLE 1

EXPECTED RELATIONS BETWEEN PATTERNS OF FAMILY DECISION MAKING AND SCORES ON ABSORBING AND SELF-ALTERING EXPERIENCES

Expected Scores	Patterns of Decision Making Having: Best Fit	Next Best Fit
High	1, 3, 5	1, 3, 5, 6
Intermediate	All combinations not otherwise specified	
Low	2, 4, 7, 8, 9	2, 4, 6, 7, 8, 9

families as agents will be those who select from among models 1, 3, and 5 the pattern that best describes their families and also the pattern that provides the next best fit. Experience as an agent should be less available—and even in competition with authorization to be a user instead of an agent—among respondents who choose from among models 2, 4, 7, 8, and 9 both the best and the next best description of their families. When model 6 is given as the best description for a family, I code that family as intermediate between the two groupings just described. When model 6 is given as the next best description, I code the family according to the model given as the best description. All remaining combinations of the nine models are coded as intermediate. Thus, as specified in table 1, my orienting hypothesis is that experiences of agency, and therefore a tendency to have the sort of absorbing and self-altering experiences on which I have focused, will be most likely among persons reared in families characterized as models 1, 3, and 5 and least likely among those reared in families patterned on the lines of models 2, 4, 7, 8, and 9.

Samples.—The questions derived from Hilgard's studies, the questions on forms of decision making, and other questions were administered to students in three courses at the University of California, Berkeley, in the fall of 1972. Most of the students were enrolled in two large upper-division courses in social psychology offered by the Department of Sociology. A few of the students were enrolled in introductory psychology and completed the questionnaire in order to meet the requirement in that course that they participate as subjects in research. The students in psychology answered the questions in a classroom. The other students were asked to take a questionnaire home with them, complete it, and return it at the next meeting of their course. In all, 99 students completed the questionnaire.[5] Fourteen questionnaires could not be used because the students' families had been disrupted by the death, separation, or divorce of the parents. Ten others could not be used because the respondents indicated

[5] Colleagues tell me that the response rate in this study—about 44%—was typical at Berkeley under the then current rules on the participation of students in research.

the best but not the next best of the patterns of decision making that described their families. Seventy-five questionnaires proved generally usable. Because the relations between the independent and dependent variables are similar in all three populations, they are treated as one sample in all analyses. I refer to it as sample 1.

A year later, students in one of the upper-division courses already mentioned were given a questionnaire containing the items on absorbing experiences that were found from an analysis of the responses given by the students in sample 1 to be the most reliable. This questionnaire was administered in class as a class exercise. One hundred and three of the 126 forms returned proved to be usable in an analysis. These constitute sample 2.

In 1974, a colleague volunteered to use the questionnaire employed in sample 2 with a group of upper-division students in psychology who had expressed an interest in participating as subjects in studies of hypnosis. All 53 of the students present for the administration turned in questionnaires. Forty-nine of these were usable in most analyses. These constitute sample 3.

Some further questions.—The students in sample 1 were asked about some further points in their backgrounds and experiences (see table 4). Some of the questions need to be noted here.

Students were asked to characterize themselves and their families on certain demographic attributes. They were also asked to supply information on variables other than their absorbing experiences that Hilgard had found to be correlated with susceptibility to hypnosis. The latter fall into three groups.

1. Hilgard found that respondents who saw themselves as resembling their parents—in temperament and personality, in playfulness and enthusiasm, in attitudes toward work—and who felt close to their parents, especially to the parent of opposite sex, had higher scores on hypnotic susceptibility. She refers to indications of resemblance and closeness as measures of "identification" and that is the term used in table 4.

2. She also found higher scores on hypnotic susceptibility for respondents who saw themselves as disciplined severely and whose parents were not likely to use rewards or reasoning in shaping a child's behavior.

3. She found a negative relationship between hypnotic susceptibility and (*a*) tendencies toward neuroticism or (*b*) a "primitive" withdrawal into oneself, and she found a positive relationship between hypnotizability and (*c*) vivid mental imagery and (*d*) social acquiescence. My questionnaire contained measures for each of these variables: (*a*) a mental health inventory (Crandell and Dohrenwend 1967), (*b*) the "prenatal" or "pre-ego" scale from the Personal Preference Scale (Krout and Krout 1954; Stagner, Lawson, and Moffitt 1955), (*c*) the Sydney revision of Betts's

Mental Imagery Questionnaire (Hilgard 1970, pp. 284–85; Sheehan 1967, 1972), and (*d*) the 15-item version of the Social Acquiescence Scale (Couch and Keniston 1960; Block 1965, 1971; McGee 1962; Bentler, Jackson, and Messick 1971).

Students in sample 2 answered a question asked by Greeley and McCready (1973; Greeley 1974) of a national sample of adults. It concerns mystical experiences and reads: "How often have you felt as though you were very close to a powerful, spiritual force that seemed to lift you out of yourself?" The respondents answered according to the following alternatives: "Never in my life; Once or twice; Several times; Often; I cannot answer this question."

FINDINGS

The responses of students in sample 1 to questions about absorbing experiences were analyzed by means of the Tryon-Bailey (1970) program for cluster analysis. The three clusters given in table 2 exhaust 65% of the mean square of the raw correlation matrix. As table 3 indicates, Clusters I and II are related to one another ($r = .26$, $P < .05$). Cluster III is essentially independent of the other two. The diagonal values in table 3 give the reliability coefficients of the cluster scores on the defining

TABLE 2

CORRELATIONS OF APPENDIX ITEMS WITH OBLIQUE CLUSTER DOMAINS
(Rotated Oblique Factor Coefficients)

Items*	Cluster I	Cluster II	Cluster III
1 Involvement in novels	.34	−.22	.44
2. Involvement in music	.42†	−.20	.27
3. Involvement in movies	.80†	−.19	−.22
4. Involvement in plays	.60†	−.50	−.16
5 Involvement in televised stories	.49†	−.06	−.14
6. Involvement in religious experiences	.11	−.01	−.07
7. Involvement in nature	.20	−.05	.58†
8. Imagination stimulated more by external experiences.	.09	−.06	.30
9. Enjoy writing, painting, etc.	−.22	.18	−.43
10. Like to try out new roles	−.27	.60†	−.06
11. Feel like a different person in different situations	−.08	.17	−.11
12. Have flair for acting	−.23	.60†	.31
13. Had an imaginary companion in childhood‡.	.01	−.12	.16
19. Like to go tripping out	−.03	.48	−.22
20. Believe own improvements on stories	−.23	.20	.24†
21. Perceive past events as if occurring now	−.03	.10	.02
22. Focus so hard on something become benumbed	.10	−.10	−.39†
25. Find imaginative involvement in physical activities	.16	−.10	.47

* Numbers correspond to those of the relevant items in the Appendix. Items 1–7 coded as 1, "Involved" or as 0, other responses.

† Defining items by the Tryon-Bailey criteria.

‡ Sixteen respondents remembered having an imaginative companion in childhood (questions 13–17). Others may have had one and not have remembered it (Manosevitz, Prentice, and Wilson 1973).

TABLE 3

CORRELATIONS BETWEEN FACTOR ESTIMATES

Sample and Cluster	Cluster I	Cluster II[a]	Cluster III
1:			
I	.70**	.26*	−.06
II[a]	...	.54**	.10
III	...	...	.39**
2:			
I	.57**	.09	.02
II[a]	...	.47**	.21*
III	...	...	.70**
3:			
I	.58**	.12	.08
II[a]	...	.75**	.04
III	...	...	.44**

[a] The scores for Cluster II are reversed so that high scores indicate high imaginative involvement.

* $P < .05$.

** $P < .01$.

items for each cluster domain. All are significant at the .01 level or beyond.

We find in table 2 that the items that correlate best with Cluster I are numbers 1, 2, 3, 4, and 5. These items indicate a person's deep imaginative involvement in novels, music, movies, plays, and televised stories. By the criteria of Tryon and Bailey, items 2–5 are defining variables for this oblique cluster domain.

Cluster I seems to consist of absorbing experiences with what I shall call "received" meanings. The pattern of relations in which the reader or viewer becomes absorbed already exists in the story or drama and he is "carried off" into it, comes to "feel . . . a part of it" and to "feel deeply all that happens; sort of 'give myself up' to it," and he may "carry the experience around" with him for a time.[6]

As Table 4 indicates, scores on this cluster are not related to a roster of standard demographic variables including age, sex, family size, and socioeconomic status. They are also unrelated to all but one of the relations in the family and the characteristics of personality which Hilgard and others have found associated with hypnotizability. The one exception is a positive relationship between scores on Cluster I and scores that indicate vivid imagery on the Mental Imagery Questionnaire ($r = .25$, $P < .05$).

[6] Too few respondents were majors in fields other than the social sciences, humanities, and related professional schools to enable the use of question 18 in fuller analyses and thus to check on Mrs. Hilgard's observation that majors in these other fields are less likely to have imaginative involvements such as those in Cluster I.

TABLE 4

CORRELATIONS BETWEEN RESPONDENTS' SCORES ON CLUSTERS I, II, AND III AND THEIR PERSONAL AND SOCIAL CHARACTERISTICS: SAMPLE 1

	Cluster I	Cluster II	Cluster III
Personal:			
Age	−.05	−.18*	.13
Sex	.04	.09	.12
Birth order	.06	.02	−.28***
Family:			
Size	.08	.05	−.08
Breadwinner self-employed	.07	−.06	−.04
Number of supervisory levels on breadwinner's job	.09	.11	.09
Family income	−.17	−.03	.28***
Father's education	−.01	−.01	.17
Mother's education	−.06	−.08	.14
Number of grandparents born in the United States	.10	.04	.22**
Relations in family:			
Use of corporal punishment	.03	.01	−.11
Severity of discipline	−.06	−.02	−.04
Number of interests shared with father	−.05	−.06	−.02
Number of interests shared with mother	.14	−.11	.01
Identification with parent of opposite sex	.09	−.07	−.004
Personality:			
Neurotic symptoms	.07	−.02	−.06
Pre-ego dispositions	.12	−.01	.15
Mental Imagery Questionnaire	.25**	−.13	.07
Social acquiescence	−.02	−.14	−.24**

* $P < .10$.
** $P < .05$.
*** $P < .01$.

Scores on the next cluster domain, Cluster II, are negatively related to their defining items in table 2 and I have reversed them for all interpretations and analyses. As reversed, they are defined by items 10 and 12: liking to try out "new roles, play new parts, really get into a different way of seeing and feeling and behaving"; "acting as if you were someone else—a character in a story or a person you've seen or heard about." Other items that load substantially on Cluster II are items 4 (imaginative involvement in plays) and 19 (the tendency to "go tripping out on all kinds of things—astrology, yoga, drugs, mysticism, philosophical speculation, even speculative science are some examples" and, by these means, to "get . . . involved in a different world"). All of these items seem to indicate an active seeking out, or the partial creation, of absorbing experiences and I shall refer to them as experiences with partially created meanings.

Taken together, Clusters I and II seem to concern absorptive experiences and to embody something of the distinction uncovered by Hilgard between receiving and entering an imagined world as against going out to

find or to help create such a world in which one then can travel.[7] In contrast, Cluster III seems to tap something other than absorbing and self-altering experiences.

The items with the largest loadings on Cluster III concern, again, (1) immersion in novels and also (7) imaginative involvement in nature, (9) the enjoyment of "writing, painting, composing, sculpting, or other things of that sort," and (25) one's finding that sports or physical activities give one immediate pleasure or provide adventure, excitement, or totally involving experiences (25: *a, c, e, h, i*). Item 7 is given by the Tryon-Bailey criteria as a defining item for this domain as are two other items: (20) having "the experience of telling a story with elaborations to make it sound better, and then having the elaborations seem as real to you as the actual incidents," and (22) focusing "at something so hard that you went into a kind of benumbed state of consciousness . . . or a state of extraordinary calm and serenity."

A picture of this cluster's domain emerges from an examination of the correlations given in table 2. It is one of reflectiveness and self-possession (imagination stimulated more by one's own thoughts than by external stimuli; little enjoyment in pretending to be someone else and little flair for such pretense) coupled with willingness to acquire and employ some sophisticated tastes and skills (reading novels, listening to music or producing it, engaging creatively in any of several artistic activities or in sports) for their own sake—that is, for the immediate excitement, enjoyment, or involving experiences that they afford. The acquisition of most of these tastes and skills and the enjoyment of them for their own sake seem more likely on the part of students reared by parents who have the education to value a culturally sophisticated outlook and the money to provide training for it. The interpretation is consistent with the positive correlations in table 4 between scores on Cluster III and two indices of high social status: family income ($r = .28$, $P < .01$) and number of grandparents born in the United States ($r = .22$, $P < .05$). It also seems consistent with the negative relationship between scores on Cluster III and the measure of social acquiescence ($r = -.24$, $P < .05$). This interpretation may also be consistent with the finding that there is a negative correlation between scores on this cluster and birth order, only children and earlier-born children tending to have higher scores than do children born later in a particular sibship ($r = -.28$, $P < .01$). Cluster III may get at the same kind of outlook that is tapped by many measures of "imaginativeness" or "creativity." At any rate, some measures of imaginativeness and creativity have been shown to have relations to social

[7] A related distinction is found in the correlates of susceptibility to hypnotic induction as against susceptibility to hypnotic trance (Lee-Teng 1964). Induction susceptibility is related to adventurousness and to obtaining enjoyment from playing roles.

status and birth order like the ones just presented (Brooks 1973; Singer 1973; pp. 67–70, 133–37, 148, 180, 189, 197–98).[8] Cluster III is of interest in its own right, but I will set it aside to focus on Clusters I and II which seem more obviously to indicate absorbing experiences.[9]

The defining items for Clusters I, II, and III were administered to the students in samples 2 and 3. Analyses of their responses produced Clusters I and II again and, in each population, a third cluster that was unique to that sample. Table 3 contains the correlations among the factor estimates.

An analysis of the data also shows that, in samples 1 and 2, the defining items for Cluster I or II, the size of the loadings, and the specific additional items that load heavily on one or the other of these clusters are essentially the same. For that reason, the cluster scores obtained in sample 2 were derived from the item weights obtained in sample 1. The items that define these two clusters in sample 3, and their loadings, are essentially the same as in the first two samples, but there are differences in the specific additional items which load heavily on one cluster or the other. For that reason, scores for the clusters derived from sample 3 are based on weights obtained from that sample.

An inspection of relations between scores on Cluster I and patterns of decision making in families and birth order uncovers one large trend. This is shown in table 5. Among firstborn children (the ones that seem most likely to be defined as agents), the scores on Cluster I are highest for those reared in families organized as social systems and lowest for those reared in families organized as associations. Moreover, the scores of firstborn children reared in social systems are higher than those of any category of later-born children and the scores of firstborn children reared in associations are lower. (Among firstborn children, only children do not differ from others in these respects.) This set of relations appears in all three samples. (Computations not shown here indicate that, in each sample, it is somewhat stronger in large than in small families but not significantly so.) A "classical experimental" analysis of variance based upon the combined data for all samples in table 5 discloses a significant effect

[8] But see Schooler (1972) for a cautionary review of many of these and related studies on birth order and its correlates.

[9] Finke and Macdonald (1976) have used the defining items for Clusters I, II, and III, together with other items that load significantly on those clusters, to form a 13-item scale (in table 2, items 1–5, 7, 9, 10, 12, 19, 20, 22, and 25). In a population of 188 undergraduates at the University of New Hampshire, they found an r of .66 ($P < .001$) between this scale and Tellegen's (1975) measure of absorbing experiences and an r of .27 ($P < .001$) with Form C of the Stanford Group Scale of Hypnotic Susceptibility. They also report that "no differential effects could be resolved among the various subfactors [of the 13 items] in relation to hypnotizability" and conclude that the "absorption factor measured by [this scale] . . . is apparently quite general, more a function of the number of items . . . than of any one particular type or class."

TABLE 5

CLUSTER I: MEAN SCORES BY BIRTH RANK AND PATTERNS OF FAMILY DECISION MAKING

Sample and Birth Rank	Families That Are Predicted to Be*: Low	Middle	High	N	η	$P\leq$
1:						
First	38.4	49.8	63.1	35	.69	.001
Later	46.2	50.8	54.5	40	.23	.13
2:						
First	43.1	47.5	57.8	40	.34	.001
Later	47.4	50.1	47.4	63	.13	.28
3:						
First	44.5	48.3	54.5	14	.59	.002
Later	48.4	53.8	48.8	35	.22	.17

* The specific patterns are given in table 1.

for patterns of decision making ($P < .001$) and for the interaction of those patterns with birth rank ($P < .006$). (In table 5 and in all succeeding tables, scores on clusters are standard scores with a mean of 50 and a standard deviation of 10.) All of these findings in table 5 are predicted by the theory presented earlier in this paper.

Given the significant correlation in sample 1 between scores on Cluster I and the Mental Imagery Questionnaire, one might wonder whether the association in table 5 remains if scores on mental imagery are held constant. It probably does. The correlation for firstborn children in sample 1 between scores on Cluster I and the models of family decision making is .67 ($P < .001$). The relationship for firstborn children remains significant when scores on mental imagery are held constant ($r_{12.3} = .52$, $P < .001$). The association between Cluster I and mental imagery drops from a significant .53 ($P < .001$) among the firstborn (for later-born children this correlation is only $-.10$ [$P < .10$]) to a partial correlation of .23 ($P < .18$) when patterns of decision making are held constant.

Because Cluster I gets at absorption in received meanings, one might wonder whether women tend to have higher scores than men. The argument, based upon a host of studies (Maccoby and Jacklin 1974), would be that women are more likely than men to be trained in a receptive orientation toward social relations. This proposal is not sustained: scores on Cluster I are not significantly related to sex in any sample. There is, however, a tendency in large families for earlier-born women to have higher scores than women who are the last born children in their families. Table 6 shows that this occurs in all samples although it is statistically significant only in sample 1 and in the combined data for all three samples. Among men, there is a lesser, and statistically insignificant, tendency toward an opposite relationship between birth order and scores on Cluster I. (An

TABLE 6

CLUSTER I: MEAN SCORES IN LARGE FAMILIES BY SEX AND BIRTH RANK

Sample and Sex	Birth Rank: Last	Middle	First	N	η	P≤
1:						
M	52.5	56.5	44.5	18	.37	.08
F	39.5	49.1	50.5	34	.32	.05
2:						
M	47.9	40.8	44.5	22	.23	.28
F	40.5	50.8	48.5	34	.31	.06
3:						
M	50.2	48.5	47.0	25	.15	.45
F	44.5	52.0	47.8	12	.23	.43
All:						
M	50.5	37.5	45.2	65	.18	.14
F	40.33	50.5	49.5	80	.29	.004

analysis of variance employing the combined data from all samples shows that the interaction of sex and birth rank has a significant relationship to scores on Cluster I [$P < .02$].) These patterns are suggestive, but only that. They may indicate that women whose roles as women are the most differentiated (earlier-born in large families) are more likely to have absorbing experiences that are receptive in character.

A study of relations between Cluster II and the characteristics of the students and of the families in which they were reared reveals significant associations only within large families. This may occur because, as table 3 shows, Cluster II is less reliable (in samples 1 and 2) than is Cluster I and therefore has its associations only in families in which personal and collective interests are more likely to be differentiated in a clear way. Or it may be more a function of the contents of Cluster II. Cluster II has especially to do with the propensity for taking new roles and experiencing self-alterations when doing so. Perhaps the critical point is that clearly differentiated roles—these including managerial roles—are more likely to be available in larger families and that, in such families, children, especially firstborn children, are encouraged and empowered more often to be *what a given role requires* than simply to "be themselves."

Among firstborn children in large families, we find that being reared in a family organized as a social system is related to a relatively high score on Cluster II and being reared in a family organized as an association is related to a low score. As table 7 shows, this pattern appears in samples 1 and 2 and, less sharply, in sample 3. In sample 3 (where Cluster II has a higher reliability than Cluster I), there is a significant tendency for this pattern to occur in the data which combines later-born children in large families with children from small families. The main effect for patterns of decision making in relation to Cluster II is signif-

TABLE 7

CLUSTER II: MEAN SCORES BY FAMILY SIZE, BIRTH RANK, AND PATTERNS OF FAMILY DECISION MAKING

Sample, Birth Rank, Family Size	Families That Are Predicted to Be*: Low	Middle	High	N	η	$P \leq$
1:						
Firstborn in large families..........	45.5	50.5	61.2	35	.70	.001
All others..........	49.7	52.8	50.7	52	.13	.35
2:						
Firstborn in large families..........	45.8	50.9	64.5	20	.49	.005
All others....... ..	52.1	50.9	45.6	83	.19	.08
3:						
Firstborn in large families..........	52.5	56.5	54.5	11	.26	.38
All others.........	42.5	51.9	47.8	32	.34	.03

* The specific patterns are given in table 1.

icant ($P < .02$) when data from all three samples are combined for an analysis of variance.

The contents of Cluster II encourage the exploration of further possibilities. We are dealing in this study with populations of young adults, all in college and most of them unmarried. It seems likely that much of their imaginative taking on of new roles occurs in anticipation of roles they are about to assume as "independent" adults: for example, roles as citizens, employees, spouses, and parents. Many of these anticipated roles differ by sex. In Parsons's (1955) convenient summary, males are trained to be task leaders; females to be social-emotional leaders.

As task leaders, males are expected to know and promote the "economic" and "political" roles in groups: to represent the collective interests of the group to the outer world and the group's overarching interests and situation to its members. It may be, then, that males are more likely to try out anticipated activities as task leaders—and, in that sense, to try out new roles instead of being "merely themselves"—if they are aided and encouraged to do so by the structure of the families in which they have been reared. Families organized as social systems may be more promotive of these activities than are associations because collective interests are more clearly defined in social systems and so are managerial or task roles. Table 8 shows that there is a tendency in all samples for later-born males in large families to have higher scores on Cluster II if they are reared in families organized as social systems. The trend is statistically significant in samples 1 and 2 and in the combined data for all three samples.

Turning now to females and social-emotional leadership: in Parsons's

TABLE 8

CLUSTER II: MEAN SCORES FOR LATER-BORN MALES IN LARGE FAMILIES BY PATTERNS OF FAMILY DECISION MAKING

Sample	Patterns Predicted to Be*					
	Low	Middle	High	N	η	P≤
1	51.2	51.5	64.5	18	.49	.007
2	45.5	53.3	52.0	22	.44	.02
3	46.3	51.4	54.5	26	.31	.09
All	47.1	51.9	55.8	66	.34	.002

* As given in table 1.

formulation, social-emotional leadership means special responsibility for the inner life, and, in small, informal groups, for the interpersonal integration and harmony—the "primary relations"—which can exist. Perhaps it is in families in which such integration and harmony are established that roles in social-emotional leadership are most clearly defined and available. A question concerning primary relations was asked of respondents in all samples:

> To what degree do the members [of your family] anticipate the needs and interests of others in the family and give freely of themselves and their resources? [Check one:] (1) Members take the initiative in providing for the needs and promoting the interests of the others. They *anticipate* others' needs and provide for them. (2) Members provide for individual needs in their family as these needs appear. They do not anticipate these needs, but, once a need becomes apparent, they give freely. (3) Members serve the needs and interests of other members when they are *requested* to do so. (4) Members serve the needs and interests of others only when they are *pressed* to do so.

Table 9 shows that, among later-born females in larger families, respondents who checked the third or fourth possible answer to this question tend to have lower scores on Cluster II than do other female respondents.

TABLE 9

CLUSTER II: MEAN SCORES FOR LATER-BORN FEMALES IN LARGE FAMILIES BY RATINGS OF PRIMARY RELATIONS

Sample	Primary Relations Rated As					
	3 or 4	2	1	N	η	P≤
1	43.1	53.3	51.2	21	.35	.09
2	46.2	48.5	53.6	27	.30	.10
3	44.5	44.5	54.5	9	.35	.26
All	44.5	49.5	53.0	57	.28	.02

This trend appears in every sample but is statistically significant only for the data from the combined samples.

We might wonder whether later-born males in those large families that are organized as social systems are more likely than other later-born males to have high scores on Cluster I and whether later-born females reared in families characterized as having strong primary relationships have higher scores on that cluster. We might also wonder whether the index of primary relationships is associated with high scores on Cluster II for any group other than later-born females in large families. The answers to these questions are negative. I will consider the questions further in the next section of this report.

DISCUSSION

There are, it seems, associations between the degree and manner in which young adults lose themselves in imaginary worlds and certain experiences provided them in the families in which they are reared. In this study, there is support for proposals that experiences as an agent of the family, as a particular kind of agent (task leader or social-emotional leader), and experiences as an agent in a group organized as a social system or as an association are important. There is also support for the thesis that conditions which make it more likely that a person's role as an agent will be clearly differentiated (larger group size) make it more likely that effects of all these other experiences in the family will be heightened.

Among the points that may require further attention is the propensity for becoming involved in received worlds as against worlds that one partially creates. I say "may" because the differences in content between Clusters I and II could prove of little importance. It is possible, for example, that these clusters are produced as much by the format of their items as by differences in their content. To be specific, Cluster I consists of items from the series numbered 1–7 in the Appendix. The respondent is instructed to use a particular set in answering these items and only these. The key items in Cluster II (items 10 and 12) are by far the most highly correlated pair of the items in the Appendix ($r = .81$) and may therefore come to be a separate cluster by virtue of their so closely replicating one another.

A second reason for caution stems from Finke and Macdonald's (1976) finding that the relation to hypnotizability of the items in Clusters I and II is essentially the same. This might mean that, as indicators of absorbing experiences, the clusters are not essentially different.

There are, however, reasons to take the clusters seriously and to think that they catch an important substantive difference. The clusters appear in all three samples and, in each, are statistically reliable and relatively in-

dependent of one another. They correspond in content to the two modes of absorbing experiences that Hilgard discovered to be independent of one another in their relations with hypnotizability. Finally, the fact that both clusters catch absorbing experiences and yet are different suggests that there should be both similarities and systematic differences in their social correlates. The interpretations and the analysis of data that I have presented are consistent with that expectation.

One finding from these analyses seems especially interesting and needs to be pursued: the finding of differences by sex in the social correlates of scores on Cluster II—differences that appear only among later-born children. I cannot say why this occurs but can suggest two possible lines of explanation to be explored. (1) It may be that firstborn children have a more uniform role in families, this uniformity transcending differences in sex roles, and that later-born children, because they have a less well-defined position in the organization of their families, are more likely to base their imaginative behavior on sex-specific activities. (2) We have to explain why the scores of later-born women are related to the strength of primary relationships in their families of origin. Perhaps later-born women are encouraged to promote the one-to-one, interpersonal relations in families and firstborn women to promote the social-emotional aspects of more broadly collective relationships in those families. It may follow from this that the imaginative behavior of the later-born is shaped more heavily by the family's degree of interpersonal intimacy.

Whatever the answers to these questions, it is clear that a great deal of the variance among individuals in absorbing and self-altering experiences remains to be explained. Efforts to explain that variance should be facilitated by a clearer picture of the universe of such experiences. The experiences that are examined here represent only one section of that universe. They need to be compared and contrasted with others. For example, there are resemblances between dreaming and the involvements on which I have focused, but dreaming occurs when we are asleep. The involvements treated in this paper entail imagery, but these images, unlike some others, are not necessarily vivid or specific, imaginative or "merely imaginary," eidetic in character or aesthetic in import (Gray and Gummerman 1975; Haber and Haber 1964; Heller 1969, pp. 99–170; Pylyshyn 1973; Singer 1966). Unlike some absorbing experiences, the ones treated here may, or may not, give a large place to ego defensiveness, irrationality, or regression (Hilgard 1965, pp. 377–97; 1971). They do seem to entail properties that are commonly associated with "primary process," but the sense in which this is so is not clear (Csikszentmihalyi and Bennett 1971; Csikszentmihalyi 1974; Swanson 1974*a*). They have been shown to be correlated with hypnotizability, but not to such an

extent that one would believe hypnotic behavior and these forms of imaginative involvement to be one and the same (Orne, Sheehan, and Evans 1968; Zimbardo, Maslach, and Marshall 1972). As described by indices employed in the present study, the imaginative traveler in inner spaces need not experience trance or bodily dissociation or any overt behavior that is in any way exceptional, yet these phenomena accompany other sorts of absorbing experience (Aberle 1966; Knox 1950; Mannheim 1936, pp. 190–97; Sarbin and Coe 1972, pp. 103–39). By contrast with some other forms of absorption, the person involved in the ways I describe in this paper may, or may not, seek out the experience of an imaginary world, plan for it, drift into that world, or unexpectedly find himself engaged with it (Peckham 1962; Poole 1972). It may or may not be his use of some collective rituals that occasions his trip and guides its course. In the end, he may feel that his experience has opened up new appreciations or understanding, or he may not.

I have, however, a bit of information on relations between Clusters I and II and a kind of mystical experience. As indicated earlier, the students in sample 2 were asked Greeley's question on mystical experience: "How often have you felt as though you were very close to a powerful spiritual force that seemed to lift you out of yourself?" The answers are not significantly related to Cluster II, but they are related, positively and significantly, to Cluster I ($r = .20$, $P < .05$).

Greeley's question and Cluster I both tap a respondent's experiences with organized, purposeful entities that have an independent existence. They differ in that Greeley gets at a form of "possession"—of being dominated and overwhelmed by such entities (Bourguignon 1968, 1973, 1974; Butler 1927; McNeil 1951; Norbeck 1961, pp. 83–100, 213–28; Wallace 1966, pp. 102–66; Zaehner 1957, pp. 153–92), whereas in Cluster I the person voluntarily makes such independent purposes his own. We have found that high scores on Cluster I are related to being a firstborn child reared in a family that is organized as a social system. Perhaps high scores on Greeley's question are related to having been reared in that same kind of family—hence to being exposed to powerful, independent purposes, but with the qualification that one has a position in that family which makes one more the *object that is shaped* by such purposes than an agent that participates in applying such purposes to others. That would suggest that the later-born children in families stressing the common interest might have higher Greeley scores. Table 10 shows that this is the case, specifically for the last-born children ($P < .02$). (An analysis of variance shows that the interaction of birth rank and patterns of decision-making in this table approaches conventional levels of significance [$P < .07$].)

TABLE 10

MYSTICAL EXPERIENCE: MEAN SCORES BY BIRTH RANK AND PATTERNS OF FAMILY DECISION MAKING

	Families That Are Predicted to Be*					
Birth Rank	Low	Middle	High	N	η	P≤
First	3.19	2.73	3.83	38	.25	.11
Middle	3.29	3.00	2.50	30	.24	.18
Last	2.58	3.02	5.50	34	.34	.02

* As given in table 1.

From the studies of G. H. Mead (1934) and Piaget (1926), social psychologists have become accustomed to thinking of reflective self-consciousness as an internalization of social structure and processes (Swanson 1974*a*). The present studies suggest that this may also be true of certain forms of absorption and self-alteration as one travels in imagined worlds.

APPENDIX

How involved or absorbed are you likely to become when you are doing the things in questions 1 through 7? Use the following numerical scale and circle the number of the alternative that describes you best:

1. Involved: carried off into it; feel as if I were a part of it; feel deeply all that happens; sort of "give myself up" to it; may carry the experience around with me for a time.

2. Interested: want to see how it develops; like to analyze the way it's put together and why it comes out as it does -- whether it seems right and makes sense; like to learn something new.

3. Rather detached: the experience has some interest but doesn't usually grip me; often find myself thinking of other things as well.

4. Gets me away from problems and pressures: the experience isn't so gratifying in itself, but it takes me away from things that are unpleasant or worrisome.

5. Not involved at all.

1. 1 2 3 4 5 When reading a novel or a story of some sort

2. 1 2 3 4 5 When playing, singing, or listening to music

3. 1 2 3 4 5 When watching a movie

4. 1 2 3 4 5 When watching a play

5. 1 2 3 4 5 When watching a story on TV

6. 1 2 3 4 5 In religious experiences

7. 1 2 3 4 5 In encounters with nature

8. Would you say that, as a child, your imagination was (check one):

___1. much stimulated by reading, movies, games, and things like that, or
___2. did your daydreams develop from within yourself, more from your own thoughts?

9. Do you yourself enjoy writing, painting, composing, sculpting, or other things of that sort?

___1. A lot
___2. Some
___3. Little or not at all

10. Do you like to try out new roles, play new parts, really get into a different way of seeing and feeling and behaving? (check one)

___1. A lot
___2. Some
___3. Little or not at all

11. Some people feel that they are one person in one social setting, a different person in a second, still another person in a third, and so on. What about you, from one social setting to another? (check one)

___1. Often seem to be a different person
___2. Seem pretty much the same person, just doing different things

12. Do you have some flair for acting as if you were someone else -- a character in a story or a person you've seen or heard about? (check one)

___1. A lot
___2. Some
___3. Little or not at all

13. Many children have an imaginary companion. Did you? (check one)

___1. Yes
___2. No (if you checked this, skip to question 18)

14. If you had an imaginary companion, did it try to tempt you or to set you straight? (check one)

___1. Tempt me
___2. Set me straight
___3. Neither

15. Did you and your companion do things together?

___1. Yes
___2. No

16. Was your companion more a comrade or an audience for you?

___1. A comrade
___2. An audience

17. Was your companion something that we ordinarily think of as behaving -- a person or fairy or animal, for example -- or was it something that we don't ordinarily talk of as behaving -- something like a picture or a chair or a tree?

___1. Behaving
___2. Not behaving

18. Most recently what is your actual or probable major subject in school? Is it in the (check one):

___1. Biological sciences
___2. Physical sciences
___3. Social sciences
___4. Humanities
___5. Professional schools: (specify which one):___________________

19. People these days go tripping out on all kinds of things (astrology, yoga, drugs, mysticism, philosophical speculation, even speculative science are some examples). What about yourself? Do you like to get totally involved in a different world? (check one):

___1. Yes
___2. No

20. Have you ever had the experience of telling a story with elaborations to make it sound better, and then having the elaborations seem as real to you as the actual incidents?

___1. Yes
___2. No

21. Have you ever had the experience of recollecting a past experience in your life with such clarity and vitality that it was almost like living it again? Or so that it actually seemed identical with living it again?

___1. Yes
___2. No

22. Have you ever focussed at something so hard that you went into a kind of benumbed state of consciousness? Or a state of extraordinary calm and serenity?

___1. Yes
___2. No

23. What are the sports or physical activities for which you feel the greatest love or enthusiasm? (Examples might be skiing, surfing, baseball, horseback riding, swimming, etc.). List the one, two or three that are most important to you.

1.
2.
3.

24. How enthusiastic are you about each of these activities? (check an answer for each one)

Activity 1___very enthusiastic
___enthusiastic
___have some interest in it

Activity 2___very enthusiastic
___enthusiastic
___have some interest in it

Activity 3___very enthusiastic
___enthusiastic
___have some interest in it

25. Check those items from the following list which most effectively express some of the reasons why you like and enjoy the particular activities you chose in question 23, and the things that seem most true to you about each of these activities.

Activity Number
1 2 3

__ __ __a. I like to do this because it gives me lots of pleasure; I enjoy it; it's fun to do; and I do it for the pleasure it gives me at the moment.

__ __ __b. This activity takes a lot of physical stamina; I have to give it everything I've got. It's that last ounce of effort that really counts.

__ __ __c. I enjoy this escape from the world of reality. When I'm into this activity, I feel as if I am in another world, and I enjoy this feeling immensely. I really lose myself in my enjoyment of the experience.

__ __ __d. I'm always striving to improve or perfect my performance in this activity. It takes a lot of effort to do well, and the effort and improvement are very satisfying to me.

__ __ __e. I get deeply involved in this activity; when I am doing it I think of nothing else, and throw myself into it completely.

__ __ __f. I have to be always alert, trying to figure out what is going on around me; it takes a lot of concentration and alertness to keep from making serious mistakes at this activity.

__ __ __g. One reason I like to do this is because I can express my feelings of competitiveness; really strive to win; to be the best. I love to win.

__ __ __h. Doing this is an adventure. It's hard to say what will happen next; things are always changing, new challenges always arising; there is always a new experience around the corner.

__ __ __i. I like the excitement and feeling of power, or thrill of speed. The experience is really exhilarating.

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Social Bases of Independent Public Expression in Communist Societies[1]

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Owing to official support of cultural traditions, the critical aspects of Marxism, and the development of limited autonomy in cultural institutions, independent public expression can and does develop in Communist societies despite the will of the political elite. In this paper, independent public expression is shown to be inherent in Communist societies; consequently, the relationship between politics and cultural life in these societies is considerably more complex than observers have posited.

A major failing of many sociological analyses of Communist systems conducted in both the West and the East is that the official sources of information and the official theoretical framework which orders this information too often have been taken at face value. This is as true for the antagonists of the system as it is for the protagonists. Thus, for example, party ideologists have declared the end of class antagonism and have stressed that in the new society the collective good is more important than the good of the individual (see Marcuse 1961). Western observers have analyzed these contentions as if they were central characteristics of the social structure of Communist nations and of the value system of the general population, while, in fact, important class cleavages still exist in Communist nations, where social exhortations have often failed to motivate the citizenry when they are not linked with individualistic benefits. (Thus the recurring problem of low productivity.)

Analysts of public expression in Communist societies have been especially prone to confuse the official viewpoint of social reality with the actual situation. The official conception involves the reduction of all public expression to its political content. Lenin, in his discussion of party organization and party literature, made explicit the party view of public

[1] The term "Communist society" is used here with some reservations. Within these societies, the term "socialist society" refers to the present order, while "communism" refers only to the distant goals of the social order. Yet, using the term "socialist society" would confuse societies directed and controlled by Communist parties with other societies which consider themselves socialist. Thus, the best distinguishing term might be "Communist-party directed and controlled society." This is indeed what I am referring to, but because of the awkwardness of the phrase, I use simply Communist society.

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expression in 1905—well before the Communist ascent to power. He declared: "All Social-Democratic literature must become Party literature. Every newspaper, journal, publishing house, etc., must immediately set about reorganizing its work, leading up to a situation in which it will, in one form or another, be integrated into one Party organization or another. Only then will 'Social-Democratic' literature really become worthy of that name . . . and merge with the movement of the really advanced and thoroughly revolutionary class" (Lenin 1974). Though Lenin may not have conceived that the practical implications of this statement would become the sanction for the elaborate system of political control of public expression which exists in present-day Communist nations (see Solomon 1974, pp. 163–69), statements such as this one have been cited by party ideologists as the explanation for the compatibility of "progressive" censorship of public expression with freedom, and so constitute the starting point of Communist policy concerning public expression (for a recent Soviet view, see Kunitsyn 1971).

Most sociological studies conducted in the West of public expression in Communist nations do not dispute the official depiction of public expression. These studies focus on the political control of public expression before analyzing the social situations, roles, and functions of officially promoted and officially repressed public expression. Many observers have concentrated their attention on the nature of officially supported public expression (see, e.g., Simmons 1961, pp. 469–78; Hollander 1966, pp. 352–64; and P. Johnson 1965). In their studies the institution of political control is scrutinized, and the function of politically promoted expression is analyzed. Not surprisingly, sanctioned public expression is found to be supportive of the ruling political elite. Other studies have focused on the social situation and function of expression that is officially repressed (see, e.g., Monas 1968, pp. 2–17; Lourie 1974, pp. 328–39). Such studies view political control of public expression as somehow unnatural and enumerate types of underground responses to this unnatural state of affairs. They show how these various responses are "natural" reactions to an oppressive order.

The line between officially supported propagandistic expression and officially repressed dissident expression cannot be drawn as neatly as these studies imply. Public expression supported by the party and state does not necessarily mirror party values, and public expression repressed by the state is not necessarily dissident. Official policies with direct influence on public expression do not simply have the one-dimensional consequence of promoting supportive expression and repressing politically dissident expression.

Friedberg has noted that much of the clandestine expression in the Soviet Union is not politically seditious and is not in opposition to the socialist

order (Friedberg 1968, pp. 18–23). He observes that since public expression which is not seen to be in the interest of the Soviet state and the party is openly censored and denounced, the amateurish, the sophomoric, and the just plain mediocre writers are able to rationalize the suppression of their work as politically motivated.

Similarly, not all officially supported forms of public expression are simply propaganda and media for the inculcation of party ideals. Some officially supported expression, in fact, has forthrightly criticized the existing "socialist" order. Ideals derived from Marxism and stressed by the political elite are used at times as the basis of criticism of party and governmental policy. Prerevolutionary high culture, both national and international, has been made available to the whole of the population for the first time due to party cultural policies and provides alternatives to party-promoted models of cultural expression. Institutions established or reconstituted for the purpose of disseminating "party-minded" expression develop limited but still significant autonomy and disseminate expressions other than party-prescribed ones. In short, the consequences of official policies toward expression are not always in keeping with the party line.

In the analysis that follows, attention is focused primarily on structural factors inherent to Communist systems which foster officially accepted independent expression. The major thesis of this paper is that these factors have been present not only during periods of "liberalization" but also during the "darkest days of Stalinism." My main concern is not simply to account for how independent expression reaches the public during severe and liberal periods (for a discussion of this, see Goldfarb 1976*b*) but to account for the propensity for independent expression regardless of the nature of the political elite. The goal is to explain the persistence of independent expression not simply as a consequence of some assumed universal human drive for freedom, but rather in terms of specific concrete social and cultural processes.[2]

PARTY-PRESCRIBED EXPRESSION

With the Communist party's ascendency to power, public expression came to depend primarily on the support of institutions controlled by the party. The general situation has developed along the lines Lenin set forth in his

[2] I am not discounting the existence of a human drive for freedom. Such a drive may or may not be operant. However, to invoke such a drive as an explanation for these phenomena overlooks a more empirically grounded sociological explanation. Whether or not the tendency to seek cultural freedom is universal, the goal of the sociologist should be to explain the tendency as much as possible *sociologically*, i.e., in terms of observable social and cultural structures and processes. The ontological question of man's drive for freedom need not be a point of contention.

essay "Party Organization and Party Literature."[3] Lenin's essay was clearly referring specifically to public expression which comes out in the party's name, but gradually his notion of integrating all party media of public expression into party organization was transformed into the notion that all public expression must be infused with party spirit (*partinost*). Along these lines, Mikhail Sholokhov, during the height of the Stalinist period, declared: "Each of us writes according to the dictates of our hearts, but all our hearts belong to the Party and to the people whom we serve with our art" (cited in Senn 1975). Complete party control and direction of public expression became the general policy.

The policies of the Soviet Union concerning public expression have been adopted in their main outlines by other Communist nations. In some cases the Soviet system was imposed upon unwilling nations; in other cases native Communist elites willingly adopted the Soviet model (see C. Johnson 1970). Even in the People's Republic of China, with its overt antagonism to the Soviet Union, the policies concerning most forms of public expression do not greatly differ from the policies of the Soviet Union (see Laychuk 1975, pp. 66–80).

Control is instituted by lavishing rewards of prestige, economic goods, and authority on those who express party-encouraged themes through party-approved forms and by a system of direct administrative control (see Moore 1954). Creative intellectuals are mostly controlled through rewards (and during severe periods, harsh punishments), while those in the mass media and in the applied sciences are chiefly controlled by administrative means.

The system of control is hierarchical (Simmons 1961, pp. 473–74). In the upper levels of the party, general ideological lines are elaborated, for which directives are issued to appropriate governmental and party offices and published in party organs. Bureaus of censorship and review committees of scientific and cultural unions and institutes function in accordance with these directives, supporting work in progress that conforms to general directives and discouraging and ultimately censoring work that does not.[4] If some undesirable work manages to reach the public, reviewers or official commentators may denounce it, and all those responsible for its appearance may have publicly to make amends. Publications in the sciences,

[3] See, e.g., report of Soviet Cultural Ministers' pronouncement on the role of art in *Polityka*, Warsaw (December 1975).

[4] The censors' interpretations of party directions may be at variance with the party elite's interpretations. Thus, at times works with little boldness are censored inexplicably, and at other times works of extreme boldness pass the censors untouched. The latter instances, probably less frequent, occur particularly at the cultural periphery, i.e., in small theaters, poetry readings, and so forth, where the audience is not very large. For an analysis of one such peripheral cultural movement, see Goldfarb (1976*a*).

arts, and in the mass media are controlled in this fashion. The party goal in each of these fields has been the same—to shape cultural development. Certain methods and subjects have come under attack: in the sciences, most prominently genetics; in the arts, almost all 20th-century movements besides party-defined socialist realism. Autonomy of intellectual pursuits has been denied. Science and technology have been supported and glorified as vehicles to transform the backward Soviet economy rather than as enterprises with value in and of themselves (Rabinowitch 1961), and the arts and mass media have been supported and directed as vehicles for the creation of the new Soviet man (Hollander 1966).

The consequences of these policies have not been entirely negative. The policies have provided a broad range of cultural products to a greatly expanded public. Through intensive support of primary and secondary education, illiteracy has been eliminated and publics for literature, the arts, and sciences have been created. Through the generous financial support and the glorification of the arts and sciences, work in these fields has become quite attractive. Through mass distribution and publication of literary and artistic classics and through popularization of the sciences, the national and international cultures have become available to the public. And, through subsidization of the fine arts, theater, publishing houses, and so forth, high culture has been brought within the economic reach of the masses. No doubt much misinformation has been disseminated (e.g., the purported distinction between Marxist and bourgeois sciences) and substantial selections of the cultural past have been ignored (e.g., the works of Kafka became available to the Czechoslovak public only in the 1960s, and then in a limited way), but, nevertheless, the Communist "cultural revolution" has succeeded in bringing culture to the people.

Party policies concerning public expression have promoted art, science, and literature that strictly conform to party guidelines. Yet, party cultural policies foster not only party-prescribed expression but also expression that falls outside the bounds of party prescription and at times is in opposition to party goals. In the following sections, I shall consider the general social and cultural forces that tend to encourage public expressions other than party-prescribed ones and the political implications of such expressions.

CULTURAL TRADITIONS AND INDEPENDENT EXPRESSION

Frequently, in attacks on "dissent" by party dogmatists, unconventional literary, artistic, and scholarly expressions are described as being vestiges of the prerevolutionary order or under the influence of reactionary tendencies from abroad. Since according to official dogma the state of socialism exists, dissent, like crime (see Hollander 1973, p. 368), cannot be explained with reference to the structure and processes of Communist society. In-

stead, it is attributed to factors outside the Communist system. Such is the case presented in Andrei Zhdanov's address to the Moscow Writers Union on September 17, 1946, which set the stage for the brutal repression of public expression in the arts and sciences in the postwar period. Zhdanov blamed undesirable aspects of Soviet literature, its elements of irony, lyricism, individualism, and its apolitical character, on western influences. He declared: "Some of our literary people have begun to regard themselves not as teachers but as pupils of bourgeois and petit-bourgeois foreign literature. Is such kow-towing fitting for us, Soviet patriots, for us who have built the Soviet order which is a hundred times loftier and better than any bourgeois order? Is kow-towing before the narrow minded petit-bourgeois literature of the West fitting for our progressive Soviet literature, the most revolutionary literature of the world?" (quoted in Mosely 1961). Because much of the public expression in Communist societies is created with reference to cultural spheres that reach beyond the immediate contemporary social situation of Communist societies, there is some truth in such attacks. Distortion begins when party officials imply in their attacks that these cultural spheres are extrinsic to the Communist system.

A primary source of the development of independent public expression lies in the party support of prerevolutionary cultural traditions of Communist nations. The literature, theater, art, and scholarly achievements of prerevolutionary times have been lavishly supported by the Communists. The party leadership strives to demonstrate that the present system is the fulfillment of the promise of the national and international cultural heritage. Even during periods of the severest Zhdanovist policies, the support for the traditional culture of the nation has provided an alternative for creator and audience to the sterile "formula culture" developed by politicians.

During the height of Stalinization, national and international traditions of cultural expression were not only available as an alternative to party-prescribed contemporary culture but more popular than contemporary socialist realistic creations. Thus in 1952, prerevolutionary literature and theater were substantially more popular among the Russian people than party formula works (Denicke 1952).

The availability of traditional works has helped to transform contemporary creative expression. The obvious mediocrity of the bulk of socialist realism, when compared with the works of Tolstoy and Kandinsky, Mickiewicz and Wyspianski, Kafka and Mann, pushes the artist in the direction of the ideals of traditional art manifested in the models of the art works themselves and in the model of the creative process involved.

The works of the postrevolutionary artists are frequently elaborations in terms of present realities of the styles and creative impulses of the ear-

lier works. The cultural heritage respected and supported by the Communist regime serves as a basis for public expression outside the narrow bounds set by party formulas. Religious sentiment and the peasants' love of the land (*The Quiet Don*) and judgments of the fallibility of political leaders (*The First Circle*) and of the ambiguity of righteousness (*Dr. Zhivago*) are explicitly based on traditional literary culture, especially as it was developed by Tolstoy. In *Tango*, the Polish dramatist Mrozek discussed the moral dilemma of the successful revolutionaries, the same revolutionaries whose ascendancy ends Witkiewicz's interwar play, *The Shoemakers*. In student theater, the names and works of 19th-century nationalist poets and young revolutionaries are used as material for contemporary national expression. The student theater group of Szeged, Hungary, recites, sings, and dances to the poetry and name of Petöfi in an unconventional production entitled *Petöfi Rock*, which while appearing to be little more than a succession of marching-band formations, is more significantly a contemporary ritual celebration of the sacred value of national independence and a youthful search for meaning in the national culture.[5] The theater "STU" of Cracow, Poland, in a modern musical, *Polish Dreambook*, considers the very problem of "Polishness" as it has been treated in traditional Polish literature. Through the images of the great Romantic poets, this theater brings to surface the primordial national myths and imaginatively calls for a national rebirth by drawing upon national traditions but transcending them, and by accepting the Communist order but calling for an overturning of its repressive aspects (Goldfarb 1976*a*).

Traditional culture supports independent expression not simply as artifact to be studied and developed. The cultural traditions present artists in Communist nations with a model of creation that is markedly richer than the party notion of *partinost* (party spirit). Narrow formulas, artificially imposed as the basis of creation, and politically imposed component parts of creative form (e.g., the necessity to portray a "positive hero") may be overcome when the creative inspiration of the past, with all its richness, is used as an alternative to the purported inspiration of *partinost*. Notions of creative autonomy, criticism of authority, and moral independence are clearly seen to be part of the creative process in traditional culture and are chosen instead of *partinost* by contemporary creators. Creators who make such a choice often find themselves at odds with the state cultural apparatus. Their works, subjected to censorship, often do not find a public legally in their own country. And even when they do, literary

[5] I observed this play and interviewed some of its creators at the 1973 International Open Theater Festival in Wroclaw, Poland, while on an International Research and Exchanges Board Fellowship (1973–74).

works are often difficult to obtain,[6] drama is presented at the margins of the theatrical world (e.g., Polish student theater; see Goldfarb 1976*a*), and sculpture and painting often must be exhibited privately unbeknownst to the general public.[7] Nevertheless, the transmission of traditional norms of creative autonomy supports the development of independent public expression.

MARXISM AND INDEPENDENT EXPRESSION

The very cultural symbols on which Communist party rule is based—Marxist ideology, the ideals of socialism, and the promise of a Communist utopia—also support independent public expression.

Marxism is propagated to all members of Communist societies as the most advanced world view. In the schools and in youth clubs, at work and at the workers' clubs, in newspapers and on radio and television, the virtues of Marxism—its method, insights, and achievements, its promise and strengths—are loudly proclaimed. As a result, the Marxist viewpoint, its key concepts, and its utopian vision generally have become an integral part of the world view of the members of Communist societies, including not only those members who support the regime but also those who are most opposed to it (see Inkeles and Bauer 1959).

The intensive teaching and praising of Marxism clearly have been intended to instill in the "masses" a positive attitude toward the accomplishments of the party as the vanguard movement building a socialist society and working for the Communist utopia. Party propaganda at all points identifies true Marxism with the official party position; the party is always pictured as the leading agent of social change along Marxist lines. No doubt, a good number of the members of Communist societies view the party in this manner. There is, though, reason to believe that a substantial portion of the population does not view the party in this way.

Scattered available evidence suggests that the official version of the proper role of the party, the meaning of Marxism, and the nature of socialism and communism have not simply been passively received and accepted by the entire population. Central values apparently have been internalized, but the real accomplishments of the ruling elite have been critically evalu-

[6] The by now classic pattern is to publish very limited editions (e.g., 1,000 copies) practically unavailable to the general public, given the large demand for the unconventional and provocative.

[7] Until his recent exile, the sculptor Ernst Neizvestny exhibited his work in this way (see Berger 1969).

ated.[8] This pattern has been continuously evident in the cultural expression by intellectuals in Communist societies.

An important part of being an intellectual is to reflect upon the problems of the social milieu, using a specific expertise or talent and a personal world view. By dictating forms and themes and by inculcating the population with the party's views, the Communist party has attempted to direct and control such reflection upon the social milieu. While an immediate consequence of this has been the party's apparent success—creation of socialist realism in the arts and the transformation of journalism and the social sciences into fields of propaganda—simultaneously the seeds have been sown for the birth of critical expression based on party ideals. As in the case for the population in general, the central values of the party become for intellectuals a potential basis for critical judgment of the nature of social reality. Though in periods of severe repression critical judgment is closely controlled, the use of key Marxist ideals is strongly encouraged, and these ideals, because not in and of themselves supportive of party policies, may promote critical as well as supportive cultural creativity. Marxist ideology functions not only in Mannheim's sense as a symbolic system supportive of the status quo but also in the sense specified by Geertz—as a symbolic system that "makes an autonomous politics possible by providing the authoritative concepts that render it meaningful, the suasive images by means of which it can be sensibly grasped" (Geertz 1973, p. 219).

Marxism has provided for some of the most outspoken critics of the Communist status quo the framework for their appraisals. Djilas's analysis of the emergence of self-interested bureaucrats as a new ruling class is based explicitly on a Marxist orientation (Djilas 1957). Similarly, the critique of the practices of the Polish United Workers Party (the Communist party) by Jan Kuron and Karol Modzelewski is a Marxist assessment of the subordination of workers' interests to the interests of the ruling party elite (Kuron and Modzelewski 1968, pp. 15–90). Kolakowski's and Schaff's Marxist humanistic explorations are attempts to find a place for the individual in a socialist system (see Kolakowski 1968; Schaff 1965). The Medvedevs, in their anti-Stalinist writings, are involved in a similar quest (R. Medvedev 1971; Z. Medvedev 1969). Each of these writers, among them past party members, believes in the future of socialism and agrees fundamentally with the central values of the party but evaluates critically the party methods utilized to achieve these central values. While the critical appraisals are quite different in each case and even conflict, the

[8] In the aftermath of the upheavals in Poland in 1956 and during the "Prague Spring" in 1968, some objective public opinion polls and surveys were possible, and they clearly point in this direction (see Nowak 1962; Piekalkiewicz 1972, esp. tables on pp. 4–5, 79–86, 132–46, and 151–52).

basic pattern of appraisal is the same; the performance of the party is measured against its promise.

Thus we have seen that critical appraisal of the Communist status quo in terms of central values of the Communist ideology is a general pattern with particular relevance for critical expression. Now we turn to a more specific dimension of this cultural pattern: the development of critical appraisals of Communist cultural policies and performance in terms of party ideals of cultural revolution.

The cultural revolution that purportedly has taken place in Communist nations has two essential aspects: on the one hand it makes available to a broad range of the population the cultural products of the nation, while on the other it creates a distinctively socialist national culture. In regard to the first aspect, the accomplishments of the Communist cultural policies, as has been mentioned above, are quite impressive, but in regard to the second aspect, the accomplishments are quite questionable. The attempt has been made to create socialist culture by decree. A convoluted Marxist cultural theory, Zhdanovism, was developed as an ideological support for this purported socialist culture. The mediocrity of the cultural product in all fields of art and on occasion in the sciences is all too well known.

Intellectuals pressured to create such a cultural product have been critical of the quality of the product, showing how it fails to measure up to its purported attributes as the quintessence of human culture. The response has been to develop critical theories of socialist culture and to develop a self-conscious "socialist" cultural creation quite different from party-prescribed models.

In *The Necessity of Art*, first published in 1959 in East Germany, Ernst Fischer argued for a broader and more realistic Marxist approach to art, one which would acknowledge that the new socialist art will develop not through artistic dogma laid down by decree but rather "in the process of work, in the free play of movements and methods, in diversity of argument and discussion" (Fischer 1964). Lukács, in his analysis of critical socialist realism, suggests that the development of a true realistic socialist literature, as opposed to the flat creation of Stalinist art, depends on a successful characterization of "the genuine tension underlying the struggle of socialism" (Lukács 1971*a*, p. 129). Both of these cultural theorists believe that a new socialist art will be born and that the new realities of the socialist order will foster a fundamentally different kind of culture. Yet they see this as developing despite the official cultural doctrine, so that for Fischer the prime example of the new socialist artist is the Soviet sculptor, Ernst Neizvestny, who recently was forced to emigrate, and for Lukács the true socialist artist is exemplified in the works of Solzhenitsyn (Lukács 1971*b*).

The development of such Marxist theories of socialist culture is of par-

ticular significance in that they help to legitimize creative expressions other than party-prescribed ones. Under Stalin, socialist culture was simply that which the party elite (and ultimately Stalin) defined as such. But with the development of de-Stalinization and splits within the international Communist movement, the question of what is and what is not a remnant of the "cult of personality" became highly problematic. In the more congenial post-Stalinist political environment, persuasive independent Marxist theories of culture helped to undermine restrictive Stalinist cultural policies (see P. Johnson 1965). Thus, even official party dogma in the Soviet Union now admits that a variety of artistic forms are consonant with socialist realism.

Perhaps of even more significance than these general theories is that creators of culture have defended their own work by presenting it as truly socialist. In a debate with Khrushchev, Neizvestny successfully defended himself at a public art exhibition as a true Marxist artist (Berger 1969). Hejduk justifies his student theater as being socialist art, critically appraising the socialist order from the specific point of view of socialist youth (Hejduk 1974; and personal interview with Hejduk conducted by the author). Litwiniec defends as distinctly progressive all theater (including his own creations at Theater Kalambur) that openly portrays both negative and positive aspects of the social milieu, and in this fashion, he, in his capacity as the director of the International Theater Festival in Wroclaw, has supported a great diversity of theatrical expression.[9]

Particularly as voiced through the art forms of literature and theater, claims of cultural revolution and of the advanced nature of party-prescribed culture have come under critical scrutiny. Because the language of political ideology cannot be translated precisely into the language of the arts, a greater degree of latitude is possible than in more direct forms of cultural expression.[10] The literary or theatrical work may ostensibly follow party-prescribed formulas, while irony and satire may be implied through the use of artistic conventions. This has been most evident in the theater of Communist nations, prominently in Czech cabaret theater of the mid-sixties, Polish student theater in the late fifties and early seventies, and some Russian theater in the late sixties.

In light of party claims of cultural revolution, such criticisms of Communist restrictive cultural policies are parallel to the general public's distaste for aspects of the system instituted by the political elite in order to achieve generally approved central values. Because of such a parallel, these

[9] Based on interviews I conducted with Litwiniec and on a public lecture he gave at the 1974 Lublin Student Theater Festival.

[10] For general treatment, see Read (1951); for treatment specifically in terms of Communist societies, see Marcuse (1961).

criticisms have been perceived by a wide audience as having broad social significance and have played an important political role in the recent histories of those nations as forums for independent political expression (see concluding section of this paper; also see Golan 1970; Goldfarb 1976*a;* Puzyna 1974, pp. 168–73).

CULTURAL INSTITUTIONS AND PUBLIC EXPRESSION

There are two fundamental types of cultural institutions in Communist societies. Each serves as a support for independent expression. On the one hand, there are cultural institutions which are manned by professionals addressing the general public under the direct control of cultural and educational ministries. Scientific academies, most professional theaters, universities, and literary publishing houses are prominent examples of these. On the other hand, there are cultural institutions not as clearly oriented to the primary goal of the creation and dissemination of culture as such. These institutions are formally tied to some political or social group or practical activity and are not under the direct control of cultural or educational ministries. The newspapers and publishing houses of various professional organizations and of social class organizations, the research institutes of various industries, and the cultural houses of specific localities are examples of this type. The cultural life in the first type tends to be more professional and more involved in "basic" culture. The cultural life in the second type tends to be more amateurish and more involved in culture of specific interests. Yet, labeling the former professional and the latter amateur, or the former basic cultural institutions and the latter applied cultural institutions, would be a mistake. Sometimes the finest professional work is done in the second type of institution, and cultural products of general interest to the whole society are produced in this type of cultural institution. These institutions often afford the intellectual the greatest degree of creative freedom and thus have definite attractions for cultural innovators, both professional and amateur. Keeping this situation in mind, we may refer to the first type as *primary cultural institutions* because of their direct ties with central party departments and governmental cultural ministries and because theirs is the primary function of cultural production for the whole of society, and the second type *ancillary cultural institutions* because of their indirect relationship with central departments and ministries and direct dependence upon other organizations which do not have the production of culture as their primary task for economic and political support, and because theirs is the ancillary function of cultural production for specific sectors of society. In both the primary and ancillary cultural institutions, the ideologically inspired cultural directions and controls meet resistance, but the type of resistance is different and there-

fore the distinction between the two types is important for the analysis of the social supports of independent cultural expression.

In primary cultural institutions the chief function is the creation and dissemination of culture. Directives are received from higher-level cultural departments and ministries, and the main instrumental tasks of cultural institutions—creation of new art, presentation of performing arts, conducting scientific research, and dissemination of knowledge—must be performed in compliance with these directives. Yet, the association in these institutions is based on cultural activity, not compliance with political directives. These institutions, though formed or reformed to put into practice party cultural policies, serve as potential institutional bases for cultural tendencies that may undermine party cultural policies.

While some kind of cultural work must be done in compliance with party policies, the orientations involved in such work often conflict with party policies. Unless *partinost* becomes the sole basis of culture (which is probably an impossibility), norms of cultural activity reaching beyond the boundaries of party policy and the ideals fostered by central party propaganda may come into conflict with official directives.

The interaction of artists, scientists, and leaders in unions, institutes, and academies is based upon their professional competence and not politics. Here, political policies are evaluated in terms of professional criteria and may be judged as being in conflict with cultural work. Thus, party-formed and controlled writers' unions in the Soviet Union (see P. Johnson, 1965) and Poland (see Gomori 1973) have served as platforms for denouncing restrictive cultural policies. Beyond such extraordinary occurrences, the conflict between cultural work and cultural policy is an underlying aspect of life in cultural institutions. My interviews with artists, actors, writers, and social scientists and my observation of theater in Poland indicate that a common attitude of the cultural intelligentsia is to consider party cultural directives and censorship as obstacles to be circumvented, obstacles antithetical to their main creative task. Observers of culture in the Soviet Union and Eastern Europe noted such attitudes as part of institutional life of the social sciences (Mosely 1961), theater (Salisbury 1968*a*, pp. 188–200), literature (Salisbury 1968*b*, pp. 166–87), and the arts (Kramer 1968, pp. 201–19). These attitudes may be suppressed, yet in primary cultural institutions they cause an underlying tension which may potentially support expressions other than party-prescribed ones.

The situation in ancillary cultural institutions is more complicated. Support does not come directly from high-level governmental ministries and party departments of culture but rather from organizations based on locality, social class, and industry. Ancillary cultural institutions are controlled by and must serve the interest of these organizations, as the primary cultural institutions must comply with high-level cultural directives.

The result is not the complete avoidance of party control but instead a five-way crosscurrent of potentially conflicting sociocultural orientations within these institutions. These include the same conflicting orientations that exist within primary cultural institutions—that is, the conflicts between (1) the values and requisites of cultural work, (2) the values and cultural policies of the party and governmental ministries, (3) the perceptions and demands of high-level governmental and party directives concerning the function of the supporting organization or social group, (4) the orientations of those within the sponsoring organization concerning its function and its relationship with the cultural institution it supports, and (5) the orientations of those within the ancillary cultural institution concerning their special role in society. The actual working out of these potentially conflicting orientations depends on the nature of party policy, the supporting organization, and the cultural institution being supported.

Ancillary cultural institutions generally have not been studied in the West. Some consequences of organizing cultural institutions in this fashion have been noted; for example, in the Soviet Union, scientific research which is closely aligned with industrial enterprises, as in the West, is more technically oriented than in basic research institutes (Rabinowitch 1961), and theater connected with cultural houses and factories is freer to experiment (Zaitsev 1975, pp. 119–28). Yet, the analysis of the potentially conflicting sociocultural orientations outlined above has not been systematically studied. This is probably because of the poverty of data available concerning the functioning of these institutions. In another paper, I have analyzed one such cultural institution and have shown that the nature of the relationship of the supporting organization, the Polish Student Organization, with the party and with Polish student theater has provided for this theater a great degree of latitude for cultural expression (Goldfarb 1975). Yet, it must be made clear here that for this to be the case, the interaction of the five sociocultural orientations mentioned above must be favorable. Party control and direction of the supporting organization and of the ancillary cultural group must be favorable. Party control and direction of the supporting organization and of the ancillary cultural group must not be too tight, and the sociopolitical orientations of the supporting organization and ancillary cultural institution must be parallel. If this is not the case, restrictions may be even more confining. When party supervision is too tight, as it was during the Stalinist period in the Soviet Union and Eastern Europe, ancillary cultural institutions become propaganda institutions for the transmission of party values to every sector of the society. When the orientations of those in the supporting organization and the ancillary cultural group are not parallel, the cultural group may be then subjected to harsh restrictions from the supporting organization as well as from the party. An example of this is a professional Polish theat-

rical group which chose to work with the support of a factory in the provincial city of Pulawy with the hope of having more creative freedom than in Warsaw where its members had graduated from the Drama College. Instead of acquiring more freedom for artistic innovation, the sponsoring organization demanded not only compliance with party conventions but also straightforward entertainment. Nevertheless, the existence of the underlying conflicting orientations in secondary cultural institutions may potentially interact to support cultural expressions other than party-prescribed ones.

INDEPENDENT EXPRESSION AND POLITICS

The traditional analysis of cultural life in Communist societies explains cultural expression simply in terms of the changing nature of the political elite.[11] We have seen above that this approach overlooks important social and cultural processes which sustain a cultural life quite apart from the one prescribed in the party elites. In this section we will see how my approach helps explain more fully the effects of the changing political context on cultural expression. I begin this task by considering the changing fortunes of cultural expression in Poland.

The Stalinist regime in Poland fell in 1956. Since the nature of the political elite is a primary factor which determines whether independent expression reaches the public, one would expect 1956 to mark an all-important turning point for public independent expression. Indeed, the cultural life of postwar Poland can be divided into two distinct periods—the Stalinist period and the post-Stalinist period. Polish cultural life during the first period was patterned on a model of Soviet cultural life. During the second period, considerable freedoms have been granted Polish artists and intellectuals, and a distinctive Polish postwar culture has begun to flourish. The analysis in this paper can help to explain why such a culture did emerge with the weakening of the Stalinist regime and in fact contributed to its weakening. If we see the Stalinist regime's totalitarian control as the only obstacle to independent expression, we would expect such expression to have emerged only after the regime fell. Yet, the social bases for independent public expression began to develop well before the Stalinist regime fell.

In the period from 1948 to 1955, much of Polish high culture was repressed. Nevertheless, one of the first independent voices heard publicly in an officially supported forum came from young students who were the primary targets of the political propaganda of that period. They did so in student satirical cabaret theater. The first critical productions were pre-

[11] The reason for this approach is clear. Communist societies are highly politicized, and the nature of cultural life does tend to mirror the changing nature of the political elite.

sented to the public in 1954, a full two years before the Stalinist regime fell (and indeed the preparation for the presentation of the first work began during the Summer of 1953).

These early student cabarets were themselves unanticipated consequences of the party policies of promoting "socialist" culture. This is evident from examining any of the early revues of the Student Satirical Theater (Studencki Teatr Satyrikow). In its first revue (premiere, May 2, 1954), this theater, the first and most renowned of an independent student theater movement that has survived and flourished to this day, produced one skit which forthrightly criticized a grotesque academic schedule filled with unnecessary political activities. This skit was the first public questioning in theater of the overpoliticized nature of the university, and by implication of the overpoliticized nature of Polish society as a whole. In later works their satirical edge sharpened. Consistently the same technique was used—taking manifestations of party ideology and propaganda and, through ironic treatment, representing them satirically.

The Student Satirical Theater and other student theaters are ancillary cultural institutions supported by the Polish Student Organization (PSO). As the patron of student theater, the PSO has provided and continues to provide the institutional basis for the independence of student theater. The PSO was founded in 1950 as an explicitly apolitical student organization. The party created this type of organization in order to reach and control a wider range of students than they could through the extremely politicized Union of Polish Youth. This was necessary because of the students' adherence to orientations based on "prerevolutionary" cultural traditions, which were negatively disposed toward the new socialist order. The goal of the party was to involve students in a party-backed organization and thus gain their sympathies. Yet, the consequence of establishing such an organization was to provide an institutional base of support for an apolitical and even politically critical public mode of expression (see Goldfarb 1976*a*). From 1954 to 1956, when the regime was still essentially Stalinist, student theater provided a unique stage for critical expression to reach students and broader audiences (Puzyna 1974). The basis for this expression lay primarily in the unanticipated consequences of the regime's propaganda and in the institutional structure established by the regime.

Until 1956 there was a great deal of resistance to any kind of independent expression in Poland. After 1956 this resistance loosened considerably but did not disappear. The central point is that the drive to express independent ideals publicly was constantly reinforced even during the most repressive years. Thus, when intensive repression was lifted, a new generation of Polish artists and intellectuals, who had been subjected to constant propaganda during their formative years, was able to develop an important forum for critical political discussion.

The importance of the analytic scheme presented in this paper is even clearer when we try to understand how the social bases of independent expression enable such expression to reach the public even though the direction of the general political situation seems to make this unlikely (for a discussion of strategies used to achieve this, see Goldfarb 1976*b*). This indeed was the case in the early seventies in Poland. At that time the judgment of outside observers was that the situation for intellectuals and artists had improved only slightly with the major political changes. It was maintained that the new regime, headed by Edward Gierek, was able to keep independent intellectuals and artists quiet by exercising fairly repressive cultural policies while improving the economic lot of the masses of Poles. The decrease in the number of books published, plays produced, and major research projects initiated was cited as proof of this observation (see Dean 1974). It was precisely at that time, however, that student theater again produced highly critical works for the first time since the late fifties (Goldfarb 1976*a*). In order to account for this development, the social bases of independent public expression must be considered.[12]

Public expression may directly reflect the will of the party, but for this to be so, the bases of independent expression must be quite weak. The base that seems to determine most effectively the variability among Communist societies in this regard is the support of national and international traditions: the nature and strength of national and international cultural traditions vary tremendously, while the support of socialist culture and the institutional structure of culture are found in all Communist societies. In societies where no substantial national high cultural tradition exists and where ties to international cultural traditions do not now exist and have not existed historically, the party can most easily dominate public expression. This is evident in Bulgaria and Albania and in the Asian republics of the Soviet Union. In contrast, in societies where there is a strong national cultural tradition, party domination of public expression is most difficult. The generally freer cultural situation in Poland and Hungary in Eastern Europe and the Baltic republics within the Soviet Union manifests this.[13]

The preceding suggests that one very important way in which Communist parties can effectively undermine independent expression is by ma-

[12] This is done in detail in my dissertation, "The Sociological Implications of Polish Student Theater" (Goldfarb 1977).

[13] The positive relationship between tradition and cultural autonomy is not confined to Communist societies. Edward Shils has analyzed this relationship as it has been manifested in Great Britain (see Shils 1958). While Shils shows that in a Western society traditions of autonomy promote liberty, in Communist societies even authoritarian traditions can and do support independent expression in that they provide alternatives to the party conception of culture. I develop this point in terms of the Polish Catholic tradition in my dissertation (Goldfarb 1977).

nipulating the national and international traditions. The most striking case of this is in East Germany where the continuity of the national cultural tradition, far from being central to the party's cultural policies, has been explicitly denied for most of the history of the German Democratic Republic. Though other reasons surely exist for the basic uniformity of public expression in the GDR,[14] the fact that national cultural traditions have been deemphasized may have been the crucial determining factor of this uniformity. Especially suggestive in this regard is that two years after an apparent major shift from overt avoidance to cautious recognition in East Germany's orientation toward its national traditions (see *New York Times*, June 22, 1975, p. 6) a major flurry of independent public expression has become evident (see, e.g., *New York Times*, November 5, 1976, p. 1).

Before concluding, one last issue concerning the relationship between the social bases of independent expression and politics must be addressed, namely, the influence of these social bases on political change. Numerous times in Communist societies bold independent expression preceded major political events. In 1954 an independent Polish student theater movement began; in 1956 the Stalinist regime in Poland fell. In 1955 the Petöfi circle, a public forum for independent debate, was founded; in 1956 there was a major political revolution in Hungary. In 1968, a production of *Forefathers' Eve* which explicitly expressed anti-Soviet sentiment was presented; following its forced closing a major student revolt occurred. Early in 1970 two Polish student theater productions critical of Gomulka were created. In December 1970, Gomulka was forced to resign as a consequence of a major workers' revolt. In these cases unconventional political views became public as a consequence of the social and cultural processes discussed in this paper. Further, in some such cases, as with the early Polish student theater, the Petöfi circle, and the production of *Forefathers' Eve*, there is evidence indicating that broader political changes were facilitated by independent public expression (see, e.g., Golan 1970; Goldfarb 1976*a*; Puzyna 1974). This suggests that the social bases of independent expression analyzed in this paper may promote independent expression, which then becomes a basis of political action, and perhaps they encourage political action by creating zones of autonomy and orientations which promote more directly not only independent expression but also independent political action.

An analysis of this is beyond the immediate task of this paper. It suffices to conclude that if the social bases of independent expression are indeed also bases of independent political action, we have in the theoreti-

[14] Such as the self-selection process made by German intellectuals in the postwar period, and the availability of West German broadcasts in 70% of East Germany.

cal arguments presented here the beginnings of a systematic political sociology of Communist societies that goes beyond the confusions discussed in the introduction to this paper.

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A Reconsideration of the Income Differences Found between Migrants and Northern-born Blacks[1]

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Southern-born blacks living in the North differ from northern-born blacks in income, labor force participation, and occupation. These economic gaps cannot be explained by differences in age and education between the birthplace groups. Currently the two main interpretations are: (1) a highly selective migration of blacks from the South involving more than age and education and/or (2) the existence of some fundamental differences between the two birthplace groups. The evidence gathered indicates that a substantial part of the selective migration process is not really taken into account when the controls normally available for age, sex, and education are applied. Further, it indicates that return migration of southern-born blacks is both numerically sizable and largely selective of blacks who did not fare well in the North. Hence, there are important selective processes involved in determining which southern blacks remain in the North as well as in determining their initial migration from the South. Based on certain assumptions about regional differences in the opportunity structure for education, ridits are used in lieu of actual attainment to control for education's influence on North-South differences in income. When selective migration is also taken into account, the results are radically different such that the southern income advantage disappears completely. The usual procedures for dealing with educational differences can generate totally misleading results. The approach to educational attainment used here may be applied to a wide variety of empirical questions involving education and similar types of variables.

BIRTHPLACE DIFFERENCES

An interest in the differences between northern- and southern-born blacks residing in the North can be traced back to speculations about the changing nature of northern race relations occurring late in the 19th century as well as during the First World War (see, for example, Kusmer 1976; Spear 1967; Drake and Cayton 1945). Nevertheless, only within the last few years has there surfaced substantial evidence that migrant blacks living in the North differ from their northern-born counterparts on a wide variety of attributes. Migrants currently have higher incomes, lower unemployment, less frequent

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reliance on welfare, more frequent employment in blue-collar occupations, and higher proportions of both ever marrieds and currently marrieds (Bacon 1971; Masters 1972, 1975; Adams and Nestel 1973; Bowles, Bacon, and Ritchey 1973; Long 1974; Long and Heltman 1975; Lieberson and Wilkinson 1976). Some of these gaps are found even without taking into account age and educational differences between migrants and northern-born blacks.

The task of this paper is to explain the income and other work-related differences observed in the North between these two segments of the black population. At present, two important explanations for the superior performance of southern-born blacks on most "bread-and-butter" variables are (1) selective migration from the South such that those most likely to succeed have a greater frequency of migration to the North and/or (2) the existence of some fundamental personality or cultural differences on the aggregate between southern and northern blacks. As a first step, it is desirable to consider the soundness of these interpretations through an evaluation of both their logic and the empirical evidence.

Selective Migration

One of the simplest steps in explaining birthplace differences among black residents of the North is to invoke the durable principle of selective migration. Since migrants from an area almost always differ from those who remain behind, all one has to do is assume positive selectivity in the migration process of a degree sufficient to account for the birthplace gaps observed in the North. Such an explanation may be both appealing and simple, but it is also less than fully satisfactory. For if the standard controls such as age, education, and sex fail to explain completely the gaps between northern- and southern-born blacks residing in the North, then either the two birthplace groups differ on one or more additional factors (which we label X) or the gaps are simply due to sampling or enumeration errors. Invoking the notion of selective migration under such circumstances is nothing more than a shorthand way of assuming not only that southern-born migrants differ from the northern-born blacks on some unmeasured X and that X in turn fully explains away the observed differences, but also that the entire pool of southern-born blacks (including those who do not migrate) is identical with the entire pool of northern-born blacks on the same unmeasured attributes. One can properly conclude that the birthplace gaps found in the North are simply due to selective migration if, and only if, these conditions exist. At the very least one would desire some evidence or indication that indeed the migrants differ from southern nonmigrants with respect to these X attributes and, in turn, that X affects the dependent variables under study. This, then, is one of the empirical issues to be considered below.

Deterioration of Black Life in the North

The second major interpretation is that the entire black population of southern origin differs from the northern-born one on certain characteristics which affect socioeconomic achievement. "It is almost as if growing up in the big cities has in some ways come to be the handicap for blacks that birth in the rural South is often alleged to be" (Office of the President 1974, p. 98). While they do not rule out the influence of selective migration, some fundamental differences are nevertheless claimed to exist between northern and southern blacks, differences such as greater social disorganization for those raised in northern ghettos; higher socioeconomic expectations among northern-born blacks and, in turn, a greater unwillingness to accept menial jobs; disillusionment among northern-born blacks; and a greater willingness to substitute welfare for work. Perhaps the most important example of this approach is a paper published in this *Journal* by Long and Heltman (1975), but a number of the studies cited at the outset support various parts of this thesis.

The higher earnings of migrants to the North along with their lower unemployment rates, reduced reliance on welfare, and more frequent blue-collar employment all provide impressive support for the deterioration interpretation. Because the advantage of employment over unemployment is held to be least among those with little education, an especially impressive finding in support of the deterioration interpretation is that migrants exceed northern-born black incomes by the greatest absolute dollar amount in this educational category (Long and Heltman 1975, p. 1403). Long and Heltman's detailed comparisons by age and education indicate that southern migrants have higher incomes than northern-born blacks in all but three of the 15 cases (table 1, cols. 1 and 2).[2] Given this striking pattern, it is understandable how one might be tempted to speculate about some fundamental differences between southern- and northern-born blacks.

However, such speculations are of limited value when based on data obtained only from residents of the North because one cannot simply assume that the characteristics of southern-born blacks living in the North represent those found in the entire southern-born black population pool. Consider what happens when analogous comparisons are made within the South between southern-born residents and black migrants from the North. Northern-born migrants to the South earn more than southern-born residents of comparable age and educational attainment in all 15 of the comparisons. In other words, the procedure used to infer fundamental North-South differences among blacks living in the North would lead to the very opposite

[2] Migrants since 1965 are excluded by the authors under the assumption that their incomes are temporarily depressed after arrival. (An alternative interpretation will be considered later.)

conclusion if applied to blacks residing in the South. If the possible influence of selective migration was ignored, one would have some grounds for speculating that there is something about blacks growing up in the South which lowers their income potential as compared with northern-born blacks who have moved to the South. Although the median income figures do ignore birthplace differences other than with respect to the fiftieth percentile (see Lieberson 1975), they are used here to maximize comparability with the earlier study. When a measure that is sensitive to income differences along the entire range is employed, the index of net difference (ND), northern migrant incomes are still higher in every one of the 15 comparisons (table 1, col. 5).[3] (The income figures for blacks residing in the South are slightly

TABLE 1

BIRTHPLACE DIFFERENCES IN INCOME BY REGION OF CURRENT RESIDENCE, BLACK MEN, 1970

	MEDIAN INCOME ($)				
	Residence in North		Residence in South		
AGE AND EDUCATION (Years)	Born in North (1)	Born in South* (2)	Born in North (3)	Born in South (4)	ND† (5)
25–34:					
Elementary, 0–8	4,054	5,298	3,517	3,352	.042
High school, 1–3	5,142	6,074	4,518	4,439	.027
High school, 4	6,696	7,334	5,276	5,200	.020
College, 1–3	7,707	7,974	6,078	5,775	.037
College, 4+	9,458	9,751	8,360	7,297	.134
35–44:					
Elementary, 0–8	4,969	6,196	4,221	3,908	.066
High school, 1–3	6,105	7,062	5,200	4,818	.083
High school, 4	7,442	7,746	5,911	5,498	.070
College, 1–3	8,579	8,479	7,855	6,320	.192
College, 4+	11,728	11,272	12,083	8,783	.313
45–64:					
Elementary, 0–8	4,757	5,522	3,673	2,169	.075
High school, 1–3	5,967	6,707	4,885	4,548	.068
High school, 4	7,061	7,390	6,363	5,096	200
College, 1–3	8,051	8,075	7,042	5,722	.153
College, 4+	11,510	10,685	12,260	8,533	.322

SOURCE.—Cols. 1 and 2 from Long and Heltman (1975, table 4); cols. 3, 4, and 5 tabulated from U.S. Bureau of the Census (1973*f*, table 25).

* Left South before 1965.

† Index of net difference; proportion of all possible birthplace comparisons among southern residents in which northern-born blacks earn more minus those in which southern-born blacks earn more.

[3] The ND summarizes the difference between two probabilities of inequality. In this case, the probability of southern-born blacks having higher incomes than northern-born blacks is subtracted from the probability of the opposite inequality. The index ranges from 1.0 (a situation in which the lowest northern-born income exceeds the highest among southern-born blacks) to −1.0 (a situation in which the opposite extreme is observed). For further discussion, see Lieberson 1975.

different because the published census reports do not permit exclusion of northern-born migrants with less than five years' residence in the South. Their inclusion may actually understate the northern-born income advantage since recent migrants probably earn less than those of longer residence.)

In all fairness, one should note that the birthplace reversal is less clear-cut for some other economic variables. Moreover, the apparent paradox need not be attributed purely to selective migration of southern-born blacks to the North and of northern-born blacks to the South. As noted earlier, it is desirable to have direct evidence of selectivity rather than use it as a convenient and easy residual explanation of the differences found. Nevertheless, if the pattern observed in the North is taken as evidence of deterioration due to a northern upbringing, then it would be equally valid to take the birthplace gaps in the South as evidence of the opposite decline and thereby conclude that residence in the South leads to deterioration.

SOURCES OF DIFFERENCE

There is at present no full or even satisfactory explanation of the factors generating the birthplace gaps observed in the North. The existing studies are inadequate for evaluating the relative merits of the two major interpretations advanced thus far, to say nothing of the reasonable possibility that more than one factor operates. For reasons to be developed later, a definitive answer would require adequate longitudinal data on black attributes before and after their movements to the North. Moreover, the relative influence of each causal factor need not be the same for all birthplace differences. Despite these difficulties, it is possible to go further in analyzing these puzzling birthplace differences than has hitherto been accomplished with the available data.

There are at least six demographic ways by which such gaps could arise whenever a migrant group (in this case, southern-born blacks) is compared with the population native to the area of destination (in this case, northern-born blacks).[4] These are (1) enumeration errors, (2) the greater responsiveness of migration to opportunities, (3) the migration experience itself, (4) selective migration, (5) the application of pseudocontrols, and (6) differences between the groups. Because of the limitations of the available data, a multivariate approach will not be used even though it is reasonable to assume that the gaps observed in the North are probably due to more than a sole causal factor.

[4] Discrimination, legal restrictions, and other such factors are therefore excluded in this analysis of demographic sources. Differences in background attributes such as age, education, health, family size, and the like presumably are taken into account through the customary controls.

ENUMERATION ERRORS, MIGRATION RESPONSIVENESS, AND THE EXPERIENCE OF MIGRATION

All three of these factors may be discussed under the same rubric since data limitations make it impossible to provide a quantitative determination of their importance. However, it is reasonably certain that each of these factors would generate differences favoring the southern-born migrant group even if none existed between the entire southern- and northern-born black populations in the nation.

Enumeration Errors

The possibility cannot be ruled out that at least part of the reported difference between southern- and northern-born blacks is an artifact of the underenumeration of blacks living in the North. Black men are generally the most undercounted group in the United States (Parsons 1972, p. 4); nearly 20% of black males between the ages of 25 and 40 are estimated to have been missed in 1970, with a similar figure for those 20–34 in 1960 (U.S. Bureau of the Census 1974, figs. 5 and 6, p. 11). The key issue is not whether less successful blacks in the North are more likely to be underrepresented than the successful component, but whether the probability of being enumerated among successful and unsuccessful northern-born blacks is a different ratio from the analogous one for migrants to the North.

To the author's knowledge, there are no data that provide the necessary tabulations. Accordingly, all one can do is speculate. Not only is there evidence that migrants are specially likely to be missed in censuses (Parsons 1972, pp. 58, 134–36), but the characteristics more commonly found among recent migrants, for example, lodgers, persons loosely attached to households, and members of extended families, are also associated with undercount (Pritzker and Rothwell 1968, p. 64). One assumes that the combined influence of migrant status and low socioeconomic position on enumeration is interactive such that unemployed black migrants living in the North or those with low incomes are relatively more likely to be overlooked than northern-born blacks of comparable position. In this regard it is interesting to note that the greatest birthplace gaps among black men are found in the ages with the largest undercounts—the young adult ages. To be sure, it is possible that the enumeration errors generate a bias in the opposite direction by overstating northern black achievements relative to southern blacks. However, the available data on this question lead one to speculate that at least some unknown part of the total difference between the two populations living in the North is due to the relatively greater chance of overlooking less successful migrant blacks than northern-born blacks.

Responsiveness of Migration

The migration of southern blacks to the North is an imperfect but partial response to spatial differences in the economic opportunity structure available to blacks with specific skills, training, and the like. By contrast, northern-born blacks living in the North include not only segments whose members have migrated within the region but also those whose members have not. (Indeed, those migrating out of the North are not even included in such comparisons.) Accordingly, one might expect the southern-born group to be more responsive than the entire set of northern-born blacks to differences between northern communities in the opportunity structure. Under such circumstances, southern blacks in the North will enjoy a certain advantage over their northern-born compatriots.

Since those who have moved in the past are also more likely to move again (for an early study of this phenomenon, see Goldstein [1954]), southern-born blacks in the North are probably more responsive to new developments in the spatial distribution of opportunities. Directly relevant here are some calculations based on the 1970 census. Among all black males in the United States, 5% of those living in their state of birth in 1965 had changed states by 1970 whereas 13% of those who were not living in their native state in 1965 changed. This difference holds for each of 11 age categories examined (calculated from data reported in U.S. Bureau of the Census [1973*e*, p. 2]; rates are for native black males not living abroad in either period with state reported in both periods). These state-of-birth data do not distinguish southern-born blacks from other blacks or even according to region of residence. However, if the age factor is ignored, the divisional moves for the southern-born blacks living outside of the South in 1965 can be compared with non-southern blacks in the same divisions. In each of six non-South census divisions, the southern-born blacks were more likely to leave by 1970 than were non-southern blacks who were living there at the initial period (calculations based on data in U.S. Bureau of the Census [1973*e*, table 8]).

In short, the greater responsiveness of migrants to shifts in the opportunity structure means that migration by itself tends to give a certain advantage to any migrant group. How important this factor is for the observed differences in income between birthplace groups cannot be answered with the available data. But one can note that the data are at least consistent with the speculation that the southern-born blacks enjoy a certain advantage over the northern-born blacks without it necessarily being due to some fundamental differences between the two populations.

Experience of Migration

The act of migration is certain to generate changes in more than residence. These shifts may be of trivial consequence (finding a new grocery or a satis-

factory barber), or they may be of greater significance (the shift in personal or communal ties, the development of new reference groups and novel opportunities). Insofar as regional differences exist in life-styles, norms, and the like, some degree of cultural shock may also occur. Accordingly, the very experience of migration may transform or modify the migrating group such that southern-born blacks could differ from northern blacks after the move even if not before. It is not possible in this paper to pursue an answer to this difficult and long-standing empirical problem. However, it is reasonable to assume that to some unknown degree this factor generates differences between northern- and southern-born blacks residing in the North which are incorrectly attributed to the characteristics of southern-born blacks prior to rather than after their northward migration.

In short, all three of these factors tend to affect North-South birthplace comparisons. Their importance is unknown, but it would appear that their effect is to overestimate the gaps in work-related characteristics which initially existed between the two groups prior to southern-born migration to the North.

SELECTIVE MIGRATION

There are four different ways by which selective migration could generate birthplace differences among blacks living in the North even if none existed between the total populations of northern- and southern-born blacks. These are nonrandom migration of southern-born blacks to the North, nonrandom migration back to the South among those southern-born blacks living in the North, nonrandom outmigration of northern-born blacks from the North, and nonrandom return migration among the northern-born blacks living outside of the North.

It is unlikely that the last two types of selective migration were significant factors in generating the observed differences. This is because the gaps remain fairly similar when all northern- and western-born blacks living outside of the South are compared with southern-born blacks living in these areas (Lieberson and Wilkinson 1976). Further, the migration of northern-born blacks to the South is relatively unimportant by any numerical criterion (in 1970 there were fewer than 200,000 northern-born blacks of all ages living in the South). Accordingly, the nonrandom outmigration of northern-born blacks and their return probably does not greatly affect the birthplace gaps found within the North. Hence the first two types of selective migration, southern-born migration to and from the North, will be considered in great detail as possibly accounting for a sizable part of the gaps observed in the North.

Migration from the South

Obviously the migration of southern-born blacks to the North is selective in the sense that the movers and nonmovers differ from each other on more than simply the migration act itself. However, the issue is not whether such control factors as age, sex, and education fully eliminate mover-nonmover differences on attributes which affect economic performance; there is little reason to think that the migration decision is purely random among southern-born blacks of a given age, education, and sex. Rather the question is, How different are southern-born movers from nonmovers after these controls are applied? If such gaps are nearly eliminated or radically reduced by the combined influence of age, sex, and education, then there is reason to believe that migrants to the North are fairly representative of all southern-born blacks within such categories. Under that circumstance, comparisons within the North between the migrant and northern-born black populations would represent true differences between the pools of all southern- and northern-born blacks.

A suitable interpretation of birthplace differences among residents of the North first requires a comparison within the southern-born population between those who migrate to the North and those who remain in the South. Such a task is not easily accomplished with existing census reports. Aside from an item on residence five years ago, there are no published 1970 census data which permit socioeconomic comparison between the migrant and nonmigrant southern black populations prior to the exodus of the former.[5] Moreover, because of regional differences in opportunity structure, on most attributes one cannot simply use 1970 data to compare southern-born blacks in the North with those remaining in the South. For example, if the occupational prestige scores of migrants to the North are higher than those among blacks remaining in the South (after standard demographic controls are applied), one would not know if this represented unmeasured facets of selective migration or merely regional differences in opportunity structure.

Although the issue will be resolved ultimately with longitudinal data that compare nonmovers with movers before the latter leave the South, certain limited but interesting comparisons are possible with census data. For three different variables one can infer the premigration characteristics of recent southern-born outmigrants and then compare them with nonmigrants. Among older women past the childbearing ages who moved to the North since 1965, it is certain that their cumulative fertility in 1970 does not include postmigration births. Accordingly, the fertility of such women can be compared with rates for southern-born women of the same age residing in the South. Similarly, since few persons get married for the first time after certain

[5] Data are available which cross-classify 1965 with 1970 industry or occupation, but adequate cross-tabulations are not published.

ages, the nuptuality rates of older blacks who recently moved to the North can be compared with southern-born nonmigrants. One can be reasonably confident again that any differences found between the two groups, after the aforementioned controls are applied, reflect selective migration processes. Likewise, for migrants of either sex, one can determine whether they lived in a SMSA in 1965 or elsewhere in the South. Hence, the migrant and nonmigrant subgroups can be compared on this attribute as well. In each case, the degree of similarity found between nonmigrants and migrants after standard controls are applied reflects the degree to which the latter's behavior can be viewed as representative of all southern-born blacks within those specified categories.[6]

The analysis is identical for each attribute: the crude rate for recent migrants to the North (table 2, col. 1) is compared with that for the southern-born nonmigrants (col. 2). The gaps between them, shown in column 3, constitute the basic dependent variable. The two-components form of standardization proposed by Kitagawa (1955) is employed here to determine

TABLE 2

FERTILITY, NUPTUALITY, AND EARLIER RESIDENCE OF SOUTHERN-BORN BLACKS: DIFFERENCES BETWEEN RECENT OUTMIGRANTS AND THOSE REMAINING IN THE SOUTH, 1970

Characteristic	Southern born Living in South in 1965 and in: North or West, 1970 (1)	South, 1970 (2)	Migrant − Nonmigrant (3)	Difference due to: Age and Education Composition (4)	Rates (5)
Women 45 and older:					
Percentage never married	6.65	5.54	1.11	.11	1.00
Children ever born . . .	3.85	3.29	56	−.02	.58
Percentage with a nonmetropolitan residence, 1965	46.4	46.0	.4	−5.3	5.7
Women 15-44 years old:					
Percentage never married . . .	34.77	39.57	−4.80	3.84	−8.64
Children ever born. . .	1.51	2.02	−.51	−.55	.04

SOURCE.—U.S. Bureau of the Census (1973*f*, tables 23–25, 29–31).

NOTE.—Figures adjusted slightly to avoid rounding errors. Compositional factors also include sex for nonmetropolitan residence in 1965. Three separate age categories used for women 45 and older standardization (45–54, 55–64, 65+) and likewise for women 15–44 (15–24, 25–34, 35–44). Nonmetropolitan analysis is based on four age categories (25–34, 35–44, 45–64, 65+).

[6] The type of migration data found in the census has certain limitations which should be recognized. It is necessary that a mover since 1965 both remain alive and still reside in the new location at the time of the 1970 census. Nothing is recorded in the census about other moves during the interim other than the change between their initial (1965) and terminal (1970) locations. These are standard difficulties and for the most part do not deeply affect the inferences that can be made about birthplace differences. For the one exception, a novel procedure is used below to gauge the migration missed by this method.

how much of the observed gap can be accounted for by a simultaneous cross-tabulation of the age, sex, and educational differences between migrants and the stationary population (given in col. 4) and how much is due to factors above and beyond these compositional effects (reported in col. 5). If all of the observed gap was due to compositional differences between the groups, then the magnitude and direction of the composition effect (col. 4) would equal the observed gap (col. 3) and the residual effect (col. 5) would be zero. On the other hand, if column 5 is relatively large then the observed migrant-nonmigrant gap among southern blacks is not adequately explained by these factors, and, therefore, migrants to the North are not a reasonably close representation of the entire southern-born population with such characteristics. In turn, birthplace differences among northern residents would not be representative of the basic differences between all southern- and all northern-born blacks.

Among women 45 and over in 1970, there is a difference between migrants and nonmigrants of about 1.11% never married (6.65% and 5.54%, respectively, in cols. 1 and 2). This difference is small in absolute terms, but nevertheless only 10% (0.11/1.11) is due to age and educational composition, with about 90% (1.00/1.11) due to factors omitted when these standard controls are applied. Similarly, in recent years, female migrants to the North had an average of 0.56 more children than those southern-born women remaining in the South (see col. 3). The residual rate effect is many times greater than the compositional influence on women's fertility (compare cols. 5 and 4, respectively). Indeed, migrant-nonmigrant differences in age and education composition would lead one to expect slightly higher fertility among nonmigrants, to say nothing of the aforementioned difference in marital status. In order to be sure that the differences between migrant and nonmigrant women reflect conditions prior to the former's move, the analysis was restricted to older women. However, for women 15–44 years of age the control factors also fail to account fully for migrant-nonmigrant differences in nuptuality. In the case of fertility, however, the gap of 0.51 in 1970 is explained largely by the age and educational differences between migrants and nonmigrants (cf. cols. 4 and 5).

The metropolitan origins of migrants to the North provide an example of interesting crosscurrents. The percentage who lived in a nonmetropolitan area before migrating is barely higher than the figure for those who did not migrate (cols. 1 and 2). Based on the linkage of age, sex, and education with size of residence, one would expect nonmigrants to be 5.3% less metropolitan than migrants (col. 4). But this is counterbalanced by an even greater residual effect in the opposite direction (col. 5) which yields the rather minimal net difference shown in column 3. Again, however, migrants from the South differ sharply from those remaining in ways that are not measurable by age, sex, and education.

The results for fertility, nuptuality, and urban residence are not definitive; there is no assurance that these standard controls would be as ineffective for the dependent variables of greatest concern here, namely, the occupation, employment, and income of migrants prior to their exodus from the South. Nevertheless, for the variables that we can study, the fact remains that a substantial part of the nonmigrant-migrant gap remains unexplained after differences in age, education, and sex are taken into account. Accordingly, this suggests that comparisons of northern-born blacks with those southern-born blacks living in the North may be extremely unrepresentative of what the overall differences are between all southern-born and all northern-born blacks. Selective migration to the North is at best modestly taken into account with standard background controls.

Return Migration

Since return migration can be sizable even in international migration (see, for example, Peterson 1975, pp. 286–87), it is of potentially great significance here if the less successful southern-born blacks in the North have a greater probability of returning to the South. The number moving to the South was a sizable proportion of the number migrating in the opposite direction during the same time span (table 3). During the 1935–40 depression period, there were 31 nonwhite migrants to the South for every 100 who moved in the opposite direction. Between 1949 and 1950, there were actually 112 nonwhites moving from North or West to South for every 100 migrating in the opposite direction.[7] There was a sizable movement to the North in the five years preceding the 1960 and 1970 censuses, but the counterstream

TABLE 3

MOVEMENT OF ADULT NONWHITES BETWEEN SOUTH AND REMAINDER OF UNITED STATES, 1940–70

Period	Number of Migrants: North or West to South	Number of Migrants: South to North or West	Southbound Migrants per 100 Moving North or West
1935–40	24,036	78,134	31
1949–50	28,450	25,340	112
1955–60	64,834	178,419	36
1965–70*	76,475	141,766	54

SOURCE.—U.S. Bureau of the Census (1946, tables 11 and 12b; 1957, tables 6, 10, and 14; 1963, table 237; 1973a, table 274).

NOTE.—Data are based on persons at least 25 years of age at end of each specified period.

* Data are for blacks only.

[7] The one-year period covered by the 1950 census overlapped with a severe economic recession (see U.S. Bureau of the Census 1960, p. 73).

amounted to 36% and 54% in the respective periods. If anything, this is a conservative estimate of the relative importance of southern-born return migration. In the 1970 census, from which it is possible to obtain data on southern-born blacks rather than use the unspecified birthplace data available for earlier periods, the return ratio increases from 54% to 66%.[8] In short, there is every indication of a sizable counterstream of blacks migrating back to the South since at least the 1935–40 period.

The magnitude of return migration is actually understated because the census excludes persons who both leave a region and then return to it within the time span enumerated (see, e.g., Price and Sikes 1975, p. viii). The number of migrants enumerated in the yearly sample surveys conducted by the Census Bureau between 1965 and 1970 provides a sound basis for estimating the migration missed by the decennial census procedures.[9] This is because all changes of a year's length are recorded even if later canceled by a return movement (however "roundtrip" S → N → S movements of even shorter duration cannot be included with this method). The 1970 census reported only about 55% of the 876,000 black moves from the South to North between 1965 and 1970 inferred on the basis of the yearly migration survey data. Although the discrepancies may reflect poorer enumeration procedures, the gap is probably largely due to the omission of round-trip movements.[10]

Since the movement of blacks back to the South is even greater than the census data indicate, return selectivity could deeply affect the birthplace

[8] These ratios are derived from data for males in tables 23–25 and females in tables 29–31 of U.S. Bureau of the Census 1973*f*.

[9] Data were obtained for nonwhites in the first two years and blacks in the last three years from the yearly Current Population Survey (P-20): numbers 156, 171, 188, 193, and 210. There are no relevant cross-tabulations for age × sex × race. Discrepancies are due to several factors. The P-20 reports refer to those moving who were at least one year old at the start of the year whereas the census covers those at least five years of age in 1970. There is no ideal correction possible for this discrepancy in the ages covered. However, data can be obtained on persons under five who in 1970 lived in a region different from their region of birth (U.S. Bureau of the Census 1973*a*, table 271). Because there is no more detailed breakdown possible, and because persons less than one year old in March 1969 were not included in the CPS survey, this adjustment tends to overestimate the census movers. A second difficulty stems from the fact that the census data include only 1970 survivors of moves, whereas the CPS data are not as restrictive since the population was enumerated each year. In view of the relatively short span covered and the fact that movers are concentrated in ages with relatively low mortality, this should not be too great a factor. Third, since the census data cover blacks whereas several P-20 reports refer to nonwhites, the P-20 figures consequently are higher than the census figures. Further, the length of recall required in the census is much longer. Finally, the CPS data may include repeated moves between the regions by the same individual. On the other hand, a round-trip migration of less than one year's length may be missed in the CPS.

[10] A comparison is possible between the yearly survey and the migration question in the 1950 census because the latter referred to location one year earlier. The percentage changing states is identical in the two sources, but there are some differences between the two sets (see U.S. Bureau of the Census 1957, p. 4D–5).

differences observed in the North. The return migrants in 1970 are younger and better educated than those southern-born ones who remained in the North (table 4). More than half of the adult men returning to the South were 25–34 years of age in 1970 compared with one-fifth remaining in the North and West, with the gap nearly as substantial among women. Also, as a general rule the probability of leaving the North within each age category appears to increase with educational attainment (last two columns of table 4). This pattern is found in the older age categories as well and, hence, is caused by more than the beginning and end of military service or the completion of the education of young adults. (For an excellent review of recent shifts in return migration, including an analysis of migration since 1970, see Long and Hansen [1975].)

The key issue is whether a considerable amount of unmeasured selectivity remains after controls are applied for known differences between returnees and migrants staying in the North. The appropriate fertility and nuptuality comparisons here are between return migrants to the South and southern-born blacks who remain in the North.[11] Among southern-born women 45 and over in 1970, nearly 2% less of those returning since 1965 were ever married compared with those remaining outside of the South since at least 1965 (93.9% and 95.8%, respectively). Fertility among those ever married was somewhat lower for women returning to the South, 2.1095 versus 2.4591 children ever born (based on data in U.S. Bureau of the Census, 1973*f*, tables 29–31). In each case, only about 5% of the observed gap between return migrants and those remaining in the North can be explained by their age and educational differences. Hence, there is every indication that return migration is selective on far more than the age and education attributes that could normally be taken into account.

Socioeconomic Characteristics of Return Migrants

Certain inferences about the selective return migration of southern-born blacks can be developed for economic variables even though such inferences are impossible for selective outmigration from the South. Because of regional differences in opportunity structure, blacks living in the North—regardless of birthplace—had higher incomes, less absence from the labor force, and a greater frequency of white-collar jobs than did those living in the South. Accordingly, if return migrants do at least as well as those remaining in the North, one can be confident there is no negative self-selection back to the South since such a performance would be counter to the regional differences in opportunity structure. However, if those returning to the South do less

[11] The analysis of metropolitan-nonmetropolitan residence in 1965 cannot be repeated here because the necessary information appears not to have been published.

TABLE 4

DIFFERENCES BETWEEN SOUTHERN-BORN BLACK RESIDENTS OF THE NON-SOUTH IN 1965 WHO RETURN TO SOUTH BY 1970 AND THOSE REMAINING IN NORTH OR WEST

Age and Education (Years)	Males (%)				Females (%)				Proportion of Southern-born Returning to South	
	Remaining in Non-South		Returning to South		Remaining in Non-South		Returning to South			
	Age	Education within Age Group	Age	Education within Age Group	Age	Education within Age Group	Age	Education within Age Group	Males	Females
25-34	21.7	...	53.6	...	23.2	...	46.9	...	...	...
Elementary, 0-8	...	12.7	...	7.5	...	10.5	...	6.8	.065	.040
High school, 1-3	..	29.2	...	18.6	..	30.8	...	25.4	.069	.050
High school, 4	...	42.0	...	50.1	...	43.4	...	44.6	.122	.062
College, 1-3	...	11.8	...	16.3	...	11.0	..	15.7	.139	.084
College, 4+	...	4.3	...	7.5	...	4.2	...	7.5	.167	.102
35-44	23.8	...	24.0	...	23.4	...	21.9	...	...	...
Elementary, 0-8	..	29.1	..	19.7	...	21.1	...	17.3	.031	.024
High school, 1-3	...	30.8	...	19.5	...	34.8	...	29.3	.029	.024
High school, 4	..	26.3	...	38.9	...	30.4	...	33.9	.066	.032
College, 1-3	...	8.8	...	12.5	..	8.8	...	9.6	.064	.031
College, 4+	...	5.1	..	9.3	...	5.0	...	9.9	.080	.056
45-64	40.8	...	16.4	...	38.8	...	21.4	...	...	...
Elementary, 0-8	...	52.5	..	46.0	...	45.4	...	45.5	.016	.017
High school, 1-3	...	24.0	...	21.0	..	28.0	...	26.5	.016	.016
High school, 4	...	15.1	...	19.0	...	18.1	...	15.9	.023	.015
College, 1-3	...	5.2	...	7.1	...	5.4	...	7.5	.025	.024
College, 4+	.	3.2	...	6.9	...	3.1	...	4.6	.039	.025
65+	13.7	...	6.0	...	14.6	...	9.7	...	...	...
Elementary, 0-8	...	79.6	...	77.3	...	72.8	...	69.6	.020	.020
High school, 1-3	...	10.0	..	8.3	...	14.0	...	12.6	.017	.019
High school, 4	...	6.0	...	8.1	...	8.0	...	8.1	.027	.021
College, 1-3	..	2.5	...	4.7	..	3.3	...	5.9	.038	.036
College, 4+	.	1.9	..	1.6	.	1.9	...	3.8	.017	.040

SOURCE --U S Bureau of the Census (1973f, tables 23-25, 29-31)

well than persons remaining in the North of comparable age, sex, and education, at least one of two processes is probably operating. Either some type of negative return migration is operating which reflects the fact that those going back were doing less well in the North than were those who remained and/or that the returnees in some other fashion differed from those remaining in the North such that they accepted a less desirable economic situation in the South. The 1970 employment, income, and occupations of blacks returning to the South can therefore be compared meaningfully with those of southern-born blacks remaining in the North.

Those remaining in the North are more likely to be employed than are return migrants (cf. cols. 1 and 2, respectively, in table 5). The difference is sizable for all but the youngest age group. If anything, one would expect slightly higher employment among return migrants (cf. cols. 3 and 4) because they are more educated and there is a positive linkage between employment and education.[12] Since return migrants may have only recently settled in the South and not have had sufficient time to gain employment, labor force participation is illuminating since this includes those who are either employed or actively seeking work. However, return migrants are also more often absent from the labor force; the differences are small in the youngest age group (a period in which school attendance may be an impor-

TABLE 5

LABOR FORCE COMPARISONS IN 1970 BETWEEN SOUTHERN-BORN BLACK MEN REMAINING IN NORTH AND WEST SINCE 1965 AND THOSE RETURNING TO SOUTH

	Percentage Employed				Percentage Not in Labor Force			
	Actual		Expected*		Actual		Expected*	
Age	Remaining (1)	Returning (2)	Remaining (3)	Returning (4)	Remaining (5)	Returning (6)	Remaining (7)	Returning (8)
25–34	84	83	84	85	10	12	12	11
35–44	85	77	85	86	10	18	12	10
45 64	78	61	77	78	18	34	20	19
65+	22	15	N.A.	N.A.	77	83	N.A.	N.A.

SOURCE.—U.S. Bureau of the Census (1973f, tables 23–28; 1973c, table 9).

NOTE.—N.A. = not available.

* Based on the age by education distribution of southern-born black males living in the North or West in 1965 and remaining there in 1970 or returning to the South, respectively. Education distribution not available for those over 65 by migration status Cross-tabulation between labor force and age by education available only for all black males in the United States regardless of migration history. The actual and expected cannot be directly compared (indeed, the expected for the two residence subgroups may both exceed or both be below the actual figures). However, the direction and magnitude of the actual gaps between the two groups can be compared with the differences expected between return migrants and those remaining in the South.

[12] The actual figures could be either above or below the expected because it is necessary to standardize with rates for the United States. However, this does not keep us from analyzing the relative importance of the control variables among the two groups.

tant factor contributing to nonparticipation) but are sizable for the other ages (cf. cols. 6 with 5 in table 5). Particularly striking is the 16% gap among men 45–64 years of age between those returning and remaining in the North. Again, these gaps are counter to what might be expected on the basis of educational composition and its linkage to labor force rates (compare the direction and magnitude of the gaps between cols. 7 and 8). In short, there is a negative selectivity among those southern-born blacks who left the North between 1965 and 1970 with respect to labor force participation. Indeed, although the data are not given here, the returnees are actually less likely to work or seek work than southern-born blacks of the same age and education who remained in the South between 1965 and 1970.

The incomes of blacks returning from the North are also lower in all but one of 15 age- and education-specific comparisons (table 6, cols. 1 and 2). Except for college graduates in the two younger groups, the median income of return migrants is no more than about three-fourths of that attained in the North and often it is considerably lower (col. 3). The same pattern is found when ND indexes are computed; return migrants earn less than

TABLE 6

INCOME COMPARISONS IN 1970 BETWEEN SOUTHERN-BORN BLACK MEN REMAINING IN THE NORTH AND WEST SINCE 1965 AND THOSE RETURNING TO SOUTH

Age and Education (Years)	Median Income ($)		Income Ratio (Col. 2 ÷ Col. 1)	ND	Median Income ($)	
	Remaining (1)	Returning (2)	(3)	(4)	Returnees Adjusted for Southern Cost of Living (5)	Moving to the Non-South since 1965 (6)
25–34:						
Elementary, 0–8. .	5,195	3,285	.63	−.308	3,700	5,077
High school, 1–3	5,931	4,520	.76	−.262	5,092	5,438
High school, 4. . .	7,117	5,390	.76	− 265	6,072	6,109
College, 1–3.	7,814	5,878	.75	−.280	6,621	6,515
College, 4+.	9,652	8,541	.88	−.155	9,621	8,576
35–44:						
Elementary, 0–8. .	6,059	3,513	.58	−.409	3,957	4,928
High school, 1–3. .	6,920	4,465	.65	−.365	5,030	5,494
High school, 4. . . .	7,574	5,503	.73	−.296	6,199	5,737
College, 1–3.	8,381	6,377	.76	−.256	7,183	6,859
College, 4+.	11,376	12,051	1.06	.041	13,575	10,239
45–64:						
Elementary, 0–8. .	5,434	2,412	.44	−.439	2,717	3,731
High school, 1–3. .	6,582	3,689	.56	−.421	4,155	4,841
High school, 4. . . .	7,327	4,854	.66	−.307	5,468	5,502
College, 1–3.	8,031	3,841	.48	−.487	4,327	5,877
College, 4+.	13,638	9,701	.71	−.019	10,928	9,417

SOURCE.—U.S. Bureau of the Census (1973*f*, tables 23–25); U.S. Department of Labor 1969.

those remaining in the North at least 25 percentage points more often than the opposite inequality (col. 4).

These income comparisons are distorted because the cost of living is generally lower in the South than in either the North or West. With the U.S. urban cost of living index for persons with moderate living standards set at 100, the regional indexes for spring 1967 were 105 in the Northeast, 102 in the West, 100 in the North Central states, and only 91 in the South (U.S. Department of Labor 1969, p. 27). An adjusted index of 102.50672 is obtained by weighting the non-South indexes by the proportion of southern-born return migrants who lived in the respective geographic areas in 1965. This means that southern incomes should be raised by about 12.6% in order to take into account regional differences in the cost of living.[13] Except among college graduates in the 35–44 age group, southern blacks remaining in the North still have higher incomes even after the living costs of return migrants are adjusted (cf. cols. 1 and 5, respectively, in table 6).

Although lower living expenses in the South do not counterbalance the substantially higher earnings of the southern-born blacks remaining in the North, a second possibility is that the return migrants' incomes are temporarily depressed because some of them have only recently settled in the South. Indeed, some returnees are reporting incomes for the year in which left their job in the North, moved back to the South, and started to search for the best possible employment opportunity. Recognizing that the ideal data are not available, suppose we assume that the transition problem also accounts for recent southern-born migrants to the North earning less than southern-born ones with longer residence there (cf. cols. 6 and 1, respectively). If these assumptions are correct and if both groups are under approximately equal disadvantages, then a comparison between recent migrants to the North and recent return migrants to the South should indicate whether there is a negative counterstream back to the latter area.

The results are far more complicated than the comparisons previously reported. College graduates who return to the South earn more than those migrating in the opposite direction during the same period (table 6, cols. 5 and 6, respectively). This is also the case in two age groups for men with some college. (Incidentally, in several cases the higher incomes for the return migrants occur only after the cost-of-living adjustment.) Among those with a grade school or some high school education, however, the returnees still earn less even after the cost-of-living adjustment. This also holds for two age groups in comparisons between high school graduates. Hence, although

[13] Obtained by dividing the adjusted non-South index by 91. Observe that intraregional variation is not taken into account. Further, the cost-of-living index excludes rural areas, is restricted to a four-person family, and is based on moderate living standards rather than those for either lower or higher levels. Moreover, because of the nature of the adjustment process, medians must be used in the comparisons rather than the more desirable ND.

college educated returnees earn more than those who recently migrated to the North, return migration to the South is income selective in those educational categories where the greatest southern income advantage is found in the North.

The reader will recall that employed southern-born men in the North were less likely to have white-collar employment and were more often in blue-collar occupations. Long and Heltman (1975) speculate that northern-born men with equal qualifications are less willing to accept blue-collar occupations and that this leads to their higher unemployment rates (see also Adams and Nestel 1973). The census does not publish the tabulations necessary for examining occupational selectivity among southern-born blacks returning to the South, but there is an alternative interpretation of their lower frequency of white-collar employment in the North which is also consistent with the observed data.

Although white-collar opportunities for blacks were greater in the North, the southern-born blacks returning to the South between 1965 and 1970 had higher levels of white-collar employment in 1970 than those who remained in the North during that period: 29.8% versus 23.8% among those 25–34; 27.5% versus 20.1% among those 35–44; and 20.5% versus 15.9% among those 45–64 (U.S. Bureau of the Census 1973*f*, table 28). This pattern may be due to the returnees being more highly educated than those who remain in the North. Standardization is somewhat sticky, but for men 25–34 years of age one can obtain the cross-tabulations between education and occupation (U.S. Bureau of the Census 1973*b*, table 4) to determine if white-collar employment among returnees deviates from the education-specific rates among all black residents of this age residing in the South. Based on this procedure, one would have expected 24.1% of southern-born return migrants to have white-collar jobs whereas actually 29.8% are so employed.

Since black men returning to the South have an unusually strong propensity to be in white-collar pursuits, the pattern found in the North may not necessarily mean that the southern-born migrants are more willing than the northern-born blacks to settle for blue-collar jobs. Rather, those southern-born blacks qualified for such employment may return to the South where the income rewards are greater and the competition is less severe. Regional differences in both the quality of education and linguistic dialects may be added handicaps for southern-born blacks pursuing white-collar jobs in the North. Moreover, at least for the North in 1960, birthplace gaps in unemployment for different age- and education-specific combinations are not linked to the relative concentration in white-collar jobs (see Lieberson and Wilkinson 1976, pp. 221–22).

In summary, the selective return movement of southern-born blacks would generate gaps in the North even if there were no aggregate differences between all southern- and all northern-born blacks. The data for return mi-

gration are not in a form where they can be cumulated over time so as to provide a quantitative value. But the numerically important return movement described here is highly selective and therefore probably makes a substantial contribution to the birthplace gap observed in the North.

PSEUDOCONTROLS: EXPLAINING THE ANOMALOUS INFLUENCE OF EDUCATION ON INCOME

A standard procedure for dealing with birthplace differences in income would be to control or otherwise take into account educational attainment. Regardless of the specific statistical technique chosen to control for education, there are certain difficulties about the conventional treatment of this and other achievement variables in a wide variety of empirical contexts. A simple control for birthplace differences in education can generate misleading conclusions.

First, consider the enormous educational gap between southern- and northern-born blacks of a given age and sex. Somewhat less than half of the southern-born black men 45–64 years of age in 1970 had achieved at least an eighth-grade education. By comparison, 78% of the northern-born black men had at least finished eighth grade. There are still pronounced birthplace differences among young adults in 1970; just under 7% of the northern-born blacks in the 25–34-year-old age group have less than an eighth-grade education whereas the figure is more than double for southern-born blacks, 15.2%. The gaps for women in the different ages tell essentially the same story.[14] It is reasonable to assume that this pattern is largely due, directly or indirectly, to regional differences in the opportunity structure for blacks. (From the point of view of the hypotheses under consideration, this is a conservative assumption since one might otherwise postulate personality characteristics favorable to northern-born blacks.)

If there is a more limited opportunity structure for black education in the South, one may assume that the personality traits and background characteristics required for southern blacks to reach a given educational level are not the same as those for northern blacks of the same age. Under such circumstances, the income gaps observed among blacks living in the North need not reflect regional differences in work-ethic distribution or other traits which affect income but may be due to birthplace differences in the association of a given personality characteristic with educational attainment. For example, being a grade school dropout is probably more negatively selective in the North of various traits affecting the ability to generate income than it is (or was) among Southern blacks.

[14] All comparisons are derived from figures reported in U.S. Bureau of the Census 1973c table 4. Data are restricted to survivors in 1970 and are based on all southern- and all northern-born blacks regardless of current place of residence.

More generally, assume that two populations (in this case, northern- and southern-born blacks of a given age and sex) have the same distribution with respect to some characteristic, X_1 (here, work ethos or some other personality characteristics which affect earning ability). Let us assume that some other characteristic, X_2, also affects income (in this case, formal educational attainment) and, moveover, that a correlation of 1.0 exists within each population between X_1 and X_2. Only if the regression of X_1 on X_2 is identical within each population will it be proper to control for X_2 without controlling for X_1 when accounting for some dependent variable (in this case, income). If southern $b_{X_1X_2} \neq$ northern $b_{X_1X_2}$, then a different average value of X_1 is associated with a given level of X_2 in each subpopulation and X_2 cannot serve as a surrogate for X_1. Under such circumstances, controlling for X_2 does not take into account the influence of X_1 and generates a pseudocontrol. This can lead to a counterintuitive pattern in which the southern-born blacks at all educational levels have higher incomes although the birthplace groups have identical distributions on all attributes that affect income except education. This would occur if the cumulative percentage of the southern-born population in each educational category is greater than the northern-born figure in all but the highest educational category. In that case, the mean northern value of X_1 would be lower than the southern-born X_1 at each educational level—including the very highest. In short, higher southern incomes may not be due to North-South differences in various achievement factors but could merely reflect birthplace differences in the association of educational achievement with these factors.

Ridits

Before considering some of the empirical implications of this argument, ridits should be applied to the educational distributions.[15] Ridits simply transform any ordered characteristic of a population into a standard distribution based on the mean cumulative proportion occurring in the ranked categories. Suppose 10% of the population is in the lowest educational category and 15% in the next lowest. The educational ridit for those in the lowest category would be 0.05; the ridit for the next category would be 0.175 (the total proportion of the population found in all lower categories, 0.10 in this instance, plus one-half of the proportion in the category itself,

[15] Ridits were formally described 20 years ago (see Bross 1958) and have received attention in the statistical literature (see Snedecor and Cochran [1967, p. 246] and the references in Williams and Grizzle [1972, pp. 56–57]), but to my knowledge this type of transformation has been relatively neglected by sociologists. Although he did not label it as such, Duncan's effort (1975) to adjust for changes in money income over time involved a ridit transformation. There are probably other instances since ridits fit in very nicely with the relative deprivation and reference group theories.

in this case, 0.075). If 5% of the population is in the highest attainment category, then their education ridit would be 0.975 (based on the 0.95 in lower categories plus one-half of the proportion found in this class). The incomes of persons with equal ridits will then be compared even if there is a wide discrepancy in formal education; for example, if the sixtieth percentile among northern-born blacks of a given age is three years of high school whereas among southern-born blacks it is the seventh grade of elementary school.

Two technical matters should be noted. First, the educational ridits for males of a given birthplace living in a specific geographic locale are based on the attainment distribution for all men of the same birthplace and age living in the United States. This step thereby eliminates distortions that might occur because of selective migration. Second, the mean ridit within each broad census category of educational attainment is used rather than the exact educational level corresponding to some specific percentile. This is because census income data are only available for broad educational categories rather than for specific percentiles within such categories. Accordingly, the median incomes within each educational category can then be regressed on the educational ridits in order to infer the linkage between income and education for each birthplace group residing in a given area. Under the circumstances, it is not possible to use the more desirable ND measure to compare incomes. (This step will be clearer when actual data are used.)

Comparisons within the North

Let us return to the issue originally raised about northern residents (here excluding the West) and again consider for 1970 the income differences between the northern-born population (also in the narrower sense and hence excluding the western-born) and the southern-born population living in the North since at least 1965. The upper left-hand graph in figure 1, based on the median income data available for five separate educational levels within each of three different age categories, represents a least-square abstraction of the earlier results. In the youngest age group, southern-born blacks have higher incomes at all educational levels with the gaps progressively narrower at the more advanced levels of education. For the most part, this pattern is repeated for the middle and older age groups as well; the gaps get progressively smaller with increasing education such that there is actually a northern income advantage in the higher attainment levels.

The patterns are rather different when based on educational ridits (shown in the upper right-hand graph of fig. 1). The ridit comparisons appear to negate completely the income differences previously found among older blacks; the northern-born blacks actually earn somewhat more than the southern-born ones at all ridit levels in this age category. The migrant income

advantage in the intermediate age remains through only about the fiftieth ridit value, but the gaps are still rather substantial—even if reduced—among the younger adults.

There are several problems which affect our interpretation. Formal education per se must surely have some bearing on income since some specific attainment level is often a job prerequisite. Accordingly, at a given ridit level, one would expect to find higher incomes for the northern-born because
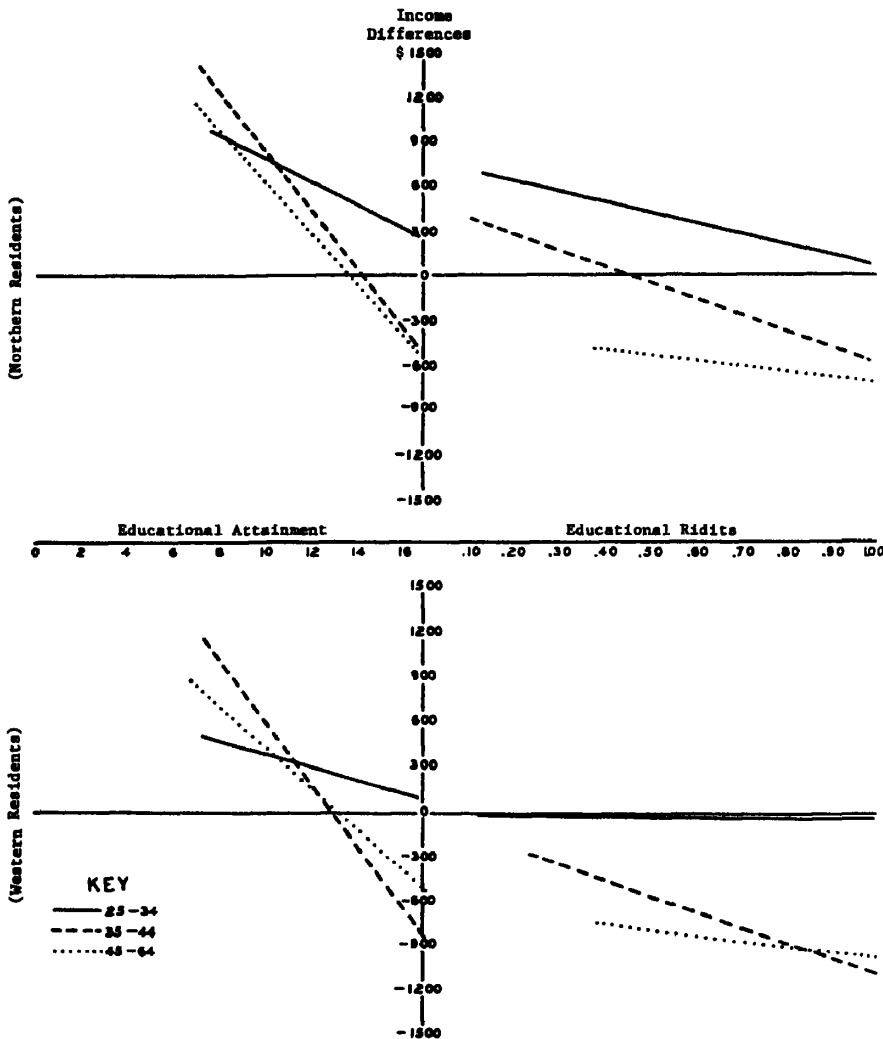
Fig. 1.—Income differences (southern- minus northern-born) by both formal education and ridits in North and West. Lines above zero indicate higher southern-born income; lines below mean higher northern-born income. Regressions and correlations available from the author. Source: U.S. Bureau of the Census (1973*f*, tables 23 and 24; 1973*c*, table 4).

they have more years of formal schooling. Another income advantage would occur if quality of education is better for the northern-born population (admittedly this is a moot issue) and has some bearing on income. Moreover, the income gaps among young adults still remain favorable to the southern-born migrants even when ridits are used. On the other hand, a bias in the opposite direction is the selective migration from the South and back again. Hence, one cannot readily determine whether the above difficulties simply reflect complications due to selective migration or if the pseudocontrol interpretation is simply inadequate.

Comparisons within the West

Although southern-born blacks in the West almost certainly differ from those living in the North and were probably selected differently than were northern-born migrants to the region, both birthplace groups residing in the West were at least subjected to selective migration processes.[16] Income comparisons in the West are probably less distorted than in the North (where only one group was affected by selective migration) and therefore provide a more desirable test of the pseudocontrol interpretation.

The northern-born blacks fare relatively better in the West than in the North when the regressions of income on formal educational attainment are compared with those of southern-born blacks (see the lower and upper left-hand panels, respectively, of fig. 1). The southern edge among young adults is considerably smaller at all schooling levels, and the northern income advantage occurs at a lower point on the educational scale among the middle and older ages. The key question, however, is whether these gaps disappear entirely, or even reverse themselves, when northern- and southern-born income comparisons within the West are made in terms of ridits.

The lower right-hand lines in figure 1 indicate that the southern income advantage disappears completely when comparisons in the West are based on educational ridits. Among the youngest set of men, the northern-born blacks actually earn a minuscule amount more than the southern-born ones; among middle- and older-aged blacks, the northern-born have a substantial income edge over southern-born ones with the same educational ridits—indeed, the gaps go up to about $1,000 for those in the higher educational percentiles. This is precisely what one might expect given the definite advantage in formal education enjoyed by northern-born blacks as well as a possible edge in the quality of their education.

These results are based on an untested empirical assumption that the

[16] Studies of northern-born blacks living in the North (narrowly defined rather than meaning "non-South") tend to overlook the possibility of unmeasured selective migration to the West and South which affects the comparisons.

regression of various personality characteristics on formal education is different (at least for older blacks) in the two regions. Under the conditions specified earlier, it is argued that the actual distribution of these personality characteristics within the entire pool of southern-born blacks could be identical with that found among northern-born blacks and yet these different income patterns would develop. A key untested assumption is that the influence of selectivity is more nearly neutralized in the West where both birthplace groups are migrants. Moreover, any transformation which depresses northern relative to southern education would have the effect of reducing the latter group's advantage net of education. Nevertheless, the approach suggested above does enable one to resolve some apparent paradoxes in the income gaps observed among blacks and clearly merits further consideration for both the problem at hand and for a variety of other research contexts in which ridits might be appropriate for dealing with the pseudocontrol problem.

COMMENT

Before attributing higher southern incomes to underlying differences between all southern- and all northern-born blacks, one must consider the selective processes associated with both inmigration and outmigration as well as the possibility that education is a pseudocontrol. There is evidence that outmigration from the South is strongly selective on factors above and beyond age, education, and sex. Indeed, the impact of selective migration is so great that one draws the opposite conclusion when income gaps are examined in the South rather than in the North. Further, even when formal education is a control variable, the birthplace gaps are smaller in the West than in the North—the former being a region in which the northern-born blacks can also be affected by the selectivity associated with outmigration. The number of southern-born blacks migrating back to the South has been substantial for several decades and is far greater than what is enumerated in the decennial census. There is evidence of severe negative selectivity among blacks with relatively little education—just the category in which the southern-born blacks in the North have the highest advantage over blacks native to the area.

A ridit transformation of formal education does help to account for the birthplace gaps found in the North and the West. This approach also helps to explain why the absolute gap between northern and southern incomes is greatest at the lowest levels of formal education, declining progressively as higher levels are compared (indeed the gaps are reversed among the most highly educated). Among those with minimal formal education, southern-born blacks experience less of a negative selection process than do blacks born in the North since opportunity for the latter was relatively more

favorable. As one moves toward higher levels of formal education, however, the opportunity structure is not that different for blacks raised in either region. Hence, with increasing levels of attainment, birthplace differences in the selectivity associated with education decline while the influence of formal schooling becomes increasingly relevant for all types of jobs.

There are a variety of assumptions in this study which themselves merit empirical study. The eventual resolution of the question will therefore require longitudinal studies of blacks before and after they migrate as well as information on the actual linkages between formal education and various personality characteristics which affect income. Very likely southern- and northern-born blacks do differ on a variety of attributes, including some under consideration here. But neither the direction nor the magnitude of these differences can be inferred from the gaps observed in the North.

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Bureaucracy and Ethnicity[1]

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A theoretical controversy has developed over the impact of social background on the attitudes, values, and behavior of public bureaucrats. Empirical analysis of the attitudes of Israeli citizens and public bureaucrats toward their national bureaucracy indicates that becoming a bureaucrat and even achieving high rank within the bureaucracy does not overcome the effect of ethnicity upon attitudes in areas of high salience to specific groups.

In recent years, the question of the extent to which bureaucratic experiences erase the impact of social identity on an individual bureaucrat's outlook and behavior has attracted increasing attention. The two polar approaches are well known. Some, like Weber ([1922] 1958, p. 228), have assumed that bureaucratic socialization processes and organizational features such as hierarchy and formalization render the individual bureaucrat "only a single cog in an ever-moving mechanism which prescribes to him an essentially fixed route of march." Consequently "the individual bureaucrat is . . . forged to the community of all the functionaries who are integrated into the mechanism." Nor can the bureaucrat "squirm out of the apparatus in which he is harnessed." Lipset (1952, p. 230), on the other hand, reached an opposing conclusion after studying how a politically conservative public bureaucracy stifled political authorities seeking rapid political change. He argued that ". . . the behavior of government bureaucrats varies with the nongovernmental social background and interest of those controlling the bureaucratic structure." Surprisingly, aside from Lipset's study, "few [empirical] efforts have been devoted to the task of determining whether in fact an individual's social identity affects his behavior as an administrator" (Dresang 1974, p. 1605). Nevertheless, several observers have concluded that "the assumption . . . that socioeconomic characteristics determine

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values for upwardly mobile, adult bureaucrats is in need of revision . . ." (Meier 1975, p. 529). The purpose of the present research note is to provide an empirical counterweight to this growing, nonempirically based line of thought.

With respect to policy, the issue involved has taken on considerable importance in the United States and other democracies. Domestically "affirmative action" raises the question whether benevolent, "reverse," or noninvidious discrimination is simply a device for achieving greater distributive justice in terms of jobs, salaries, and economic, social, and political influence or whether it is a technique which will qualitatively change the content of that influence. In other words, will black cogs simply replace white ones, or will affirmative action have ramifications in terms of organizational outcomes? This question, of course, is especially important with regard to public bureaucracies, in which proportional representation and responsiveness are often deemed highly desirable (see Krislov 1974).

Conceptually, at least four questions could be addressed in analyzing the relationship between social background on the one hand and bureaucrats' attitudes and behavior on the other. First and simplest, it is necessary to ascertain the extent to which members of different social groups are found in a bureaucratic organization. Second, assuming that various groups are at least moderately well represented in a proportional sense, differences in the attitudes of these groups could be explored. Third, if significant differences are found, it becomes crucial to determine the relationship of these differences to those found among the relevant social groups in the population as a whole. Naturally in so doing the effect of some organizational variables such as rank and seniority would be considered. Finally, if it is found that bureaucrats from a given social group reflect the values and attitudes of that group better than do other administrators, it would be desirable to determine what effect, if any, this has upon the content of decisions made by the organization.

Unfortunately the importance of the last step varies directly with the difficulty of making such an assessment. Although aggregate data could be of some utility in this regard, some access to decision-making processes is needed in order to observe directly the role of social identity in the formulation of organizational outcomes (Dresang 1974).

The following analysis is at best suggestive with regard to policy formulation. However, it does indicate that becoming and being a public bureaucrat do not remove the impact of ethnic background in areas of high salience; consequently public bureaucrats have the potential to represent the values and attitudes of the groups from which they come. How well they do so, though, depends on a host of factors, including organizational dynamics.

ANALYSIS

In view of the theoretical and practical concern with the role of bureaucratic socialization, we suggest that facets of a national bureaucracy itself can serve as referents of attitudes. If becoming and being a bureaucrat do not remove one from one's ethnic roots with regard to such attitudes, it is unlikely that this experience has a major impact on other relevant attitudes either. Furthermore, it has been suggested that this is especially true if the group or groups under analysis are proportionally underrepresented in a national bureaucracy (see Krislov 1974). Specifically, it could be expected that an ethnic group which is disadvantaged, self-conscious, and significantly underrepresented in a national bureaucracy will be more likely than others to evaluate it negatively, to have difficulty comprehending its raison d'être and operations, to expect to be treated unfairly by it, and perhaps to be less cognizant of its overall impact on their daily lives (Krislov 1967; Sjoberg, Brymer, and Farris 1966).

We undertook an analysis along these lines with special reference to Jews of Asian and African background in Israel. This ethnic group, which constitutes about half of the nation's population, has less adequate employment, housing, income, and education and is generally considered to suffer discrimination in many aspects of Israeli life. Concluding an extensive survey of the literature on patterns of stratification in Israel, Weller (1974, p. 275) states, "The principal finding that emerges is that one factor separates the population, whether it be in academic achievement, criminality, prostitution, social class, mobility, family size, intrafamily relationships, or religiosity. There is always the dichotomy of Jews of Eastern [Asian-African] descent *vs.* Jews of Western [European-American] descent . . . we are confronted with consistent findings in every area of sociological research that not only do Eastern Jews differ from Western Jews, but that the difference is virtually always to the detriment of the former." These differences are not contaminated by social class differences. In studies in which the joint effects of social class and ethnicity were considered, it was found that the latter exerts the stronger independent impact (p. 276). As might be expected in view of the nature of their disadvantage, Jews of Asian-African background are proportionally underrepresented in all but the lowest ranks of the Israeli national bureaucracy and are extremely so in its upper levels (Rosenbloom and Nachmias 1974).

The data for the study were obtained in a series of interviews with Israeli citizens and public bureaucrats lasting from July to September 1973. Citizens selected for interviews constitute a probability-stratified sample. Israel's 22 largest cities (those with populations over 15,000 in 1969) were all included in the sample and then subsampled with probabilities proportional to their size. The remainder of the Israeli population is located in smaller

cities and rural areas; these were divided into strata by regions and types of settlements. Two thousand sample addresses were selected from the 1973 electoral lists. The overall sampling fraction is 1/700 and applies to all strata and self-representing units. A total of 1,600 citizens completed the interview. Also selected with a probability-proportional-stratified sample were 850 potential interviewees holding positions in the Uniform (personnel grading) Schedule of the Israeli bureaucracy. The two strata were determined according to the size of each ministry and the proportions of civil servants in the various grades. A total of 630 civil servants completed interviews.[2]

The mean scores for citizens' attitudes toward the national bureaucracy by ethnicity are presented in table 1. It is evident that there are considerable differences of attitude between Israeli citizens of Asian-African background and others with regard to several aspects of the Israeli national bureaucracy. The former are less aware of the bureaucracy's impact on their daily lives; they find it considerably more complicated; they feel less efficacious with regard to it; they are more likely to expect unequal treatment from it; and their overall evaluation of the bureaucracy tends to be more negative. The last observation must remain tentative because of the level of significance ($P < .12$). At the same time, citizens of Asian-African origin are less likely to believe that the bureaucracy has been contributing to social integration ($F = 11.74$; $P < .05$), a finding which is congruent with their actual representation in the bureaucracy (Rosenbloom and Nachmias 1974).

The extent to which organizational recruitment, socialization, and rank remove the impact of ethnicity upon bureaucrats' attitudes is examined in tables 2 and 3. Rank is included in the analysis because of its centrality to

TABLE 1

CITIZENS' ATTITUDES TOWARD THE NATIONAL BUREAUCRACY, BY ETHNICITY*

	European-American		Asian-African			
Attitudes†	Mean	SD	Mean	SD	*F*	*P*
Impact of bureaucracy	3.9	.93	2.8	1.2	10.81	.001
Complexity of bureaucracy	2.8	1.1	3.7	1.4	8.23	.01
Promotion of social integration	3.5	1.6	2.9	2.1	11.74	.05
Bureaucratic efficacy	3.5	1.3	2.2	.97	6.71	.01
Expected treatment	3.7	2.2	2.3	1.5	5.05	.05
Overall evaluation	3.6	1.4	2.9	1.8	2.36	.12

* Because of unequal numbers of observations, statistical adjustments were made in this and the next table. For the procedure employed, see Kirk (1968, pp. 276–82).

† Measures were five-point scales. For additional information concerning scale construction and the text of questions, see Nachmias and Rosenbloom (in preparation).

[2] A detailed description of the sample procedures will appear in Nachmias and Rosenbloom (in preparation).

the nature of bureaucracy. Presumably "the deference accorded to a person who performs a hierarchical role gradually modifies his self-characterization and therefore his self-projection" (Thompson 1962, p. 73). Moreover, it has been contended that organizational socialization is likely to be more intense as a person passes through hierarchical or inclusion boundaries because organizations are most concerned about members' attitudes at the point at which more authority and importance are granted to the individual (Van Maanen 1976, p. 78).

It is apparent from table 2 that civil servants of Asian-African origin differ considerably from their counterparts in the general population in attitudes concerning the bureaucracy on all but two issues: the extent to which they believe that the bureaucracy has been promoting social integration and their expectations regarding equal treatment. Thus, as compared with citizens of Asian-African background, bureaucrats of this ethnicity attribute to the bureaucracy a greater impact on the daily life of the average Israeli ($F = 7.42$, $P < .01$); they are less likely to view the bureaucracy as complex ($F = 2.81$, $P < .05$); they also feel more efficacious with regard to it ($F = 10.14$, $P < .05$); and their overall evaluation of it is more positive ($F = 6.81$, $P < .05$). However, there are no significant differences between

TABLE 2

BUREAUCRATS' ATTITUDES TOWARD THE NATIONAL BUREAUCRACY, BY ETHNICITY AND RANK*

	Asian-African			European-American		
Attitudes	All ($N = 130$)	Low Rank ($N = 75$)	High Rank ($N = 55$)	All ($N = 419$)	Low Rank ($N = 230$)	High Rank ($N = 189$)
Impact of bureaucracy. . .	3.6	3.5	4.3	4.1	3.7	4.1
Complexity of bureaucracy	3.1	3.6	2.8	2.9	3.7	2.0
Promotion of social integration.	3.3	4.1	2.9	4.2	3.8	4.5
Bureaucratic efficacy.	3.5	3.9	3.3	4.0	4.3	3.6
Expected treatment.	2 7	2.8	2.3	3.3	3.1	3.5
Overall evaluation.	3.8	3.4	3.9	4.3	4.2	4.5

* Adjusted marginal means.

TABLE 3

SOURCES OF VARIANCES AND F RATIOS FOR BUREAUCRATS' ATTITUDES

Source	Impact	Complexity	Integration	Efficacy	Treatment	Evaluation
Ethnicity (E). . .	14.51**	9.51*	22.36**	19.88**	25.75**	31.14**
Rank (R).	4.30*	6.11*	18.92**	9.46*	6.45*	10 63**
$E \times R$.	1.5	5.26*	6.90*	2.06	8.23*	2.46

* $P < .05$.
** $P < .01$.

these bureaucrats and citizens with regard to expectation of equal treatment ($F = 1.73$, N.S). That is, both groups have a lower expectation than Israelis of European-American background of being treated equally when interacting with the bureaucracy. Moreover, citizens and bureaucrats of Asian-African origin both believe that the national bureaucracy has done too little in promoting social integration ($F = 2.05$, N.S.). Finally, the congruence between the attitudes of respondents of Asian-African background in the society and the bureaucracy's upper levels in these two areas is greater than that between citizens and such bureaucrats of European-American descent.

The effects of rank on attitudinal congruence are differential, depending on the issues involved and ethnic affiliation. Bureaucrats of European-American background attribute a greater impact to the national bureaucracy than do their counterparts of Asian-African origin ($F = 14.51$, $P < .01$). Furthermore, higher-ranking bureaucrats attribute a greater impact to the bureaucracy than do their counterparts in lower ranks ($F = 4.30$, $P < .05$). Understandably, the higher one's rank in an organization, the broader and more inclusive are one's knowledge and experiences concerning the responsibilities, activities, and influences of the organization. With respect to the perceived complexity of the national bureaucracy, both rank and ethnicity exert significant main effects ($F = 6.11$, $P < .05$; $F = 9.51$, $P < .05$, respectively). The interaction effect is also significant ($F = 5.26$, $P < .05$). That is, the effect of rank on perceived complexity is stronger among bureaucrats of European-American descent.

The main effects of ethnicity and rank are both significant with regard to the extent to which bureaucrats believe that the national bureaucracy promotes social integration. In addition, there is a significant interaction here ($F = 6.90$, $P < .05$). Higher-rank bureaucrats of Asian-African background believe that the bureaucracy is doing less for social integration than do their lower-rank counterparts. This can be partly explained by the fact that the lower-rank bureaucrats of Asian-African origin are often relatively new recruits who are aware of highly publicized efforts to increase the representation of citizens of Asian-African descent in the national bureaucracy. Also, since Asian-Africans are considerably better represented in the lower ranks, it stands to reason that they have more opportunities to interact socially with their ethnic counterparts at these levels. Indeed the informal groups in the national bureaucracy tend to be formed along ethnic lines.

The main effects of ethnicity and rank are also of considerable importance with respect to bureaucratic efficacy and the overall evaluation of the national bureaucracy. There is, however, a tendency for higher-rank public bureaucrats to believe that they themselves are less efficacious, presumably because of their more varied experiences in attempting to influence the course

of the bureaucracy's policy and activities. Finally, although both ethnicity and rank are significant in explaining attitudes concerning expectations of equal treatment, there is a significant interaction effect here ($F = 8.23$, $P < .05$). The expectation of equal treatment among higher-rank officials of Asian-African background is *lower* than that of their lower-rank counterparts. Perhaps these higher-rank officials interact more frequently with bureaucrats of other ethnic backgrounds and consequently are more apt to perceive discrimination.

CONCLUDING REMARKS

This analysis yields several conclusions. First, bureaucrats are differentiated from other citizens, whether by self-selection, recruitment, and promotional processes that reward those whose values are most consistent with those of the bureaucracy or by organizational socialization processes or by a combination of the two kinds of factors. Although our analysis does not discern the precise impact of each factor, it is evident that even low-rank bureaucrats of Asian-African origin differ in their attitudes from members of their ethnic group in the society at large and that this incongruence tends to widen as they advance up the ranks of the Israeli bureaucracy. Second, however, and politically more important, such incongruities are not substantial with regard to issues of high salience for the status of a particular disadvantaged ethnic group. Hence there is a considerable congruence among the views of Israelis of Asian-African descent concerning the expectation of (un)equal treatment and the role of the bureaucracy in promoting social integration—whether members of this group are found in the upper ranks of the bureaucracy, its lower levels, or in the society at large. Clearly then, public bureaucrats are not simply unthinking cogs: as Lipset indicated, they may react as members of a social group. For advocates of democratizing public bureaucracy, the lesson is obvious: socially representative bureaucracies have a greater potential to become politically responsive ones. In sum, our analysis lends support to both approaches mentioned in the opening paragraph. However, it indicates that the extent to which bureaucratic recruitment and socialization processes overcome the impact of ethnicity on bureaucrats' attitudes is likely to vary with the issue involved and its salience for the group in question.

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Social Origins, Education, and Fraternal Mobility[1]

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In Australia and Norway, education makes brothers alike in mobility, and groups of brothers different, to a considerably greater extent than it generates individual differences which are independent of group membership. These findings imply that education cannot be regarded as an egalitarian influence merely because it is a means to mobility. Comparison of several societies shows them to be almost alike in degree of fraternal similarity in education, although they are interpretably different in fraternal similarity in status and mobility.

In industrial societies, education is a means whereby a person can rise above his beginnings, and failure in educational attainment is a bar to later success. In the sober statistical language of mobility research, one expects to find a substantial partial correlation of son's education and son's status after controlling for father's status.

Because it is a means to mobility, and for other reasons, education is commonly viewed as an egalitarian influence in society, and of course this view is correct in many respects. But data on groups of brothers make it possible to answer a deeper question: To what degree is it family groups who benefit from the occupational opportunities afforded through education, as contrasted with individualized benefit? In the statistical language of this paper, how much of the variance of fraternal mobility is explained by education, and how does this proportion compare with the proportion of explained variance which arises from individual differences which are independent of family membership? To the extent that men benefit by virtue of family membership, the simple view of education as an egalitarian influence needs qualification with respect to mobility. To answer the question, this paper

[1] We thank the Department of Sociology of the Research School of Social Sciences, Australian National University, for allowing the analysis of data on groups of brothers from their Social Mobility in Australia Project. Paul Duncan-Jones clarified problems of using weighted data The senior author also thanks F. Lancaster Jones, professor and head of the Department of Sociology, for office space and related services provided by the department. The study was carried out while the senior author was on sabbatical leave from Boston University and was a visiting fellow at the Research School of Social Sciences at the Australian National University.

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presents findings from two industrial societies, Australia and Norway, and supplements the findings with certain comparable information from studies in three other industrial societies. Sweetser (1975) compared Norway with the other societies; the term "sibling mobility" was used in previous publications.

Basically, this paper traces influences on the variance of group and individual subscores of status. The data therefore must come from groups of brothers. However, the measurement model also applies to men without brothers, and we consider the findings to apply to all men, since in both studies the correlations of variables in a sample of men and their brothers and the correlations for all men were quite similar.

Birth into one family rather than another results in exposure to a broad range of forces, at earlier and at later periods in time. Some of these, such as socialization or genetic endowment, are familial in origin. Others are nonfamilial and stem from the location of the family in space and time, for example, the nature of the local school system, the cost of living at the time the children are ready for high school or college, or the availability of jobs when they are looking for work. To emphasize that this broad range of forces comprises more than family background in a narrow sense, we apply the term "social origins" to its totality.

A measure of fraternal similarity on some variable is a measure of the total effect of a subset of these forces which produces likeness within a family and differences between families. The composition of this cluster of forces doubtless differs for different variables and overlaps for some. Education is temporally prior to occupational status, and later we report correlation coefficients which show among other things the total effect of fraternal similarity in education on fraternal similarity in status, both before and after control of father's status. The aim of the analysis is proximal, not distal.

Fraternal similarity in education and status will be measured by an intraclass correlation coefficient. This coefficient is based on a measurement model which defines a man's score on a quantitative variable as consisting of two subscores whose variances are independent and additive (Snedecor and Cochran 1967, pp. 279–83, 294). One subscore is constant for all members of a group and varies between groups. The variance of this subscore arises from fraternal similarity, that is, from the tendency of brothers to be alike and of groups of brothers to be different. The second subscore varies between individuals within groups. The variance of this subscore is due to individual differences which are independent of group membership. The intraclass correlation coefficient gives the proportion of total variance contributed by the group subscore.

Fraternal similarity in mobility is measured by a partial intraclass correlation coefficient (Blalock 1960, p. 380). Mobility is defined here as divergence of a man's status from what it would be if it were linearly determined

by his father's status. Father's status is the same for all his sons; therefore, in our measurement model only the group subscore is influenced by father's status. The residual variance of the group subscore, after subtracting the variance explained by father's status, is variance reflecting the residual tendency of brothers to be alike in status and of groups of brothers to be different. The total variance of mobility is the sum of this residual variance and the unaffected variance of individual differences. The partial intraclass correlation coefficient gives the proportion of mobility variance which is contributed by the residual group subscore, that is, the proportion representing fraternal similarity in mobility.

In order to compare the influence of education on fraternal mobility with its influence on individual differences which are independent of family membership, it is first necessary to test whether the residual between-groups regression is significantly greater than the within-groups regression. This test justifies the separate analysis of the two lines of influence. Considering the numerous antecedents in addition to father's status which are common to the group subscores of education and status, a significant difference is highly likely. But this paper asks not merely which influence is the greater, but by how much.

The comparison requires a partial correlation of the group subscores of education and status, controlling for father's status. It also requires the average within-groups correlation of education and status, preceded by the usual test for uniformity of the within-groups regressions.

The partial correlation of group subscores can be calculated from their bivariate correlations. Despite the fact that the group subscores of education and status cannot be measured, these bivariate correlations can be found because covariance can be partitioned in the same way that variance can be partitioned (McNemar 1969, p. 414; Snedecor and Cochran 1967, pp. 282–83). To find the covariance of group subscores of education and status, the within-groups mean cross products are subtracted from the between-groups mean cross products, and the remainder is divided by the average number per group (Blalock 1960, p. 268). The same formula applies to covariances involving father's status, except that the within-groups term is zero. Then, the covariances and the square roots of the variances are used to find the bivariate correlations.

The square of the partial correlation of group subscores of education and status, controlling for father's status, gives the explained proportion of the variance of fraternal mobility (variance of the group subscores of status which is unexplained by father's status). The square of the within-groups correlation gives the explained proportion of variance arising from individual differences.

Each coefficient will be weighted by multiplying it by the proportion of mobility variance which is involved: the proportion of mobility variance

arising from fraternal mobility, or the remaining proportion arising from individual differences. This weighting expresses the magnitude of each line of influence from education to mobility in relation to the total variance of mobility. Finally, to complete the comparison, the two proportions of total mobility variance explained by education will be summed and the proportion arising from fraternal mobility explained by education will be expressed as a percentage of the sum.

In summary, this model is recursive, linear, and additive, and variances and covariances are partitioned into components. Using I to stand for an intraclass correlation coefficient, F to stand for father's status, Yg and Eg to stand for the group subscores of status (Y) and education (E), and y and e to stand for their individual subscores,

$$r_{YE} = r_{YgEg} \sqrt{IY} \sqrt{IE} + r_{ye} \sqrt{(1 - IY)} \sqrt{(1 - IE)} .$$

Also, $r_{YF} = r_{YgF} \sqrt{IY}$ and $r_{EF} = r_{EgF} \sqrt{IE}$. Of course, the correlations of Yg, Eg, and F can be stated in terms of standardized partial regression coefficients. Finally,

$$r_{YY} = 1 = IY[R^2_{Yg \cdot EgF} + (1 - R^2_{Yg \cdot EgF})] + (1 - IY)[r^2_{ye} + (1 - r^2_{ye})] .$$

Since $IY(R^2_{Yg \cdot EgF}) = r^2_{YF} + r^2_{YgEg \cdot F}(IY - r^2_{YF})$, the total variance of mobility is:

$$1 - r^2_{YF} = (IY - r^2_{YF})[r^2_{YgEg \cdot F} + (1 - r^2_{YgEg \cdot F})] + (1 - IY)[r^2_{ye} + (1 - r^2_{ye})] .$$

Dividing through by $(1 - r^2_{YF})$ gives the following formulas: $(IY - r^2_{YF})/(1 - r^2_{YF})$ is the partial intraclass correlation coefficient which measures fraternal similarity in mobility, $(1 - IY)/(1 - r^2_{YF})$ is the remainder of mobility variance and consists of variance arising from individual differences, $r^2_{YgEg \cdot F}(IY - r^2_{YF})/(1 - r^2_{YF})$ is the proportion of mobility variance which consists of fraternal mobility explained by education, and $r^2_{ye}(1 - IY)/(1 - r^2_{YF})$ is the proportion of mobility variance which consists of individual differences explained by education.

The Australian data were provided by the Social Mobility in Australia Project of the Department of Sociology of the Research School of Social Sciences at the Australian National University, described in Jones, McDonnell, and Williams (1977) and in Broom et al. (1977). The project's 1973 survey obtained information from about 5,000 persons aged 30 to 69 in a probability sample covering all parts of the country except the Northern Territory. About two thirds of the sample were men, and of these, 295 had at least two sons in the labor force. These 295 male respondents provided data on 681 sons.

Occupational status was measured by the "ANU II" scale, which was constructed by regressing prestige scores of occupations on census data about

the social characteristics of individuals in occupations and then using the resulting equation to estimate prestige scores for all occupational groups covered by the census. In the Australian data, father's occupational status refers to the status of the job held by the male respondent 30 years after his first job. The interview inquired about the first job and about jobs held at 10-year intervals after the first job. The 30-year point was chosen as most comparable with the father's job as defined in the Norwegian survey. In that survey, father's occupation was that on which he had spent most of his working life. Son's occupation in the Australian data means the current (1973) occupation as reported by his father, the respondent. Education means years of full-time school completed.

The Norwegian survey was designed and supervised by the senior author and was supported by the Institute of Applied Social Research in Oslo (Sweetser 1973). Data were collected in 1970. A probability sample of 500 Norwegians, aged over 19, was interviewed. The sample was drawn from residents of areas defined as urban by the Norwegian census. This definition includes quite small settlements as well as larger towns. At the time of the survey about 45% of the population was living in urban areas.

Respondents were asked about the education and occupation of siblings, as well as about their own and their father's education and occupation. Education means years of school completed. Occupation referred to that which the person had held for most of his working life. Occupational status was measured by a status scale developed by applying regression and factor analytic procedures to 1960 census data on the power and rewards of occupations. Male respondents with at least one brother and female respondents with at least two brothers provided the data on groups of sons. There were 219 such groups containing 619 men.

Findings from studies in three other societies will be cited for comparison with the Australian and Norwegian data. The first of these was made by the senior author in Helsinki, Finland. A probability sample of Helsinki residents was interviewed in 1965 for a study of kin relations (Sweetser 1968). In the survey a person's occupation means the one on which he had spent most of his working life. Occupational status was scored on Rauhala's occupational prestige scale (Rauhala 1966). The data on fraternal similarity came from 182 groups of brothers containing 528 men.

Müller (1972) surveyed all of the 33-year-old male inhabitants of Konstanz, West Germany ($N = 398$). The status of son's current occupation was measured by an occupational prestige scale. Son's education was defined as years of formal education plus years of adult education. The education and occupation of all sons from families with two or more sons were ascertained.

The degree of fraternal similarity in education in the United States is taken from Duncan, Featherman, and Duncan (1972, p. 263), who report

the correlation of respondent's years of school completed with oldest brother's years of school completed for native, nonblack males surveyed in a 1962 study of occupational mobility in the United States. The correlation of the same variable in pairs of brothers approximates an intraclass correlation coefficient calculated for all brothers (Snedecor and Cochran 1967, p. 295). The degree of fraternal similarity in status and in mobility is based on a correlation of occupational status of brothers which was reported by Jencks et al. (1972, p. 329).

A surprising uniformity across societies was exhibited by the measure of fraternal similarity in education. The proportion of variance of education attributable to the effect of social origins was approximately .60 in all four studies. Specifically, the measure was .58 for Australia, .60 for Norway, .58 in Müller's study in Konstanz, West Germany, and .57 in the United States. In addition, a notable degree of stability in the coefficient over time in the United States has been reported by Hauser and Featherman (1976). They give the correlation of respondent's and oldest brother's education for nine five-year cohorts of men included in a 1973 survey of the U.S. labor force. The degree of fraternal similarity in education among nine cohorts spanning nearly half a century ranged from .52 to .59 over the period, with six of the nine coefficients falling into the range from .57 to .59.

Such uniformity across studies in four different societies, and over time in one society, calls for explanation. The uniformity could arise either from the accidental agreement of different forces at work in each society or from similar forces at work in each. Taking the latter of the two conjectures as the more plausible one, the question arises as to what these forces may be. One may have to do with the nature of the educational system in industrial societies. Another may pertain to psychological factors.

All of the societies have compulsory education for a certain period, followed by additional stages such as degree programs. Sons of one family may drop out at one stage and sons of another family at another stage. Also, in these societies there may be about the same degree of fraternal similarity in capacities and taste for educational attainment.

The five studies exhibit an interesting pattern of differences in fraternal similarity in status. Listed in order for Konstanz, West Germany; Norway; Australia; Helsinki, Finland; and the United States, the measures of fraternal similarity in status are: .59, .44, .37, .33, and .29. This pattern of differences is in agreement with the conceptualization of the measure as reflecting the influence of social origins on status. Impressionistic judgment of these societies suggests that the West German occupational status system is the least open and the U.S. system the most open, with the other societies intermediate in this regard.

The rank order of the studies on the measure of fraternal similarity in mobility is virtually identical with their rank order on fraternal similarity

in status. Taking the studies in the same order as they were listed previously, the measures of fraternal similarity in mobility are: .45, .30, .30, .27, and .15. This agreement in rank order supports the interpretation of fraternal similarity as reflecting the influence of social origins, in other words, the influence of a broad range of forces. Since the order remains the same after controlling for father's status, the measure is not merely picking up the degree of influence of father's status. For information, the correlations of son's with father's status, taking the societies in the same order, are: .51, .45, .32, .29, and .40.

We now turn to the central question of this paper: the degree to which the contribution of education to mobility consists of its contribution to fraternal mobility. Table 1 contains the basic correlational information for Australia and Norway and the required analysis.

As can be seen in table 1, in Australia the variance of fraternal mobility which is explained by education constitutes 58% of the sum of the two proportions of variance explained. This is almost the same as the figure for Norway, which was 57%. In both societies, therefore, it is by no narrow margin that the contribution of education to mobility lies more in generating differences between groups of brothers, and likeness within groups of brothers, than in generating individual differences which are independent of group membership.

The close agreement of the findings on education and fraternal mobility for these two societies is interesting and calls for explanation. The four soci-

TABLE 1

COEFFICIENTS AND ANALYSIS FOR AUSTRALIA AND NORWAY

	Australia	Norway
Coefficients:		
1. $\sqrt{IY}$	.611	.665
2. $\sqrt{IE}$	.762	.777
3. r_{YE}	.537	.596
4. $r_{Y_gE_g}$	.745	.800
5. r_{ye}	.372	.390
6. r_{YF}	.318	.452
7. r_{EF}	.339	.438
8. r_{Y_gF}	.521	.680
9. r_{E_gF}	.446	.564
10. $r_{Y_gE_g \cdot F}$*	.671	.688
11. $IY - r^2_{YF}$	.272	.238
12. $1 - IY$	.627	.558
13. $1 - r^2_{YF}$	.899	.796
Analysis:		
14. Fraternal mobility variance explained by education as a proportion of total mobility variance: $(10)^2(11)/(13)$	.136	.142
15. Individual differences explained by education as a proportion of total mobility variance: $(5)^2(12)/(13)$	.097	.107
16. Fraternal mobility variance explained by education as a percentage of the sum of the proportions explained: $\{(14)/[(14)+(15)]\}(100)$	58	57

* Calculated from lines 4, 8, and 9.

eties compared had about the same degree of fraternal similarity in education. They differed in the degree of fraternal similarity in status and mobility, with Australia and Norway resembling each other and differing from the United States and West Germany.

Would we find that all societies of the type studied here are alike in the degree to which education generates fraternal similarity in mobility, just as they were shown to be alike in the degree of fraternal similarity in education? Or would we find that societies differ in the degree to which education generates fraternal mobility, just as they differ in the degree of fraternal similarity in status and mobility? The question can be answered only by investigating the relation of education to mobility in societies with differing degrees of fraternal similarity in status and mobility.

However, it can be said that in these two societies, at least, education tends to make brothers alike in mobility, and groups of brothers different, to a considerably greater extent than it generates individual differences. This is possibly a benign, and doubtless an unalterable, social fact. It does, however, qualify to an appreciable degree the idea that education is an egalitarian influence in society because it is a means to mobility.

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Commentary and Debate

The comments printed in this section are limited to brief critiques of articles and reviews published in the *AJS*. These comments are expected to address specific errors or flaws. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* does not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted themselves as articles.

REPLY TO PARSONS

I am grateful to Professor Parsons (*AJS* 83 [September 1977]: 335–39) for his comment on my recent paper (ibid., pp. 320–34), because it indicates to me that I should have been more explicit on a number of points.

Parsons is correct in his identification of the general problem which I addressed, that of the relationship between a scientific theory and a part of concrete reality about which it is intended to inform. Specifically what is at stake in my paper is Parsons's suggested solution to this problem.

As I see it, a theory is developed to supply the nomological hypotheses needed for scientific explanations. What is to be explained are certain observable variations of empirical phenomena. Since explanations involve some type of language, the variations must be conceptualized; that is, they must be described as certain kinds of alteration undergone, or kinds of behavior exhibited, by certain kinds of things. Thus, although the variations in question can be observed only with regard to concrete objects and events, a scientific theory is not concerned with the total concreteness of these objects and events. Instead, it is "abstract": it always deals with conceptualized reality, with kinds of variation of kinds of things, of which concrete phenomena are treated simply as instances. The subject matter of a theory, then, is circumscribed by the kinds of variation to whose explanation it is claimed to contribute the nomological element, that is, by the kinds of question which it is intended and declared fit to answer.

If the raison d'être of a theory rests in its function for scientific explanations, then it makes sense, in the case of a particular theory, to ask what particular questions it is intended to help answer. With regard to a sociological theory, this means looking at sociological questions. Because of the somewhat inchoate state of sociology and in order not to get embroiled in all sorts of disputes, I did not give in my paper a general characterization of the type(s) of question asked by sociologists. Instead, I selected one question which I considered to be rather uncontroversially sociological, namely, the question of order in society.

I concluded that Parsonian sociology is inadequate for an explanation of this (type of) question. At the same time I indicated that Parsons, while generally considering it an interesting and worthwhile question, has argued that it is not of the type which could be satisfactorily answered with the help of sociological theory alone. In other words, Parsons's position is that, from a theoretical point of view, it is not a properly sociological question. This argument of course presupposes an idea of what sociological theory is or ought to be and therewith an idea of what sociological questions are or ought to be.

According to Parsons, the subject matter of sociology is the ways in which actions are interrelated. However, this statement must be immediately qualified as follows: in his eyes, (1) the interrelations of actions are of sociological interest only as far as their normative dimensions are concerned; (2) these interrelations are of sociological concern only to the extent to which they are related to the assumed human tendency to form moral communities. This conception of Parsons's is not at all descriptive of the referent of the collection of ideas and questions which has hitherto sailed under the flag of sociology and sociological theory, even if only the mainstream is taken into account. Parsons's conception is clearly prescriptive or legislative, and thus one may justifiably inquire into his reasons for recommending this restricted conception. This is what I did in my paper, and I found the reasons unconvincing. My arguments in this respect were (1) that Parsons's attempted delineation of sociology is derived mostly from ontological considerations, namely, the alleged multispherical nature of social reality, and (2) that the falsifiability of his proposed analytical sociology is tied to the existence of the very thing it is intended to circumvent, namely, an encyclopedic social science.

My objection to the multispherical view of social reality was not directed primarily against its metaphysical character. What I had in mind, rather, was its barrenness as a regulative principle of theory construction. Nor did I intend to argue that it is impossible or a priori illegitimate to restrict one's attention to questions concerning the relationship between a (possible) human disposition to form moral communities and the normative integration of social activities. Yet I have no use for the kind of meta-

physical dignity with which Parsons tries to endow his restrictions. I believe the only considerations which are fruitful in this context and therefore worthwhile are pragmatic ones. Parsons's ontological argument has to my mind the undesirable consequence of blocking the natural flow of questions by restricting by fiat which ones may be addressed by sociological theory. His indictment as empiricist of any sociological theory which is more comprehensive than his own analytic brand serves no purpose other than that of clearing the turf of competitors. The beauty of this procedure, of course, is that questions of explanatory superiority never arise; unfortunately explanatory superiority is of crucial importance to empirical science.

In his reply, Parsons says that the issue between us is clear. "Burger is of course right that sociology . . . has long been caught in the dilemma of whether to define its subject matter in analytical terms or in concretely empirical terms. . . . As *theorist* I have chosen the analytic path . . ." (pp. 335-36). To my mind, this statement shows that the issue between us is not quite so clear as Parsons thinks it is. I believe that the dichotomy set up by him does not pinpoint the nature of the problem. As I argued in my paper, the issue "analytic sociology versus encyclopedic sociology" is inadequately conceptualized in terms of the dichotomy "abstract (analytical) science versus empirical concreteness." More properly, it should be viewed as the question of the kinds of variables to be included in sociological theory. The choice, in short, is not between "abstract" and "concrete" but between a more restricted set of variables and a more inclusive one.

Since I reject the dichotomy constructed by Parsons, I must also reject his attempt to impale me on one horn of the dilemma he postulates. The programmatic ideas actually implied in my paper are the following. (1) The nomological element of explanations in social sciences such as economics, anthropology, political science, and sociology is provided by a social psychological theory. (2) The models constructed by sociologists—their descriptions of hypothetical structures—should include (at least) economic, political, and cultural as well as "properly" sociological elements. Of course, these models may represent a variety of phenomena such as modernization, bureaucracy, capitalism, social control mechanisms, etc. I fail to see why this is any more eclectic than Parsons's procedure. Admittedly, however, it may be too ambitious. In any case, it has nothing to do with a hankering for the totality of concreteness. It is a conception of a science no less abstract than Parsons's own.

Once it is realized that even an "encyclopedic" sociology is abstract, the dichotomy of "analytical" versus "concretely empirical" terms can no longer serve to legitimize the specifics of Parsons's view. As long as any theory or model which is more comprehensive than his can be equated with the historicist's alleged concretely empirical approach, by elimination analytic theory appears to be the only viable alternative. With the recognition

of the fallaciousness of this strategy arises the need for justifying the specifics of the analytic alternative as compared with other a priori equally viable alternatives. Here it is interesting to quote Parsons's characterization of the more comprehensive approach advocated by me. "The alternative, which Burger chooses, is to treat the empirical problem area as the main determinant of the structure of theoretical schemata . . ." (p. 336). In the context of our discussion, the implication seems to be that it is not the empirical problem area which determines the variables included in Parsons's schema. What then determines this selection? Parsons does not give any explicit answers. In my paper I articulated my impression that his selection is not pragmatic but based on a peculiar metaphysics. As I already stated, the sole function of this metaphysics is dogmatic; it is to lay a monopolistic claim to a piece of the action, so to speak, by giving, with seeming modesty, each social science a restricted area where it reigns supreme. In my opinion, absolutely nothing is gained by thus endowing boundary disputes with an ontological dimension.

Parsons begins by acknowledging that my paper provides some specific issues on which to bite. However, the curious feature of his comment is his avoidance of a discussion of specific issues. The strategy followed is to show that I am wrongheaded in my idea of science, especially sociology. This may be the case, of course, but it would not imply that Parsons is right in his particular views. I presented a variety of quite specific arguments which do not necessarily stand and fall with my peculiar notions about science. These arguments receive rather short shrift in Parsons's comment; they are simply depicted as standing outside a long and therefore presumably correct tradition: Whitehead, Weber, Pareto, Henderson, Schumpeter, the long line of economic and sociological theory, etc. To all this I can only say that my arguments also build on a long and illustrious tradition. Yet such discourse is unilluminating: the focus should be on arguments, not authorities. Therefore I should like to indicate that Parsons has contributed a new element to the tradition to which he sees himself an heir. Whereas all previous representatives of this tradition were bent on developing an empirical science and would have been disturbed by a lack of falsifiability, Parsons has avowedly given up on this pursuit and has canonized the lack of testability—that is, unless one can believe that from the somehow accomplished combination of four or five unfalsifiable analytic sciences in a particular explanation empirical science somehow emerges.

By way of concluding I should like to say that in my paper I attempted to do precisely what Parsons considers the most fruitful thing to do: to transcend the dilemma which *he* has posed. Unfortunately his comment deals with my paper on the basis of premises involving the postulation of this very dilemma, and therefore he misses much of what I tried to say.

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POSSIBLE EFFECTS OF USING ALCOHOL TO CONTROL DISTRESS: A REANALYSIS OF PEARLIN AND RADABAUGH'S DATA[1]

Pursuit of etiological factors to account for deviant behavior within the framework of social epidemiology has long been a preoccupation of sociologists of deviance. As a subject for such study, the deviant use of alcoholic beverages has, for several reasons, attracted less attention from social scientists than types of deviance which are less prevalent and less socially costly (Straus 1976*b*). Thus, the attention given by Pearlin and Radabaugh (*AJS* 82 [November 1976]: 652–63) to factors associated with a particular type of drinking behavior is a relatively rare event in a major sociological journal.

Unfortunately, the analysis and interpretation undertaken by these authors are bound within an etiological paradigm that is not the only formulation supported by their data. Pearlin and Radabaugh utilize cross-sectional data to posit an etiological chain in which it is contended that those facing economic strain are more likely to experience intense anxiety. They then argue that intense anxiety predisposes those who suffer from it to utilize alcohol for distress control. Finally, they introduce personality measures to indicate that those with low self-esteem and those with weak senses of mastery over their environment are most likely to use alcohol for distress control under conditions of intense anxiety.

While the implication of the authors' interpretations is that they are examining cross-tabular relationships between these behavioral and personality variables within the portion of the sample experiencing economic hardship, an examination of their data indicates that these statistical relationships are, in fact, calculated from data for that portion of the entire sample for which complete information on these variables was available. While the authors reintroduce the economic hardship variable in reporting a multiple correlation at the close of the article, no data are provided on the construction of this regression model although such data are of substantial importance in assessing the strength of their final theoretical formulation.

Aside from this rather opaque methodological strategy, the authors unquestioningly accept their scales of self-esteem and mastery as measurements of personality constructs. Fortunately, the presentation of their data allows for reanalysis within a different paradigm in which these questionable constructs are considered as dependent variables.

Research on and clinical experience with alcohol abuse clearly allow the

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consideration of deviant alcohol use as an independent variable leading to changes in coping strategies, social relationships, and social status. For example, one of the repeated conclusions in the corpus of work entailing a search for the "alcoholic personality" is that longitudinal data are essential to untangle the psychological antecedents and consequences of deviant drinking (Williams 1976). Furthermore, research on and clinical experience with alcohol problems have led to widespread acceptance of the "progression" associated with chronic excessive drinking; patterns of deviant use of alcohol over time are conceptualized in "stages" of increasing dependence on alcohol associated with deterioration in social, psychological, and physical functioning (Jellinek 1960; Trice 1966; Strauss 1976*a*; Davies 1976). Thus, within the tradition of alcohol studies, the paradigm of deviant alcohol use as an independent variable is prominent.

The type of alcohol use measured by Pearlin and Radabaugh—drinking to control distress—is a socially deviant act, despite their rather benign conceptualization of this behavior as a coping strategy. To the extent that American drinking norms can be identified, acceptable drinking is that done in a social setting to promote conviviality and social solidarity (Bacon 1976; Straus 1976*a*). The use of alcohol for its drug effects, while probably at least as prevalent as indicated by Pearlin and Radabaugh's data, is regarded as symptomatic of psychological dependence on alcohol (Straus 1976*a*), which in turn may be defined as symptomatic of alcoholism (National Council on Alcoholism 1972).

Given the deviant nature of this behavior and the implication of its actual existence through its admission in interviews by a subset of Pearlin and Radabaugh's respondents, it is most likely that these respondents have experienced adverse self-reactions and social reactions to the manner in which they use alcohol. These reactions, in turn, may escalate anxiety and lower self-esteem. It is further reasonable to surmise that this repeated pattern of behavior would generate reactions which, in turn, would produce a reduced sense of mastery over one's environment.

To test this general hypothesis about the deleterious effects of the use of alcohol to control distress in terms of subjective consequences, the data presented by Pearlin and Radabaugh were reanalyzed, with predisposition to utilize alcohol for distress control considered as the independent variable. The authors presented in their article demonstration of the significant relationship between this predisposition and anxiety level. Consideration of its relationship to self-esteem (gamma = .35) and mastery (gamma = .25) also revealed significant relationships in the reanalysis.

Utilizing the multivariate strategy employed by Pearlin and Radabaugh, table 1 indicates the effects on sense of mastery of predisposition to use alcohol for distress control, with anxiety level as the moderating variable. Under all three conditions of predisposition to use alcohol in this fashion,

TABLE 1

PREDISPOSITION TO USE ALCOHOL FOR DISTRESS CONTROL, ANXIETY, AND MASTERY (%)

Mastery	Strong Predisposition			Weak Predisposition			Minimal Predisposition		
	Intense Anxiety	Moderate Anxiety	Low Anxiety	Intense Anxiety	Moderate Anxiety	Low Anxiety	Intense Anxiety	Moderate Anxiety	Low Anxiety
Limited	49 (37)	23 (15)	20 (21)	16 (10)	12 (9)	12 (16)	24 (48)	13 (43)	12 (74)
Moderate	38 (29)	56 (37)	41 (43)	63 (39)	53 (41)	54 (75)	51 (101)	46 (151)	38 (239)
Great	13 (10)	21 (14)	39 (40)	21 (13)	35 (28)	34 (48)	25 (49)	41 (134)	50 (310)
Total	76	66	104	62	78	139	198	328	623
	$\chi^2=28.00$; 4 df; $P<.001$ $\gamma=0.41$			$\chi^2=4.62$; 4 df; N.S. $\gamma=0.14$			$\chi^2=45.65$; 4 df; $P<.001$ $\gamma=0.27$		

NOTE.—N's in parentheses.

TABLE 2

PREDISPOSITION TO USE ALCOHOL FOR DISTRESS CONTROL, ANXIETY, AND SELF-ESTEEM (%)

Self-Esteem	Strong Predisposition			Weak Predisposition			Minimal Predisposition		
	Intense Anxiety	Moderate Anxiety	Low Anxiety	Intense Anxiety	Moderate Anxiety	Low Anxiety	Intense Anxiety	Moderate Anxiety	Low Anxiety
Low	55 (42)	36 (24)	16 (17)	35 (21)	16 (12)	19 (25)	25 (51)	14 (47)	9 (56)
Moderate	21 (16)	35 (23)	24 (25)	30 (18)	27 (21)	16 (22)	24 (47)	22 (71)	16 (99)
High	24 (18)	29 (19)	60 (62)	35 (21)	57 (44)	65 (87)	51 (102)	64 (209)	75 (473)
Total	76	66	104	60	77	134	200	327	628
	$\chi^2=39.78$; 4 df; $P<.001$ $\gamma=0.50$			$\chi^2=17.99$; 4 df; $P<.01$ $\gamma=0.30$			$\chi^2=52.84$; 4 df; $P<.001$ $\gamma=0.33$		

NOTE.—N's in parentheses.

the relationships are stronger than when Pearlin and Radabaugh employ mastery as an independent variable affecting predisposition to use alcohol for distress control. Statistically significant relationships are found under two of the three levels of predisposition, whereas Pearlin and Radabaugh found statistical significance only under the condition of limited mastery.

Utilizing self-esteem level as the dependent variable, the reanalysis presented in table 2 likewise produces statistically significant relationships between anxiety and self-esteem under the three different conditions of predisposition to use alcohol for distress control. Pearlin and Radabaugh's analysis, by contrast, produced a significant relationship only under the condition of low self-esteem.

When compared with the results presented by Pearlin and Radabaugh, the relative strength of the statistical relationships in this reanalysis and their significance under five of six conditions indicate that the alternative sequence of associations among the variables is more strongly supported by the data. A strong predisposition to use alcohol to control distress may lead to greater anxiety, to a lowered sense of mastery, and to lowered self-esteem. It is likely that these outcomes, in turn, add to distress, which in turn encourages further drinking. Finally, the relationships revealed here indicate that a minimal predisposition to utilize alcohol in this fashion may act to sustain less distressing levels of anxiety, self-esteem, and mastery.

While this formulation is, likewise, tenuous in the absence of longitudinal data, it appears just as plausible as the formulation offered by Pearlin and Radabaugh, if not more so. It is obvious that lack of access to their data precludes calculation of multiple correlations to compare with those reported by them in the concluding section of their article. It is, however, striking that these authors would present their formulation and analysis without recognition of alternative approaches, especially in light of the limitations of cross-sectional data.

It is evident that little of a conclusive nature is known about the etiology of deviant drinking or alcoholism, especially the role of social factors (Tarter and Schneider 1976). In the light of methodological problems accompanying such research, it would appear at least equally reasonable to consider these drinking behaviors as independent variables in analysis. In light of the comparative results of Pearlin and Radabaugh's analysis and my reanalysis, considerable caution should be exercised in accepting Pearlin and Radabaugh's interpretations as a contribution to understanding the role of social factors in the etiology of deviant drinking.

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THE SOCIOLOGICAL STUDY OF A SOCIAL PROBLEM: A REPLY TO ROMAN

We can discern two somewhat related issues threaded through much of Roman's critique, and a discussion of these might be worthwhile.

The first issue concerns data that are collected at a single point in time but which then are used to make statements about processes that unfold through time. If it was Roman's intention to remind us that the results of such analyses cannot be definitive, then he shall get no argument from us or, probably, from anyone else. Indeed, partly because of our own concern with the limitations of cross-sectional data, we are at this very time engaged in a longitudinal examination of some of the questions that grew out of our paper on the coping functions of drinking. However, if the thrust of Roman's criticism is that *nothing* can be learned of process from cross-sectional data, then clearly we are in disagreement. The network of relationships yielded by cross-sectional data, although not conclusive, often provides an invaluable accumulation of evidence that can lend support to theoretical perspectives, to their modification, or to outright abandonment of them.

The findings that emerged from our analysis were entirely consistent

with our theoretical perspectives. But since these findings cannot be considered as definitive, Roman was able to select out some of our data and recast them to conform to a different line of interpretation. The second issue we shall discuss concerns the substance of the alternative he is proposing. It is an important issue, for it involves not only the stance Roman adopts toward the study of drinking, but one too commonly used in the study of social problems in general.

The essential feature of the alternative model with which we are presented is its nonsocial character. This begins to manifest itself in the very conceptualization of drinking behavior itself. Since the care of alcoholics and problem drinkers has traditionally been the responsibility of clinicians, it is perhaps understandable that there has been a widespread use of clinical perspectives in trying to understand how and why people drink. This has resulted in a distinct tendency, even among nonclinicians, to adopt as their own the concepts of clinical theorists, concepts such as the "alcoholic personality." Once part of one's conceptual armory, they constrain one's ability to view drinking in terms other than individual pathology. This is apparent in Roman's keeping his consideration of drinking strictly within intrapsychic boundaries, and it is also apparent in the judgments he passes on drinking behaviors, declaiming some drinking behaviors as deviant and others as socially acceptable.

Aside from the questionable accuracy of these kinds of clinical judgments, they are essentially irrelevant to our attempt to identify and assess a widely shared drinking behavior having social ramifications. We are not in doubt that for many individuals drinking is supported by pathological personality problems. We do assert, however, that if there is a sociological contribution to be made in the understanding of drinking it will require the use of a different conceptual repertoire than that employed in trying to piece together the clinical portraits of individual problem drinkers. Thus, we found it useful to think of drinking as involving several distinct patterns of behavior. One such pattern is that which is represented in the efforts of people to deal with emotional distress arising out of the circumstances of their lives. Such a conceptualization ignores, without denying, the possible presence of individual pathology in order to delineate a type of shared behavior whose social antecedents can then be explored.

We submit, then, that the sociological study of drinking ought to expand the conceptualization of the behavior beyond clinical terms. And, furthermore, it ought to analyze this behavior in a way that brings into focus those elements of social structure contributing to it. Whether it be drinking or other behavior we are trying to understand, too often we not only define the problem but also proceed to examine and interpret the problem solely in psychological terms. The end product is an analysis of antecedents and consequences all residing within individual personality. This,

of course, is what Roman is doing in his selective reuse and recasting of our data and in his interpretation of the results. An inquiry that starts and stops with individual personality can hardly be considered of sociological interest.

Thus, far from overlooking the sort of alternative interpretation being proposed by Roman, we quite consciously eschewed it. It was our purpose to learn from our data, first, if patterns of drinking for the relief of distress do exist and, second, if this type of drinking is rooted in the larger social and economic order to which people belong. With this as our interest, we succeeded in showing that people's level of anxiety is related to the intensity of their economic strain and that anxiety, in turn, is associated with the use of alcohol for the control of emotional distress. This use of alcohol, however, is not an equally attractive coping mechanism for all people. When people enjoy a sense of mastery and a positive self-esteem, they are likely to deal with their anxieties in other ways. But we emphasized that mastery and self-esteem do not simply spring up willy-nilly; as we showed, these crucial self-attitudes are themselves influenced by the experiences of people in society's opportunity structures. What we aimed for in our analysis, therefore, and what we think we accomplished, is a sociological analysis of a social problem, not a psychological analysis of a clinical problem.

Does this mean that there is nothing to be learned from Roman's statistical exercise? We are afraid so. We are unable to accept what seems to be his assumption that somehow one's theoretical perspectives should follow from the way one happens to manipulate one's data. We have seen nothing to dissuade us from believing that it is best the other way around. And we certainly find unacceptable the notion that the value of divergent theoretical orientations can be judged on the basis of the values of the χ's they yield!

Roman expresses uneasiness about our treating drinking for the control of distress as a coping behavior. We, too, are somewhat uneasy, although probably for different reasons. To the extent that this pattern of drinking controls or reduces the pain and discomfort of emotional distress, it does represent coping behavior. But, whatever its efficacy in controlling distress, it may very well also produce negative consequences. Most obvious among the negative consequences is that drinking may be so effective in reducing tensions that it leads ultimately to alcohol dependence. Thus, its short- and long-range consequences may be very different. There may also be a difference between the consequences it has for individuals and those that it has for society. Drinking for distress control is a palliative, and a temporary one at that. It has the capacity to produce tranquilizing effects that help to sustain people who must accommodate to strainful circumstances but who have little hope of altering such circumstances. Viewed in these terms, alcohol can be seen as an instrument of social control; that is, in providing

relief from unpleasant symptoms of conflict and strain, it may at the same time help to maintain the very circumstances that contribute to such symptoms. To the extent that alcohol enables individuals to attain continuity of behavior and affect while exposed to severe pressures in daily life, alcohol also contributes to the continuity of the imperfect societal arrangements that may underlie these pressures. The point of these speculations is to underscore that drinking behavior is richly amenable to the study of both its social etiology and its social consequences, and as sociologists we need not confine ourselves to a concern with whether or not there are clinical pathologies involved in individuals' use of alcohol.

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COMMENT ON BRONFENBRENNER'S REVIEW OF *BEYOND ECONOMIC MAN*

To a considerable degree, Bronfenbrenner's review of my book, *Beyond Economic Man* (*AJS* 82 [November 1977]: 780–82), is about his attitude toward innovation in economics, his experiences at the Carnegie Institute of Technology, and his reaction to the Carnegie approach to economics. For the most part, it is not about my book at all. One of my complaints is that the review gives the reader very little indication as to what the book is about.

Bronfenbrenner believes that my subtitle, which suggests that the book contains "A New Foundation for Microeconomics," makes an exaggerated claim. I believe it is warranted. While there is no standard terminology in this area, a foundation for a theory may be said to contain two basic sets of elements: a set of postulates, and a set of variables. Thus, two theories which have different postulates and variables may be said to be based on different foundations. In table 1, I list as brief titles the variables and postulates based on the theory (selective-rationality theory) described in *Beyond Economic Man*, and their counterparts in the neoclassical theory. An examination of the table suggests that these postulates are indeed different. Hence, the system of analysis based on postulates I have chosen does in fact involve a new foundation for microeconomics—whatever its virtues or deficiencies may happen to be. I would even argue that the neoclassical theory is a special case of the more general theory which I have attempted to develop. Of course, in both theories prices and quantities of goods are variables.

TABLE 1

SELECTIVE RATIONALITY AND NEOCLASSICAL THEORY CONTRASTED

Category of Postulate or Basic Variables	Conventional Microtheory	Selective-Rationality Theory
1. Behavioral	Maximization or minimization	Selective rationality
2. Units	Households and firms	Individuals
3. Inert areas	None	Operational and behavioral variable
4. Effort	Implicitly assumed given	Discretionary variable
5. Contracts	Complete	Incomplete
6. Agent-principal relationship	Identity of interests	Differential interests
7. Interpersonal interactions	None	Some
8. Individual motivation as an input	Assumed given	Significant variable
9. Impactor-impactee relations	Not relevant	Impactor frequently not impactee

Some of the best minds of a large profession have refined modern microtheory for over a century, so that at present it is in a highly polished state. My effort does not result in a theory nearly as well honed or complete, but it is an open theory, and, with time, effort will be put forth to fill in the gaps. But differences in refinement and completeness do not alter the fact that different foundations are involved.

Bronfenbrenner seems to contend that my theory is essentially the same as that of the Carnegie school. While there are some overlapping areas, my approach is very different. Furthermore, some assertions made by Bronfenbrenner in this connection are incorrect. For example, the concept of inert areas is in no way the same as the concept of aspirations. The inert-area concept, within the context of the theory presented in the book, depends on the idea of effort positions (i.e., a job interpretation). If someone's effort position is in an inert area, he does not move to another position because the cost of moving is larger than the gain to be gotten from such a move. This is true quite irrespective of aspirations, and whether these aspirations are within or outside the inert areas involved. I find it completely unnecessary to use the concept of aspiration or its equivalent in my framework.

Nor is my X-efficiency concept the same as that of "slack." According to Cohen and Cyert, in their excellent review of the Carnegie school theories, the "difference between total resources and total necessary payments is called *organizational* slack. Slack consists in payments to members of the coalition in excess of what is required to maintain the organization" (Cohen and Cyert 1963, p. 333). The concept of X-inefficiency does not in the least involve excess payments to firm members over what is necessary to maintain the organization; X-inefficiency arises out of effort discretion and the notion of incomplete contracts and has nothing to do with claims, payments, or coalition bargains. There may be no slack, using the word in

its ordinary sense, but X-inefficiency may exist because inappropriate tradition-bound production procedures are used. It is also worth noting that incomplete contracts is a major postulate in my theory, which helps to determine X-efficiency, but it is not an element of the Carnegie school framework, nor of the neoclassical school. Whatever the virtues or limitations of my ideas, the main thrust of the theory and the way it operates are different from those found in the Carnegie school. In fact, I had not read most of the Carnegie school writings prior to developing my theory.

A few brief remarks about my selective-rationality concept may be helpful to readers. I do not believe that most individuals engage in maximizing behavior most of the time. As far as I can judge, most noneconomists agree. To broaden the possibilities of types of behavior included in economic behavior, I assume the following: Individuals work out a compromise between behavior based on the way they feel they *ought* to behave and that based on the way they feel they *want* to behave, that is, unconstrained behavior. Or, alternately, a compromise is reached between superego and id considerations. The end result of such a compromise is that the degree to which people deviate from maximizing behavior will depend on their personalities (i.e., the compromise) and the pressures that exist in a given context. Thus, individuals psychologically comfortable with "lackadaisical" behavior will not maximize in most contexts, but their behavior may approximate maximization in highly pressured contexts. Of course, peer group pressures and authority relation pressures are among the considerations that determine the outcome. These and related ideas are worked out in detail in the book.

There are a number of concepts which are significant in my theory and do not enter or are not emphasized in the Carnegie school approach. Among these are the incomplete contract postulate, the use of effort as a basic variable, the postulate of the existence of effort discretion, the selective rationality postulate, the emphasis on the differences of interest between principals and agents, the individual as the basic unit of behavior, and finally, perhaps most important, the difference between the impactor and the impactee (see chap. 15). This last point warrants some special consideration. The basic idea is that those who have an impact on the activities of others (and on the outcome of those activities), and ultimately on production costs, need not be burdened with the consequences. In fact, organizations are so arranged that frequently people attempt almost to minimize negative burdens. As a result, those who have an impact on others pass the consequences of their effort activities on to others, ultimately on to either stockholders, or consumers, or a mixture of the two.

Bronfenbrenner's claim that my book does not contain empirical evidence is incorrect. Chapter 3 (pp. 29–47) is mostly concerned with empirical evidence. Furthermore, the section spanning pages 42–44 reviews some recent econometric results which fit the X-efficiency theory. The studies by

Shen, Primeaux, and Shelton deserve special mention. For example, Primeaux and Shelton studied cases in which my theory and the neoclassical theory yielded different implications. The neoclassical theory argues that firms minimize costs irrespective of the degree of competition. My theory suggests that when competition diminishes costs rise. Primeaux found that the 49 cities that were served by two electric utility companies had lower costs per unit produced than those served by a monopoly. Similarly, Shelton found that owner-operated fast-food franchises had lower costs than did hired manager-operated units, although everything else about the units was identical.

It is important for theories to raise questions before a good deal of empirical work is stimulated and carried out. Thus, one cannot expect a great deal of empirical work prior to the development of a competing theory and the formulation of appropriate questions that would settle conflicting implications. Nevertheless, some data and general knowledge already exist, and I find the results encouraging. However, I do not believe that this is entirely an either/or problem. There are problem areas where mainline theory is useful, but there are other areas where it is quite inadequate, and, in my view, that is where the research frontier ought to be. A major area of inadequacy involves those cases that deviate from an approximation of perfect competition, including mixtures of noncompetitive governmental and private enterprise, or government-fostered economic activities, such as those handled by municipalities, states, and the federal government. By analyzing the organizational structure under which such activities in these areas are carried out (say, along the lines of my chap. 15), we may be able to understand why costs are so high or why they deviate so much from what we believe to be minimum costs. The important thing here is that these areas involve questions which the conventional theory does not ask.

A major difficulty in judging my theory, or any relatively new theory, is that it has to be compared with one which is exceptionally well established in the sense that it has been developed for over a century and studied assiduously by all practitioners in the field—one that is drilled into all graduate students in the course of becoming licensed practitioners. The practitioner's investment in the theory increases with experience. As a result, it is not surprising that there is resistance to new ideas. To get such ideas accepted involves uphill sledding. If my theory and the neoclassical theory had been presented simultaneously, I suspect that the result would have been quite different. To many the neoclassical theory would appear, on its face, so unrealistic as to be unacceptable. Its acceptability is in part a consequence of well over a century of tradition, the nature of the training in the field, and the fact that people are loath to give up a high intellectual investment. Part of the reaction to a theory such as mine, as well as to other different approaches, probably involves the psychological phe-

nomenon of brain encoding. This is illustrated by pictures in which there are two faces, but if one's mind is focused on one, it is almost impossible to see that the other exists. However, I have also found when giving talks on my approach that there is a significant body of economists who are clearly aware of many of the inadequacies of mainline theory, and who feel that microtheory may be facing a crisis unless more adequate solutions are found.

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REJOINDER TO PROFESSOR LEIBENSTEIN

Professor Leibenstein, my ex-colleagues at Carnegie-Mellon, and I all accept a minority view (among economists) that contemporary microeconomic theory has grown "too big for its britches" (its social-psychological underpinnings), that it stresses precision against accuracy and relevance, and that systematic biases (as well as embarrassingly large random errors) sometimes result. (That is why I personally dislike teaching the subject at any level above supply-and-demand *cum* pure competition.) Where Leibenstein and I differ is about the efficacy of his proposed remedy or substitute.

Here, too, the issue is not who cribbed from whom; nobody cribbed from anybody. Nor is it whether Leibenstein is simply Carnegie in Harvard dress; there is much more than that in *Beyond Economic Man*. But putting the matter crudely, I feel that most of what is best in Leibenstein overlaps with "the Carnegie school," and that much of the rest is vague, difficult to understand, and highly resistant to empirical testing. Leibenstein replies (when translated into an equivalent crudity) that I suffer from too many years at Carnegie-Mellon, plus overexposure to the standard stuff, plus further hardening of my intellectual arteries from other causes. These are all subjective issues for readers of Leibenstein—and "Carnegie"—to decide for themselves.

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Review Essay[1]

Schooling in Capitalist America: Educational Reform and the Contradictions of Economic Life. By Samuel Bowles and Herbert Gintis. New York: Basic Books, 1976. Pp. vii+340. $13.95.

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Schooling in Capitalist America is a bold and eloquent attempt to wed a Marxist analysis of the role of schooling to a radical critique of liberal reforms in education. The methods and analysis are eclectic and interdisciplinary; by its very nature the book defies a comprehensive review. The work is flawed, both theoretically and empirically; however, as an ambitious effort to integrate diverse findings into an overarching framework, it deserves the attention of specialists. Instead of grappling with global concerns, the bulk of this essay will be devoted to an assessment of the empirical work which overlaps and takes issue with the sociological literature on stratification and social mobility.

In brief, Bowles and Gintis argue that a humane and liberating educational system is incompatible with the organization of work in a capitalist society. Although schooling does not directly affect the degree of social inequality, since this is determined in and by economic institutions, schools foster the patterns of control and hierarchical subordination crucial for the work place. The function of education is to reproduce and legitimate social inequality rather than to create it. School reform in a capitalist context therefore tends either to be naive and unworkable or to serve the interests of the dominant class. Schools are strategic to the articulation of an alternative vision and school reform is potentially an important part of a broad assault on the structure of privilege. Yet all past reform movements have failed because they neglected to develop an appropriately critical or revolutionary consciousness among adherents, because they neither analyzed nor understood the nature of capitalism, and because they were not based on a unified, egalitarian mass movement.

The authors examine the historical dynamics of educational change during three periods of prolonged economic crisis and social dislocation and ferment. The contradictions inherent in capitalism are seen as the source of institu-

[1] Helpful comments and criticism were provided by Robert M. Jackson, Rosemary Taylor, and Kay Trimberger.

tional disjuncture and protest. Capitalist growth necessitates the incorporation of ever broader segments of the population into the wage-labor system; this process is inevitably accompanied by widespread discontent and protest. Education during such periods mediates and deflects class conflict and serves to stabilize the social order through accommodation.

Bowles and Gintis are best when integrating diverse historical data on the expansion of public schooling, detailing the repressive aspects of educational practices, or when outlining the contradictions implicit in the ideologies of liberal reform. Unfortunately, the crucial theoretical linkages between these arguments are never drawn with equal force or analytic rigor. The notion of "correspondence" is introduced both to support the contention that education is essential to the reproduction of labor power and to account for the observed historical congruence of periods of economic crisis and educational change. The authority structure of classrooms "corresponds" to the social relations of the work place; schooling therefore is a mechanism for inducing attitudes and values necessary to the economic order through the socialization of compliance rather than through the transmission of skills. Equally, it is argued that changes in the educational system "correspond" to the needs of the capitalist system during particular historical periods.

This theoretical framework is sufficiently general to encompass both change and stability without, I think, providing an explanation of either. The reasoning is similar to that found in functionalist thought, with all the analogous pitfalls. The authors are prepared to argue, albeit in different contexts, that schools are marginal to the workings of capitalism yet central to its perpetuation; somehow the origins of both student revolt and worker acquiescence are to be found in classrooms. Widespread economic disruption clearly influences other institutions, yet the pattern of educational accommodation varies enormously and, within the model provided, inexplicably. That educational systems respond to social change is obvious; that the historical changes observed bear any necessary or inevitable relation to capitalist development is not. Even accepting the primacy of economic institutions in generating change, industrial capitalism seems to flourish in societies with vastly different systems of schooling.

Bowles and Gintis argue, I think correctly, that social change in both education and the economy reflects conflict between competing interests; however, the issues and social composition of opposing forces are very different. Educational elites are not capitalists. Conflicts between teachers and students, professionals and laity, agrarian parents and urban politicians, or between taxpayers and the state do not fit neatly into a capital-labor dichotomy. The authors tend to regard the political interests and bureaucratic squabbles which have so dramatically shaped American education as epiphenomena. In my opinion, a compelling class analysis should link the

social bases of power to the struggles and outcomes observed within a particular institutional arena. Although capitalism is no doubt implicated in the process of change, it is not identical with urbanization, industrialization, or bureaucratization and should not be treated as such.

Although Bowles and Gintis have not written a treatise on stratification per se, they assume a relatively static class structure in both the analysis and their discussion. Class is defined in Marxist terms as based on relations of property and production; however, the analysis presented is based on the more familiar set of continuous variables used in status-attainment models. While there is no inherent contradiction between a stable and enduring system of class relations and substantial intergenerational mobility based on occupational prestige, Bowles and Gintis explicitly argue that parental status is largely transmitted to children, that schooling provides scant opportunity for betterment, and that the liberal meritocracy based on talent and ability is rhetorical rather than factual. Moreover, they support these conclusions with slightly modified versions of the mobility models current in the empirical literature.

This debate has of course numerous theoretical implications for the discipline. The presence of substantial intergenerational mobility resulting largely through educational attainment has not been seriously contested since Blau and Duncan published *The American Occupational Structure* (1967). Analysts have differed about the significance and interpretability of unexplained variance and about whether the important features of stratification could be captured by individual-level analysis; the basic methodological innovations and conclusions, however, have remained unchallenged. Bowles and Gintis have presented such a challenge and have done so in a cogent and technically sophisticated manner. For this reason, if for no other, their work deserves serious attention.

The critique of status-attainment models offered by the authors is technical; it is found largely in the appendices and in prior published papers. They raise three interrelated issues. First, the equations found in the status-attainment literature are misspecified by not including background variables of sufficient quality and quantity to capture the effects of social class origins on adult outcomes. Second, an imperfect congruence between the construct and the operationalization of variables leads to attenuated relationships. Third, response errors are not random but systematic and routinely operate to depress particular relationships and thereby minimize their unique contribution relative to other variables in the model.

Equations are misspecified because critical aspects of socioeconomic status are omitted from the models. Bowles and Gintis argue that a number of background variables should be included such as parental income, wealth, and position in the hierarchy of work relations. They attempt to rectify these omissions by introducing parental income as an aspect of

ascribed status which influences schooling and later attainments. The construct central to the measurement of adult outcomes is, for Bowles and Gintis, economic success—by which they mean income rather than occupational status. Following other economists, they use lifetime earnings or permanent income as the major dependent variable. Finally, they argue that each background variable is measured with considerably more error than are the self-reported achievements of the respondent. They contend quite plausibly that retrospective responses about the status of one's father at the age of 16 are likely to be less accurate than reports about the years of schooling and current occupational positions of the respondent. Bowles (1972) has culled the literature for estimates of the reliability and routinely corrects every correlation for attenuation. The resulting matrix of corrected correlations forms the basis of his estimates of the "true" effects; the revised models tend to support Bowles and Gintis's assertion that parental status is a considerably more important aspect of status transmission than previously assumed and that it operates with far less dependence on schooling.

Substantively, it is worthwhile to summarize the issues that are not in contention as well as those that are. Bowles and Gintis would agree with conclusions reached by Blau and Duncan (1967), Duncan (1968), and Duncan, Featherman, and Duncan (1972) that (1) schooling exerts a direct effect on adult attainment, (2) IQ influences years of schooling attained, and (3) IQ is not the principal mechanism through which intergenerational transmission of status occurs. The points at dispute are (1) the relative importance of socioeconomic background and the IQ of children on schooling and (2) the magnitude of the direct and indirect effects of socioeconomic status on later attainments.

An article by Bowles and Nelson (1974) provides the evidence on the first point. The piece is aimed most directly at theories of genetic inheritance, although it also takes issue with conclusions reached by Duncan (1968) and others. According to Duncan, IQ contributes a substantial amount of variance unrelated to social background but which must be "translated into skills" or "certified" by formal education in order to influence status outcomes. For Bowles and Nelson (1974), early IQ is much less important than socioeconomic background, while schooling simply "ratifies" the influence of origins on later success. The debate here turns primarily on the value of the correlation between IQ and schooling, since accepting the value used by Duncan and correcting the estimate for errors in measurement of the order assumed by Bowles and Nelson would imply that the direct effects of background and ability were nearly equal in size. The corrected correlation between early IQ and schooling of .47, used by both Bowles and Nelson (1974) and Bowles and Gintis, is derived from the California Guidance Study. This figure is based on 70 children in grades one through three and is substantially lower than the uncorrected value of .54 posited by Duncan for

grade six. The discrepancy between these values is probably due to the greater unreliability of early test scores. The correction for attenuation used by Bowles and Nelson is based on a reported test-retest reliability estimated at .90, which cannot begin to account for the sharp increase in correlations reported a few years later. While it is possible that this increase is due to some aspect of primary schooling, I know of no evidence which indicates such a pronounced and enduring effect of early education on IQ. I suspect that the true correlation is understated by Bowles and Gintis and that, even if one were prepared to believe the substantial effects posited for socioeconomic background, differences in ability would account for perhaps half of the total variation in schooling.

The question of central importance concerns the transmission of parental status intergenerationally—that is, the direct and indirect effects of socioeconomic status. Disentangling the assumptions regarding socioeconomic background is rather complex. First, Bowles and Gintis use three measures to assess the effects of parental status: father's occupation, father's education, and parental income. The parental-income measure is largely hypothetical, since the requisite data were not available. It is assumed to be correlated with own income and occupation to the same degree as father's occupation and correlated with other measures of parental status to the same degree as the status measures are intercorrelated for an older cohort of respondents. These assumptions are tantamount to assuming that an unmeasured variable which has predictive power equal to that of father's occupation should be included in the equations. In addition, Bowles and Gintis assume that each parental background variable is measured with substantial error. Estimates of the unreliability of reports on father's occupation and educational attainment were available from a small sample of males in Chicago, whose responses could be matched with those of their fathers from census returns a generation earlier. These estimates of the reliability coefficients are substantially lower than the best estimates available for response error calculated from census returns. Corrections for attenuation therefore augment the magnitude of the relationships between social class background and later attainment disproportionately. In addition, Bowles and Gintis assume that the errors in reporting are positively correlated within generations, although not between them. The rationale is that respondents make their fathers' attainments more consistent than in fact they were. Corrections for correlated error tend to reduce the observed joint variance and thereby increase the estimated unique effects of each background variable.

What is the status of such assumptions and procedures? The incorporation of parental income has become commonplace in status-attainment models, and this variable does seem to contribute unique variance to the determination of schooling as well as to have an impact on adult income not mediated by other intervening variables (Sewell, Hauser, and Featherman 1976),

although not as dramatically as the models of Bowles and Gintis suggest. There is also evidence that other indicators of status such as one's position in the hierarchy of production interact in important ways with schooling and income (Wright and Perrone 1977). I am dubious, however, about whether class position defined by authority relations could be transmitted to sons independently of paternal schooling and income. Wealth, in contrast, is a component of parental status which could be readily "inherited," perhaps irrespective of son's achievements. Wealth tends to be quite difficult to measure, however, and for this reason has not yet been used in mobility models, despite its theoretical importance. Since wealth is highly concentrated and affects only a small minority to any significant degree, however, I would not be greatly surprised if models of status attainment were substantially unaffected by its inclusion. One of the recurring confusions about models of status transmission is that they "refute" theories assuming the stability of elites. A "ruling class" almost by definition would be too small to change the parameters of attainment significantly. It is easy to demonstrate that a small elite, capable of transferring status to sons without diminution, is compatible with mobility of the magnitude observed. In sum, the evidence that omitted variables would alter the coefficients markedly has yet to be documented, and the introduction of hypothetical relationships of the strength suggested by Bowles and Gintis cannot be taken as evidence of their importance.

Corrections for attenuation, however, do change the magnitude of effects and the conclusions. Measurement error has become an issue of no small importance in empirical work. Recent work questions the validity of the estimates used by Bowles and Gintis and cautions against "borrowing parameters" from diverse sources. Bielby, Hauser, and Featherman (1977*a*, 1977*b*) present data and estimates of response bias which incorporate explicit measurement models, based on reinterviews. In particular, the data for nonblacks indicate that background variables are not measured less reliably than self-reports and that errors tend to be random rather than correlated. Response bias is not, of course, equivalent to errors due to faulty recall; Bowles and Gintis could conceivably maintain that retrospective reports are still an inaccurate account of reality, despite the fact that the responses given at two points are consistent. Correlated errors, however, seem far less likely. It is the case that measures of parental status are less highly interrelated than those collected for respondents old enough to be the relevant fathers. However, the discrepancies are probably due to demographic factors and sampling rather than correlated measurement error. Fathers are simply not a random sample of the respondents two decades older.

The strongest evidence for the proposition that background factors are underestimated is, in my opinion, the large observed correlation between the attainments of brothers. Bowles (1972) uses this fact to justify his adjust-

ments in the correlations. Mechanisms of intergenerational influence of sufficient strength to account for the similarities of brothers have yet to be isolated, short of presuming that siblings have an enormous direct impact on each other. The list of possible causal factors is very large, limited only perhaps by the imagination of the analyst. It should be possible, however, to categorize potential effects and to separate interfamily differences which are primarily indicative of class position, such as home environments, from those which could operate independently of social class, such as parental values and child-rearing practices. To ascribe all the effects operating within families to socioeconomic status is misleading without very specific knowledge of the mechanisms at work.

Technical criticism results in improved models to the degree that explicit, quantifiable assumptions are made. The errors-in-variables critique is at once an elegant method for refining the measures and models utilized and a tool for refuting substantive arguments. Bowles and Gintis should be credited with raising these issues and applying them in a substantive context in which their importance is dramatized. However, improved measurement models have not and perhaps cannot fully resolve the substantive issues at stake. Had Bowles and Gintis relied on equations identical in form with those prevalent in sociology, they would have reached conclusions quite similar to those of the status-attainment literature. When Bowles and Nelson (1974) present data on the determinants of occupational status, the coefficients are quite similar to those estimated by Bielby et al. (1977*b*). The standardized partial effect of schooling, net of age and background, is estimated by Bielby et al. at .628, while the complete equation explains just less than half of the variance. Bowles and Nelson estimate the direct effect of education, net of background, and early IQ as between .452 and .643 for different age cohorts, with R^2 varying between .45 and .56. Both sets of results are based on matrices corrected for measurement error, and both equations suggest that the effects of parental status on occupational prestige are largely transmitted via schooling, with a goodly portion of the variance left unexplained.

Bowles and Gintis do not, however, accept occupational outcomes as indicative of relative attainment. The single most important difference between their models and those in the status-attainment tradition is the choice of dependent variable. Since schooling is linked much more tangentially to income than to occupation, models which omit occupational status yield considerably lower estimates of the effects of schooling relative to other variables. The assumption that income is the important component of stratification leads quite directly to the interpretation that schooling is not a powerful or unique influence. Indeed, if income is the central measure of adult status, the stratification system appears quite loose and indeterminate.

These issues are substantive rather than technical. To my mind the implicit theoretical questions posed by *Schooling in Capitalist America* are both more important and more difficult to resolve than the technical criticism. Perfect measurement is likely to remain elusive, even for those concepts which can be determined unambiguously. Sociologists need to spend at least as much time conceptualizing the mechanisms and contingencies of attainment as in replicating and refining models. The substantive meaning and institutional context of notions such as intervening or mediating influences are clearly as important as their standard errors. Bowles and Gintis have shown that interpretations of the attainment process depend on assumptions about measurement as well as on the varying interpretations about what is most important theoretically. On balance, the latter seems to me far more important than the former.

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Review Essay

Culture and Practical Reason. By Marshall Sahlins. Chicago: University of Chicago Press, 1976. Pp. x+252. $17.50.

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Many social scientists practice their art like a sectarian religion. Much as a child grows into the religion of its parents, they adopt the credo of their graduate school, find shelter and community, and regard other sects with condescension. Discussions within the walls of a given school are often therefore paradoxically on a higher intellectual plane than those which occur across them.

Marshall Sahlins is splendidly free from such intellectual parochialism. To paraphrase André Gide, he does not sail cautiously in sight of a coast but crosses open seas. The major issues which divide alternative approaches to understanding culture take for once center stage, and that fact alone is enough to make this an important book.

Fundamentally, Sahlins orders the contending forces into two opposing camps. On one side he groups together the diverse attempts to explain the phenomena of culture from utilitarian, ecological, adaptive, and other orientations of that general rank. These constitute the "practical reason" approach to anthropology of his title. According to this approach, cultures serve concealed yet finally unsubtle and pragmatic purposes. The most graspable summation of this way of understanding culture may be Morgan's phrase that the key to the mystery of culture lies in the "advantages" which it confers (pp. 57–64).

There is no equally convenient emblem for those in the other corner of the ring. Sometimes they define their position by negation: culture *cannot* be so explained; it is not a contiguous sequitur to nature but is autonomous and can be understood only if an independent "structure" is postulated. One speaks of this *novum* as something that inserts itself among the biological, economic, or utilitarian forces, and that not only gives them "meaning" but also qualifies, interprets, and refracts them. The most direct—though by no means the clearest—formulation of the conflict is couched in terms of a contested primacy: for the first camp culture is derivative, while the adherents of the other insist that culture is somehow primary and basic.

The critique of any anthropology founded on practical reason is brilliantly articulated in this essay through a synopsis of the history of that discipline and richly illustrated with elaborate examples. Still, the embellishments of that presentation hide the fact that the cardinal objection is

really very plain. A diagrammatic example, purposely simplified, will make the logic of the point at stake more readily apparent. If we abstract all ethnographic detail and isolate a single feature from its context, we might end with the observation that father and son in a given culture cooperate in the task of food production (pp. 8–14). Within the paradigm of practical reason, the general form which any explanation of that fact must take converges on some practical "advantage," which in this instance might be obvious: four hands in cooperation do more work than four hands individually. Though this sounds plausible enough, the crucial flaw is easily exposed, for the same material advantage could evidently be realized in countless other ways: for example, if any other pair made a team who might be brothers, cousins or neighbors or who might just work well together. Indeed it is highly likely that some of these other pairs might perform more efficiently and thus fulfill the postulated "function" better than father and son. The practical advantage therefore gives us only the illusion of an explanation. Customarily one puts this more charitably and says that some questions have been left unanswered. But this is an understatement: strictly speaking, no questions at all have been answered satisfactorily. If other pairs—or trios or foursomes—can produce food together just as well or even better, then we do not know why one culture should select father and son over every other conceivable combination—and that was the phenomenon to be explained.

Once one recognizes that there is no hope of ever giving a convincing demonstration that an observed cultural arrangement is the only and best way to secure any—and especially a vaguely specified—"advantage," the generality and seriousness of this objection should appear in its full force.

Sahlins's case for the competing paradigm is again a tapestry of complex arguments into which he weaves broad and finely worked examples. The crux of the matter, however, can be clarified once more if we strip one illustration of all verisimilitude and bring the abstract logic of the situation into relief. Take the instance of the Moalans (pp. 24–47): the "structural" explanation of their system could be compressed into the claim that certain patterns are isomorphic to each other. There is a fundamental distinction between "land" and "sea"; this geographical division is reproduced on the social plane via a set of multiple associations as between "land" and the "indigenous" and "sea" and the "foreign," so that it recurs as a distinction among groups: the Land- and the Sea-people. With some alterations, that same opposition also organizes social space: the "chiefly side" of the Moalan dwelling house is set facing seaward, the "common side" facing inland. The same pattern operates in numerous other contexts. For our purposes, it is important to note that the relationship is often less direct; thus we also encounter its complete inversion: the cooking done by men—who are generally associated with the "noble" sea—is done in underground ovens in the "lower" medium of the land, while women cook in pots, above ground, with the medium of salt water.

In the debate between the advocates of practical reason and those of

structuralism, the explanations that each gives have been viewed consistently as alternatives in competition with each other. Practical reason is charged with a failure to explain the very features which constitute the culture of a group, and the presumption is therefore that structuralism is superior if it succeeds in the task in which its rival fails. That this is the basic relationship between the two appears to be so obvious that one assumes it without further argument, and it is certainly the fundamental premise of Sahlins's entire book. Nonetheless in my view this constitutes a mistake, and its commission is the one most serious defect of this otherwise admirable book.

On one level the heart of the matter can be put in almost childlike terms. The essence of structuralism is avowedly the "primacy" of culture; its credo is that the code or symbolic system is already "in place," somehow, from the beginning. One has only to take this at face value to realize that the two paradigms are alternative roads to the same goal. The assertion that the system already is "in place" in effect announces that it will not and cannot be explained; and the insistence on the primacy of culture amounts to the same thing. At bottom, the very possibility of offering an explanation is as it were ruled out by definition: for if cultural systems are "primary," then where should the explanation come from?

The true relationship between these two approaches to anthropology is therefore actually quite different. Bluntly put, practical reason has tried and failed, while structuralism does not even make the attempt. It takes its stand on this refusal. One could say it is a capitulation in disguise.

Of course there is still a second and more intricate level to the question. Perhaps the basic code is indeed "in place" and has to be accepted as a given. Still, the promise held by structuralism is largely that surface embodiments and refractions can be derived from this underlying structure and that they can be explained at any rate more adequately in this fashion than by recourse to practical "advantage." At this juncture structuralists, however, often substitute other terms for the still relatively manageable idea of a derivation. They speak of "meanings," disregarding the possible protean interpretations and the profound opacity of that word, and invoke analogies to language, suggesting that a culture can be understood like a text. Yet this comparison illuminates the baffling with the downright mysterious, for we are miles from grasping what our comprehension of a text in fact involves.

Some of this obscurity, however, is dispelled easily enough. We can dispense with the analogy to meanings and the deciphering of manuscripts, and we can also do without the notion that surface manifestations are "derived" from a depth structure. What a structuralist interpretation actually does can be assessed much more directly.

Consider how really parallel is the abstract logic of our two examples. Since any material "advantage" could have been realized just as well by a host of other forms of cooperation, practical reason could not explain the cooperation between father and son. True, but is structuralism not faced

with exactly the same difficulty? The land/sea distinction may recur in a great range of different contexts among the Moalans, yet it is inconceivable that it would appear monotonously in every feature of the culture. But if not, then what explanation, what reason can be given for the selection? In other words, if this dichotomy separates social groups and organizes social space yet plays no role in other spheres (assume, purely for the sake of the argument, that it is not operative in the segmentation of time), then what answer can the structuralist give to the question of why this should be so? And are there not literally countless questions of the same type? If the division is inverted in the cooking of food but not in the dwelling house, could one not ask again, Why here and not there? And so on.

We can go further and apply a harsher and more revealing gauge by turning the table and for once subjecting our own culture to this kind of structural explanation. This is entirely in the spirit of Sahlins's book: his penultimate chapter bears the title "La Pensée Bourgeoise: Western Society as Culture." If we simplify again to a ludicrous degree, though still with the same serious purpose of achieving greater clarity, we might observe that the division between male and female is for us no doubt very basic (more so than that between land and sea), and—following Sahlins—we can then also see that, for instance, we generally associate a square shape with the category "male" and circular and round shapes with the category "female" (p. 219). But to what degree and in what fashion does this furnish us with an explanation of any features shown by our culture? How does it help us with the fact that our city blocks and most of our houses are generally square, while cooking pots are almost uniformly round? What questions does it answer for us, especially if we note an endless number of "inversions" such as the fact that nearly all of our kitchen sinks are square, while men's pocket watches are almost always round?

Permit me to spell out what this application of structuralism to our own society brings to light. (1) It is naturally possible to *register* the embodiments, refractions, and inflections of a cultural code and to discover similarities between its postulated structure and recurrent patterns. But it is not possible to specify with any real exactness why the divisions of a code are projected into one context but not into another and why the correspondence takes one form in one instance and different forms—such as inversions—in all kinds of others. In short, the relationship between a code and its manifestations remains unavoidably extremely "loose." (Just how loose can be observed in Lévi-Strauss's practice.) (2) But if this is so, then the observed isomorphisms between a code and any other aspects of a culture have not been in any genuine sense *explained*. The intriguing particulars of the ethnographic reality are apt to hide this, yet if they are collapsed into a diagram this absence becomes glaring. The associations between the female and roundness and the male and squareness do not provide us with an explanatory base for any part of our culture for at least two fundamental reasons: first, cooking pots are not round because they are female but because that shape makes them easier to clean, and their femaleness there-

fore does not explain their roundness. Second, there are countless mal objects which are not square but round (e.g., pocket watches), and to cal this an inversion does not explain their roundness, which again has othe determinants. But if this is true in our own case, it is also—though mucl less obviously—true of the Moalans. That the land/sea division occurs i many contexts is by itself merely an observation, though one of extraordi nary interest. However, the recording of this fact alone does not explai anything whatever. On the contrary, that this isomorphism occurs an where and how and for what reasons is itself a feature that must be ex plained. (3) Sahlins's general theoretical position is that the forces of na ture are always interpreted and made specific through the imposition o the meanings of the code "before" they impinge upon and operate withi the realm of culture. As a critique of the varieties of anthropology founde on practical reason, this is both true and extraordinarily important. M basic disagreement is over the reverse side of this coin, for Sahlins seem to believe that, once the material or natural elements are understood t have been filtered through this screen of meanings, an *explanation,* both o our own and of "primitive" cultures, will be feasible. And this I see n reason to accept.

The point is not that Sahlins's positive construction falls somehow shor It is the other way around. The solution he tries to offer in his book goe too far. He is still looking for an anthropology that will explain a cultur and consequently he measures his own stance against that standard. Bu even his integration of a material dialectic with the idea of a symboli code cannot fulfill that aspiration.

Conceivably anthropology will have to undergo yet another large transi tion—the break with the tradition of imitating the natural sciences may b only a first step. In the next stage one might have to revise the aim an turn away from the ambition to find explanations. This need not be th calamity that some might fear. On the contrary, a new and different epis temological orientation might well establish that there are forms of knowl edge which possess great power and legitimacy without allowing us to "ex plain" the phenomena in their domains. Thus one could imagine an anthro pology that, freed from the requirement to offer explanations, would no see itself as unemployed but would, instead, place a much higher value o other epistemic enterprises (much as art critics do not presume to "ex plain" a work of art but try, instead, to unfold it). If such a metamorpho sis should actually occur, Sahlins's book might be seen in retrospect in tw quite different perspectives. Someone might then say, "Of course, neithe practical reason nor any version of structuralism (including Sahlins's own can answer questions such as why father and son cooperate in certain cul tures. However, this book brought us much closer to the point at which w turned from the idolatry of explanations—and this was the most importan function it performed."

Review Essay: Rituals, Symbols, and Society—Explicating the Mechanisms of the Moral Order

Purity and Danger. By Mary Douglas. Harmondsworth, Middlesex: Penguin Books, 1970. Pp. 220. £4.50 (cloth); £0.30 (paper).

Natural Symbols: Explorations in Cosmology. By Mary Douglas. New York: Pantheon Books, 1970. Pp. xvii+177. $5.95 (cloth). New York: Random House, 1972. Pp. 218. $1.95 (paper).

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Religious rituals, performed with all their pomp and ceremony for the gods, obviously have something to do with the reaffirmation of the social order. For that matter, we have even come to take for granted Erving Goffman's (1956) concept of interaction rituals as part of the daily props maintaining the definition of the social order. But what of straightening our desks, brushing off our coats, or tidying up in general? Can these seemingly mundane activities, with no gods and not even performed for occupants of other social statuses, also carry ceremonial significance? Mary Douglas's answer is yes, for the moral order enlists our cognitive classification system—the social assignment of things to their place—as a ritual. The moral order is infused into our structuring of reality with categories and classifications such that sorting, tidying, cleaning, and putting things in their place in general act to reinforce ritually both the structure of social reality and the larger moral order itself. "Place," in the sense of things belonging in their place, is more than mere factual or cognitive location. There is a moral component in assigning reality to different categories and relationships that becomes particularly apparent when things get out of place. At that point we are mobilized to reset the structure of things and thereby reinforce ritually the structure and substance of the social order. From large-scale religious ceremonies to the sorting of papers on our desks, the search for those ritualistic props of social reality has been extended by Douglas to whole areas of behavior not previously seen as part of society's arsenal of ritual. Questions of pollution, cleanliness, hygiene, and above all the idea of *dirt* involve beliefs she analyzes brilliantly in *Purity and Danger* and *Natural Symbols* to approach the old Durkheimian question of just how the social order ritually renews and rejuvenates itself. This essay focuses on those two

works, published some time ago. Her most recent work, *Implicit Meanings: Essays in Anthropology* (1975), which elaborates many of the same ideas, has already been reviewed in this *Journal* (*AJS* 82 [March 1977]: 1152–56). I turn first to some of the issues raised in *Purity and Danger*, originally published in 1966.

Douglas's conception of dirt is similar to that of deviance in sociology: "Dirt is essentially disorder. There is no such thing as absolute dirt: it exists in the eye of the beholder." Also our reaction to dirt and pollution performs the same function as the discovery of deviance: "In chasing dirt, in papering, decorating, tidying, we are not governed by anxiety to escape disease, but are positively re-ordering our environment, making it conform to an idea" (p. 12). Finally, the sociological origin of dirt is the same as the Durkheimian origin of deviance: " . . . Dirt, then, is never a unique, isolated event. Where there is dirt there is system . . . we are left with the old definition of dirt as matter out of place . . . [which] implies two conditions: a set of ordered relations and a contravention of that order" (p. 48). This is the Durkheim of the *Division of Labor:* ". . . We must not say that an action shocks the common conscience because it is criminal, but rather that it is criminal because it shocks the common conscience." Douglas's version would be something to the effect that we must not say that an object is out of place because it is dirty but, rather, that it is dirty because it is out of place. "Shoes are not dirty in themselves, but it is dirty to place them on the dining-room table; food is not dirty in itself, but it is dirty to leave cooking utensils in the bedroom; similarly, bathroom equipment in the drawing room, clothes lying on chairs; out-door things in-doors; upstairs things downstairs . . . in short, our pollution behavior is the reaction which condemns any object or idea likely to confuse or contradict our cherished classifications" (p. 48).

Since the *Division of Labor*, our understanding of the origins of deviance has been extended by Kai Erikson (1966). His notion of the community *creation* of deviance makes the seemingly simple analogous issue of being out of place a somewhat more complex matter. Deviance and dirt can be seen as arising in two ways. First, the original Durkheim proposition, adopted by Douglas, focuses upon the individual crossing moral boundaries or things being out of place. The second, advanced by Erikson, argues that society need not wait to reaffirm the moral order until some person or thing strays across moral boundaries or gets out of place. The community can move its moral boundaries in such a way that individuals who have done nothing are now on the other side of the line and are deviant or out of place. In effect, the "place" can be moved while the individual stands still; the result is the same; the only difference is how he got there.

Can we expand the Douglas formulation in the same way Erikson expanded the initial Durkheim proposition? The answer seems to be yes.

Consider, for example, the common phenomenon of discovering that one's office is a mess: papers strewn on tables, pencils lying everywhere, computer printout on chairs, and books piled on the desk instead of on shelves. In short, a classic Douglas situation, with things clearly out of place. Given this situation, she would predict that we reorder our environment by straightening up the office. But we do not always tidy up when we confront the objective presence of things clearly out of place. In some sense, then, a mess is simply not a mess. Just as dirt is not dirt in and of itself, so some things are not perceived as polluting, dirty, or deviant simply because they are objectively out of place. Many things are out of place all the time and in all aspects of our social and environmental relationships. The question becomes, Is it things being objectively out of place or our *reaction* to them that generates their moral designation as dirty, polluting, or deviant?

The issue here is something like the idea of primary and secondary deviance. There are many things out of place, like primary deviance, but only some elicit a response and get labeled deviant or, in our case, dirty. Our offices are always more or less messy. The phenomenological status of the world as in or out of place does not necessarily inhere in the actual structure of objective reality. The idea that we create deviance as a response to social crises suggests that whether our offices are experienced as messy or not may have less to do with strewn pencils and papers than with what is happening to the structure of our social relations. A departmental crisis, a rejected paper, the termination of one project and the beginning of another, or any number of other crises can result in the discovery that one's office is a mess and needs cleaning. With Erikson's boundary crisis, we create deviance and experience a crime wave. With a crisis in our own social relations, we can create dirt and experience a messy office. The functions of both are the same: to reaffirm ritually the blurred, unstable, ambiguous, or somehow threatened social order. In sum, it seems that the Douglas and Erikson models both operate. A shoe on the table, being itself out of place, does generate a reaction. But a messy office may not be so acknowledged until a crisis in our social relations occurs.

The crucial element for success in the analysis of symbolic systems is the goodness of fit, so to speak, of the substantive variation within the symbolic material and the variation in the morphology of the social system that these symbols are supposed to mirror. In this regard Douglas ingeniously opens our eyes to the incredible range of things that can take on symbolic significance. She focuses upon the biological body as a symbol of the social body. Her central proposition is that bodily control symbolizes social control. If social control is strong, bodily control will be strong, as in concern over the propriety of laughing, breaking wind, belching, publicly yawning, and so on. If social control is weak, the body will be relaxed, informal, or untidy and

sloppy, as with loose-fitting clothes or shaggy hair. Academics and artists, she points out, symbolize the less formal definition of their role with a "display of carefully modulated shagginess according to the responsibilities they carry" (*Natural Symbols*, p. 72; all citations are to the Pantheon Books edition). Douglas also suggests that where there is explicit social control over individuals a positive value will be placed on consciousness. Trances and trancelike behavior or other situations involving a loss of control over the body will be frowned upon in more highly structured situations. The body can also be a symbol for concern about group boundaries. Where there is concern about entrance and exit to the social body there will be a concern about bodily orifices: spittle, blood, milk, urine, feces, and tears, which have traversed the boundary of the body, can serve as symbols for concern about entrance and exit to the social body. Similarly, ideas about the relationship of the heart, head, and sexual parts can symbolize social hierarchy or relations between groups and institutions. The biological body, in short, is a perfect tabula rasa on which the form and structure of society can be imprinted.

Douglas is also interested in specifying some general features of social systems which generate this concern about pollution and ritual cleanliness. She discusses two kinds of social situations. One line of Durkheimian thinking, found in Peter Berger's (1967) notion of anomic terror, Goffman's (1967) breakdown in social interaction, and Erikson's boundary crisis—and supported by Douglas—is that ill-defined, ambiguous, or marginal social situations generate ideas of power, potency, and danger. "Danger lies in transitional states: simply because transition is neither one state nor the next, it is undefinable . . . whenever the lines are precarious we find pollution ideas come to their support" (*Purity and Danger*, pp. 116, 165). In contrast, though, she also argues that ". . . pollution is a type of danger which is not likely to occur except where the lines of structure, cosmic or social, are *clearly defined*" (p. 136; my italics). This position represents the strand of Durkheim's thought which derives from the *Elementary Forms* and argues that a sense of power and potency goes with the presence of a strong corporate order: the gods are sacred, powerful, and dangerous. This whole business gets a bit tricky, as we find ourselves arguing that power, potency, and danger are generated both by the presence of a well-bounded social order and by the presence of social breakdowns, marginal situations, boundary crises, and ill-defined areas. We seem to be arguing that both form and nonform generate the same social experience. It might be that positive rites such as celebrations and ceremonies are intended to reaffirm corporate social reality and that negative rites such as those based on pollution beliefs pertain to boundaries and ill-defined areas. But this hypothesis oversimplifies: there are moral interdictions against touching, seeing, and polluting

sacred religious objects which are collective representations of corporate society, not of the margins of social life.

How then can order and disorder generate the same beliefs? It may be that a strong corporate order is accompanied by ritual and pollution beliefs to reaffirm its collective reality, with the margins of the system having an extraordinary sense of potency and danger because the contrast between form and nonform is so great. Further, given a boundary crisis (the Erikson effect), a strongly bounded system will generate more of a ritual response (manufacturing deviance and dirt) than a society with weaker boundaries and less corporate reality. A weaker corporate order, conversely, will have less ritual and fewer pollution beliefs, as there is less collective reality to renew; and if all its social relations are somewhat ill defined, the presence of ambiguity at the boundaries will be less threatening and hence not emit such a sense of power and danger. Finally, given a boundary crisis, there will be less of a response, as there is less corporate reality to be threatened and ritually reaffirmed.

In her subsequent book, *Natural Symbols*, Douglas attempts to specify further the complex relationship between ritual and society that she suggested so imaginatively in *Purity and Danger*. Ritual, she argues, should be seen as a condensed symbolism communicating collective understandings, not simply a form devoid of content. Ritual is a mode of communication, a language. She now needs to link theoretically the communicative aspect of ritual with the larger social order, and in what is perhaps her most ingenious insight, she notes the similarity of Durkheim's mechanical and organic solidarity and Basil Bernstein's restricted and elaborated codes. The elaborated code "arises in a small-scale, very local social situation in which the speakers all have access to the same fundamental assumptions . . . every utterance is pressed into service to affirm the social order. Speech . . . exercises a solidarity maintaining function closely comparable to religion as Durkheim saw it functioning in primitive society" (p. 22). It seems to me that the importance of the Durkheim-Bernstein connection is that we can now causally link variations in group organization (solidarity) with variations in the structure of symbol systems (elaborated or restricted codes). Language can be seen as a kind of ritual, and where there is more collective reality to reaffirm ritually, a society's symbolic system will take the form of a restricted code. Differences in group solidarity generate differences in a group's symbolic life. Further, the notion of language need not be limited to speech. For example, questions of artistic, musical, literary, or architectural *style* can be seen as variations in elaborated or restricted codes which should vary systematically with the solidarity of the group.

Douglas, though, reverses the issue and views ritual as a kind of language. Her focus shifts to variations in the immediate context of individual speakers

and to questions of socialization and internalization. We are now talking of solidarity in terms of "uniting the speaker to his kin and to his local community" rather than the solidarity of the group as a whole. This is fine for analyzing differences among individual speakers, but it severely limits the analysis of ritual in general. The Meadian conception of language comes through very strongly; in it the cultural-transmission function of language is found in the ability of the individual to act as a carrier of his social structure. Speaking in a restricted code may be a means of "uniting the speaker to his kin," but it is also the performance of a ritual act with direct effects upon the solidarity of the group as a whole, regardless of what it does for the psychological reality of the speaker. Speaking in code is a highly structured and ritualistic act which directly activates shared assumptions and collective understandings and thereby directly reaffirms the substance of the social order. The point here is similar to Goffman's (1956) understanding of deference and demeanor rituals: they shape the identity of interacting participants, but they also have direct effects, as rituals, upon the status structure as a whole.

Another difficulty with the linguistic analogy arises when "small-scale, very local" situations are identified as the origin of restricted codes. This makes comparative analysis extremely difficult, for at the societal level of analysis restricted codes are limited, by and large, to primitive societies. The small-scale, very local situation is simply not a property of modern national societies. Douglas argues that modern industrial society with its "geographical and social mobility . . . detaches [people] from their original community . . . [and] here are the people who live by using elaborated speech to review and revise existing categories of thought. To challenge received ideas is their very bread and butter. They (or should I say we?) practice a professional detachment towards any given pattern of experience" (p. 31). This distinctly modern use of language is contrasted with language in "most primitive cultures in which speech forms are firmly embedded in a stable social structure. The primary use of language is to affirm and embellish the social structure which rests upon unchallengeable metaphysical assumptions" (p. 28). That speech should be used to "review and revise existing categories of thought" in modern society and merely "embellishes social structure which rests upon unchallengeable metaphysical assumptions" in primitive societies seems like the same sort of ethnocentric nonsense she herself debunked in *Purity and Danger*. Her progress made earlier in showing how modern hygienic practices are sociologically similar to those of primitive societies seems to have been completely forgotten when it comes to the wonders of language. No one exists outside his society. Language is used as much for embellishing our social order with its own unchallengeable metaphysical assumptions as that of our primitive brethren.

The final section of *Natural Symbols* presents a scheme for linking different properties of groups with the presence of ritualistic cosmologies. Douglas's most general proposition is that, "when the social group grips its members in tight communal bonds, the religion is ritualist; when this grip is relaxed, ritualism declines. And with this shift in forms, a shift in doctrines appears" (p. 14). She creates two dimensions in which the society can "grip" its members: *group* and *grid*. "Group is obvious—the experience of a bounded social unit. Grid refers to rules which relate one person to others on an ego-centric basis. Grid and group may be found together. In this case the quality of relations is ordered and clearly bounded" (p. viii). The idea of group seems clear enough, evidently referring to the distinctly corporate aspect of collectivities, whereas the idea of grid remains somewhat fuzzy. It appears to refer to all sorts of normative commitments, status obligations, and role reciprocities through which institutions organize and regulate social action. Group and grid are cross-classified, creating a fourfold table. "Three sectors . . . predispose toward ritual in its most magical and concentrated sense. Where grid is strong and group weak, magic is at hand to help the individual in a competitive society . . . where both group and grid are strong, magicality supports the social structure and moral code. Where group is strong and grid weak, magicality protects the borders of the social unit. Only in the area of zero organization are people very uninterested in ritual or magic" (p. 144).

In examining the specifics of this scheme, the problem of focusing upon local situations as the origin of restricted codes, and hence ritual, becomes apparent. In studying cosmologies we are dealing no longer with the local situation of individual speakers but with religious systems in general. The unit of analysis has shifted from individuals to societal collectivities, and not all societies exist on a small scale or at a low level of organization.

This problem is clearly seen in the account of the conditions which generate the witchcraft cosmology: "If we have social units whose external boundaries are clearly marked, whose internal relations are confused, and who persist on a small scale at a low level of organization, then we should look for the active witchcraft type of cosmology" (p. 113). The problem is that, by focusing upon a small scale, a low level of organization, and confused role relations, Douglas has in effect limited herself primarily to primitive societies. This might be all right if witchcraft cosmologies were found only there, but they are not. Modern societies also, existing on a large scale with a high level of organization and well-defined role relations, have been plagued by witchcraft activity in the form of the modern political witch-hunt (Bergesen 1977*a*). Her "four characteristics of the witchcraft cosmology: the idea of the bad outside and the good inside, the inside under attack and in need of protection, human wickedness on a cosmic scale, and these ideas used in political manipulation" (p. 114) are also the characteristics of nations

that find themselves beset by all sorts of subversives. *The* People, *The* Nation, or *The* Revolution is the "good inside . . . under attack and in need of protection" from infiltrators, foreigners, spies, and alien ideas representing the "bad outside" and "human wickedness on a cosmic scale." Charging someone with subversive activity or dangerous thoughts has certainly been used for "political manipulation." Further, the states which seem to have the most active witchcraft cosmologies also have very strong and well-defined state bureaucracies which organize and structure a great deal of daily life. Hardly a situation of confused internal relations; in fact, almost the opposite.

Douglas's reasoning seems to have been that of *Purity and Danger:* confused or ambiguous social relations generate ritual. But she may have focused upon the wrong dimension. Since strongly bounded groups are found in both primitive and modern societies, whereas confused internal relations, small scale, and low level of organization seem largely limited to more primitive societies, it may be that group rather than grid is generating the ritualistic cosmologies. Also, if it is the "gripping" nature of society that generates ritual, the category strong grid–strong group should have the most active witchcraft cosmology; but it does not. As we have just seen, that attribute is generated by strong group coupled with weak grid. Finally, there is some evidence suggesting that strong grid and strong group may be antithetical dimensions and as such probably not found in the same social formations. Studies of highly corporate (strong) groups suggest that they are prone to subvert the power and legitimacy of institutional structures which link individuals (strong grid). For example, the process of nation building has seen the central state undermine extended kinship relations, community or regional ties, and ascriptive associations in general to create a closer relationship between the sovereign state and its individual citizens (Bendix 1964; Tilly 1975; Bergesen 1977*b*). There have also been studies of highly corporate groups such as monastic orders, revolutionary parties (Coser 1974), and utopian communities (Kanter 1968), suggesting a similar hostility between the interests of the corporate body and the extended relations and obligations structuring the interaction of individuals. In effect, it seems that we may have a strong group and a weak grid or a strong grid and a weak group, but probably not both strong group and grid together.

What then of group and grid? I would like to suggest that group and grid can be considered as different positions on an underlying dimension of social organization: corporateness. Ritual carries collective information (the analogy with restricted codes) and thus reaffirms the collective reality of group life. Ritual is performed for the gods or other collective representations which stand for or mirror the collectivity as a corporate entity. Following Swanson (1971), we can consider corporateness as a continuum, with collectivities varying in the extent to which they are organized to realize their distinctly collective interests and purposes. Although group and grid may

in some sense equally "grip" individuals, they are not equivalent dimensions of corporateness. Group, emphasizing the well boundedness of collectivities, represents a more highly corporate order than grid, which pertains to collectivities as aggregations of social interests rather than as unified corporate actors. Grid, then, can be considered an intermediate stage of corporate organization, in which a collectivity expresses its corporateness as a collection of constituent parts rather than a well-bounded whole or strong group, in Douglas's terms. As there is less distinctly corporate reality to reaffirm ritually when a collectivity is an aggregation of parts rather than a corporate whole, there should be less ritual with strong grid than strong group.

Douglas's four group-grid combinations can be rearranged to reflect low to high degrees of corporateness. First, there is weak group–weak grid, which has little social organization of any kind, little corporate reality to reaffirm, and little ritual. Second, there is strong grid–weak group. Here corporateness is expressed as the linking or aggregating of individuals and social interests into a collectivity. There is more ritual than in the first stage but less than in the later stage. Personal magic rather than large-scale public rituals seems more characteristic. "Where grid is strong and group weak, magic is at hand to help the individual in a competitive society. He trusts implicitly his know-how, his private destiny or star, and in the power of the rules . . . the cosmos is morally neutral and basically optimistic . . . [his] world is not controlled by independent ghosts or witches, or evil men. There is no sin: only stupidity" (pp. 144, 137). This is the cosmology of Western societies, in which the individual and his institutions (strong grid) are not totally subordinated to the overarching purposes of the nation as a corporate whole (strong group). As mentioned before, there is reason to believe that the third category, strong grid–strong group, does not exist empirically: even if it does, group and grid are probably not quite equally strong. Finally, there is a fourth category, strong group–weak grid, comprising well-bounded, highly corporate societies. They have the most ritual, as they have the most collective reality to reaffirm periodically. The ritual is more public in character, performed by formal agents of the collectivity as a whole, as in political trials, purges, or rectification campaigns. In sum, strong groups generate the most ritualistic cosmologies, followed by strong grid, with weak group–weak grid generating the least ritual. As the corporate character of the collectivity increases, so does the rate of ritual, as there is more collective reality to be periodically reaffirmed.

Although I have tried to engage many of Douglas's arguments intellectually, all I can think of in closing are the numerous examples, insights, and reinterpretations I have not mentioned—her analysis of ritual dietary rules, for instance. These books are formidable, and anyone claiming the slightest understanding of the complex interrelationship among rituals, symbols, and society must come to grips with her arguments.

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Book Reviews

New Rules of Sociological Method: A Positive Critique of Interpretive Sociologies. By Anthony Giddens. New York: Basic Books, 1977. Pp. 192. $10.95.

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Yes, Virginia, there is a spirit of the sociological imagination alive and, well, alive anyway. Just take a look at how easily Tony Giddens stands on the shoulders of giants by modernizing a title! One can only marvel at and grudgingly envy the temerity of youthful British sociological scholarship. Here, out of Camelot, comes a bold knight who happily flings the gauntlet at the dragon of functionalism by going straight into the lair of the Beast (positivism) armed with miraculous weapons: the shield of post-Wittgenstein philosophy, the mace of contemporary critical theory, and the sludgehammer of ethnomethodology. With such and other weapons at his side, what is there to fear?

Obviously, the brash borrowing of such a hallowed title as *Rules of the Sociological Method,* written by Durkheim to make explicit the rules of procedure which guided his anterior *Division of Labor,* invites a brief comparison. Durkheim composed *Rules* in his mid-thirties; according to informants, Giddens may still be on the fortunate side of 40. The length of the two texts is just about the same, with just about an equivalent number of sections. Durkheim wrote his treatise when sociology lacked methodological articulation and differentiation from other disciplines; his work helped immensely to give sociology a focused scientific identity. Giddens's study comes at a time when sociology is firmly established as an academic discipline yet is in a state of methodological anomie. Not that there is a lack of sophisticated techniques, of course, but rather a lack of an accepted general frame of analysis, a logic of sociological inquiry which could reintegrate the discipline. *New Rules of Sociological Method* may have been conceived with this reintegration in mind.

New Rules does not exactly honor its model. Giddens is quite critical of Durkheim, of the latter's *Rules* (p. 92), and of Durkheim as an inspiration for Parsons's theory of action. These objections form the alpha and the omega of the study. In the introductory chapter he faults Parsons by stating, "*There is no action in Parsons' 'action frame of reference'* . . . is it any wonder that the layman finds it hard to recognize himself in such theories?" (p. 16; italics in original). Never mind that the layman would surely not recognize himself in the theories which are conjured up in place of Parsons. In the concluding chapter Giddens repeats the accusation that

"orthodox functionalism" à la Durkheim and Parsons fails to recognize the member of society as a creative, skilled, reflexive being; moreover, Giddens faults the functionalist stance for not being able to cope with "asymmetries of power and divisions of interest" (p. 157). Well, there is nothing very original in this critique since for a good many years now "humanistic" sociologists have faulted Parsons for his image of idle man, while neo-Marxist ones have faulted him for an image of idyllic society. As a matter of fact, I am not quite sure what is original about this "positive critique of interpretive sociologies," as the book is subtitled. The thrust at Parsons is more of a quixotic gesture than a bold venture these days. If Giddens makes it with such bravado, it is because he wishes to knock aside the Parsonian perspective, which itself developed as a critique of positivism, the latter understood narrowly as the commitment to adopt for the social sciences a logic of inquiry similar to or identical with the one which frames the inquiry of the natural sciences. If positivism won't do, and Parsons won't do, what will Giddens do?

After breathtaking sallies and forays into various contemporary themes, which make Liszt's rhapsodies seem like plodding pieces, our virtuoso crashes the sociological keyboards in the last four pages with his own "rules," which are reproductions of the basic sections of Durkheim's own treatise. The final dominant chord struck reads: "In sum, the primary tasks of sociological analysis are the following: (1) the hermeneutic explication and mediation of divergent forms of life within the descriptive metalanguages of social science; (2) explication of the production and reproduction of society as the accomplished outcome of human agency" (p. 162). If I can offer my own hermeneutic explication of the above Delphic pronouncement, it boils down to saying that social life must be accounted for in terms of both its subjective and objective dimensions, that is, in terms of the many intersubjective meanings of its actors and in terms of the social structures which mediate their actions. If it means something else, I regret having missed the point, but if that is what is intended, what else is new? I fail to see how Giddens adds anything to the sociology of knowledge perspective advanced in Berger and Luckmann's *The Social Construction of Reality* (oddly enough, though he invokes Schutz as a major formulator of contemporary interpretive sociology, Giddens makes no mention of Berger and Luckmann, or of Natanson, all three prominent students of Schutz). For that matter, one can argue that the grand finale of *New Rules* adds little to earlier methodological perspectives not treated by Giddens, such as those of Sorokin, McIver, Znaniecki, and even several of the Durkheimians (in particular Halbwachs).

Similarly when much ado is made in introducing as a novelty the notion of "structuration" as "the true explanatory locus of structural analysis" (p. 120)—that is, when Giddens seeks to complement the "subject-less" notion of "structure" with an "active process, accomplished by, and consisting in, the doings of active subjects" (p. 121)—he is less original than

he thinks. I have treated the theme of structuration and destructuration in my essay in the McKinney and Tiryakian volume, *Theoretical Sociology* (New York: Appleton-Century-Crofts, 1970), and I am the first to credit Georges Gurvitch (another unfortunate omission from *New Rules*) with reconceptualizing the notion of social structure in a dynamic sense. Incidentally, since Gurvitch formulated a thorough critique of positivism, worked his way through both phenomenological sociology and Marxist analysis, and grappled with the basic sociological antinomy between freedom of human action and determinism of social structures—all major themes quite germane to Giddens's concerns—one can only hope that Giddens will discover him in the near future. I am also puzzled as to why Giddens, while giving so much attention to philosophers of language, does not seem to find the occasion to discuss his own British social anthropology colleagues who are doing such interesting studies pertaining to language, rules of conduct, meanings, and social structure—I have in mind Rodney Needham, Mary Douglas, and Basil Bernstein, among others.

Of course, there is a considerable amount of the new sandwiched between the beginning and the end of *New Rules*. One can only respect and admire Giddens for being one of the few Anglophone sociologists who is so au courant on an extremely varied intercontinental literature: American ethnomethodology, Anglo-American post-Wittgenstein linguistic philosophy, German critical realism and German post-Heidegger philosophy, French structuralism, post-Popper philosophy of science. He takes us on a whirlwind tour of contemporary ontological and epistemological perspectives, dropping a cornucopia of names from Apel to Ziff that emulates the spray-painting technique of Jackson Pollack. At the end of the ride, one is not sure whether one has been on a tour de force or a tour de farce: the work reads like a dazzlingly written comprehensive examination which leaves the examiner unsure whether the questions have really been answered, albeit so many right names and chic expressions have cropped up. "Hermeneutic," for example, is an "in" term in smart phenomenological circles, like "reproduction" in Marxist ones, and they are both used liberally by the author.

The extensive splicing and grafting of terms and concepts suggest that Giddens may have in mind the formulation of a grand methodology, but in lifting them out of their respective theoretical contexts he also runs the risk of producing conceptual ambiguity instead of a sound basis for a future program for sociological research. Thus, the very Marxist themes of "praxis," "production," "reproduction," and even "power" seem to become quite ethereal and abstract as they are molded into such bland statements as: "Every act which contributes to the reproduction of a structure is also an act of production, a novel enterprise, and as such may initiate change by altering that structure at the same time as it reproduces it . . ." (p. 128). In spite of Giddens's overt flirtation with Marxism and existential phenomenology, I do not sense how the *New Rules* develops a logic of inquiry for dealing either with historically con-

flicting social forces, which are central to Marxism, or with the incarnation of human consciousness and its various affective modalities, which are crucial to existential phenomenology.

I am also bothered by the feeling that Giddens does not really explicate or reflect very long on very important philosophical issues: he dashes off statements such as, "Gadamer argues that hermeneutics is 'a discipline which guarantees truth.' But this means that truth inheres in being, *the fundamental error of existentialist* phenomenology, and one not rescued by Gadamer's appeal to dialectics" (p. 62; my italics). On what grounds and from what perspective is this a fundamental error? That dismissal is similar to his cavalier dismissal of the notion of function: "I shall make no attempt to trace through the career of the concept of function . . . since I propose to abandon the notion completely" (p. 120).

Of course, boldness is a quality of Giddens which makes his analysis provocative, but not every provocative collage is an enduring work of art. The old *Rules* was the second of only four books which Durkheim published in his lifetime, but it lasted like McGuffey's eclectic readers in teaching social scientists how to read the social world lucidly. The *New Rules* is obviously the production (or should I say reproduction?) of a talented mind, but one that I find more of a pastiche than a magisterial revamping of methodology. Too many strands that don't match are woven together hurriedly, leaving the reader with a blurry impression of the juxtaposition. The *New Rules* hints at forthcoming works designed to spell out a research program for sociology (p. 159). I only hope that now that the dragon is slain and the damsel is sighted in the dungeon, our author will pause long enough to reflect on one of Pareto's favorite rules, *chi va piano va sano e va lontano.*

Durkheim's "Suicide": A Classic Analyzed. By Whitney Pope. Chicago: University of Chicago Press, 1976. Pp. ix+229. $13.00.

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Eighty years after its publication, Durkheim's study of suicide appears beset by damaging design flaws. His data base was vast, but impossibly heterogenous in detail and uneven in reliability. Durkheim intended primarily to identify the determinants of three modalities of suicide (altruistic, egoistic, anomic); but as the data did not differentiate among those modalities, they had to be *postulated* as distinctive, which made his argument circular. He conceptualized the determinants as variables, yet his indicators were mainly attributes; and to impute effects to these Durkheim often had to treat information on aggregates as if it applied to individual units. His theory did not always lead one to expect sizable differences in the suicide rates of different groups; in fact, Durkheim postu-

lated a curvilinear relationship between his key variables (integration, regulation) and the suicide rate, so that groups both high and low on each might in principle exhibit the same suicide rate; yet, to give empirical bite to his argument, he made it revolve largely around differential rates. Another reason for emphasizing these was that, according to Durkheim, stable and sizable differentials between groups somehow established the distinctively social nature of the "forces" causing and maintaining rates; yet he focused his attention on the relatively small differentials obtaining, say, between widowers with and widowers without children, while leaving unaccounted for much larger differentials between the sexes, age groups, or national populations. Finally, Durkheim's theoretical reasoning, while clearly his strong suit, was not *that* strong: for instance, his distinction between integration and regulation was conceptually shaky.

These and other difficulties with the design of the study reported in *Suicide* are reviewed or independently argued by Whitney Pope, mostly in part 1 (pp. 9–60) of his fine monograph. Part 2 (pp. 61–152) moves on to a much more novel, systematic assessment of the extent to which the book's data, as analyzed by Durkheim or reanalyzed by Pope himself, empirically support its theoretical argument. His conclusions on "the theory-data fit" (p. 98) are mostly negative. Time and again Durkheim is shown overstating the empirical grounds for his propositions; evading or casually disposing of uncomfortable evidence; arranging hypotheses in such a way as to make them unfalsifiable; failing to consider alternative interpretations; covering gaps in the data through doubtful assumptions.

In very few cases, if any, is it a matter of Durkheim's data establishing the opposite of what he thought they did; mostly, Pope pronounces verdicts of "not proven." For example: "Modern sociology has long believed that the social suicide rate is higher for Protestants than for Catholics and higher for Catholics than for Jews. Insofar as this idea derives from *Suicide,* it is not empirically well founded" (p. 77). Or: on pages 126–27 Pope lists six generalizations Durkheim proposed on the relation between "chronic domestic anomie" and the suicide rate; he then shows that all suffer from "serious difficulties" (p. 140) in their relationship to the data.

Even when, in part 3 (pp. 153–200) Pope assesses *Suicide*'s argument that there exists an emergent, distinctively social level of reality on which sociological discourse should be grounded, we find that this "case for social realism" receives inadequate support from Durkheim's attempted elimination of psychologistic and naturalistic explanations of differential suicide rates.

The upshot of the whole argument is best phrased by Pope himself: "What has hitherto been widely considered one of *Suicide*'s great strengths —the way in which Durkheim marshalled confirmatory empirical evidence—must now be judged a major weakness" (p. 201). While it still constitutes the best test of Durkheim's theory of suicide, it is not satisfactory.

Throughout, Pope's argument is fair, sophisticated, and lucid (though often demanding). Only a few criticisms are worth mentioning. In my view Pope has conducted an all-too-exclusively "internal" analysis of *Suicide* (and of the directly related literature, among which he seems to have missed only one item of significance: J. Madge, *Origins of Scientific Sociology* [London: Tavistock, 1963], chap. 2). Not even the protracted and laborious genesis of the book itself is discussed. And Pope's remarkable Durkheimian scholarship might have been put to use following up in other works his point that Durkheim remained ever convinced of the analytical priority of morphological variables (p. 110); or his provoking remark that "though everywhere using the term and addressing himself to its basic nature, Durkheim (somewhat curiously) never defined society" (p. 191).

Finally, while I agree with Pope that, for all its (mostly newly revealed) weaknesses, *Suicide* thoroughly deserves its standing as a "classic," I think he should have inquired *why* Durkheim, and many others after him, wildly overestimated the book's empirical strength. But perhaps the question is not well posed, inasmuch as the standards by which empirical support is evaluated legitimately vary in time; or, perhaps, sociological discourse is by its very nature persuasive rather than apodictic in nature, and to that extent it appropriately employs and assesses even statistical information and logical reasoning as (at bottom) rhetorical devices.

Enlightenment and Despair: History of Sociology. By Geoffrey Hawthorn. New York and London: Cambridge University Press, 1976. Pp. 295. $17.95 (cloth); $5.95 (paper).

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"This is not a scholarly book," acknowledges the author, who teaches sociology at Cambridge, on his first page. I heartily agree; in fact, it is a most unscholarly one. But aren't Cambridge dons supposed to do scholarly work, and aren't university presses subsidized because they are meant to publish works of scholarship?

The subtitle of the volume is misleading. This is not a history of sociology. It is a highly selective account of the work of some sociologists as seen through the eyes of a British author—a provincial one at that—who, inter alia, spends seven pages on Leonard T. Hobhouse and nine lines on George Simmel and devotes a page to Anthony Crossland while failing to mention even once Robert K. Merton.

Hawthorn states that he wishes to eschew reductionist approaches to the history of ideas, be they Marxian, Hegelian, Durkheimian, Mannheimian, or Kuhnian, and to follow instead the guidelines suggested by Collingwood, Quentin Skinner, and others who argue that it is the business of the historian of ideas to "recover the intentions" of the authors he is

discussing. Historians must ask "what any past thinker thought he was right about, and what his reasons were for thinking he was" (p. 6). So far so good. But at the same time the author stresses in his introduction that ". . . it is difficult to understand the history of social theorizing in western Europe and the United States since the eighteenth century without understanding the extent to which it has been a failure" (p. 7). Unfortunately, Hawthorn's prepotent desire to document the failure of sociology distorts his attempt to understand the ideas and authors he wishes to examine. In addition, alas, he just does not have the intellectual equipment to undertake the tasks he sets for himself, so that he succumbs to an embarrassing number of factual errors.

The book consists of 10 chapters. The first two, on the Enlightenment and on German idealism, are written on the level of elementary textbooks of philosophy. The remaining eight chapters purport to deal in 200 pages with the history of European and American sociology from Comte to the present. These chapters exhibit a condescension and disdain for seminal thinkers on the author's part that surpass anything in that vein that I have come across. Marx "argued from premises that were either incoherent or false" (p. 64). Spencer was "very confused" (p. 97). Durkheim's general argument was a "failure on all levels," and his thesis in *The Division of Labor* was "extraordinarily poor, both in its logic and in its purported empirical 'proof'" (pp. 123–24). And so it goes throughout the book: each author's intentions are explicated or rather caricatured, and it is then shown to Hawthorn's satisfaction that each and all of them failed miserably to present a coherent and tenable sociological vision. All these sociological systems, Hawthorn gleefully "demonstrates," were logically deficient and empirically false. The history of sociology, it turns out, is a graveyard of inept theories and ungrounded assertions. All of it, in Hawthorn's dyspeptic vision, is but error, falsehood, and vanity. The sociological enterprise has no legitimacy because it is founded on the shifting sands of false prophecy and fraudulent claims.

Hawthorn is of course entitled to engage in a wholesale attempt at slaying our ancestors. But his knowledge of them and their works is so deficient that instead of harming them he only manages to kill straw men.

ITEM: Hawthorn asserts that American social theorists as a whole had no firm grasp "of what sociologists would now call 'social structure'" (p. 193), and as a consequence, Cooley, for example, "paid virtually no attention to institutions" (p. 208). The fact is that Cooley is usually taken, together with Veblen, to be the father of institutional economics. Moreover, part 5 of his *Social Organization,* comprising some 80 pages, is called "Social Institutions."

ITEM: The early fathers of American sociology, Hawthorn asserts, "began from something approaching a state of nature and in general proceeded to account for the development of never very clearly defined social entities in terms of a cumulative sense of association between initially freely acting individuals" (p. 125). In fact, almost all American sociolo-

gists of the early generation were deeply influenced by Comte, or Spencer, or German historical scholarship; as a consequence, none of them subscribed to the nonsensical ideas Hawthorn attributes to them.

ITEM: "American social theory has been an attempt to secure a coherent liberalism on the basis of nothing at all" (p. 216). No comment.

ITEM: "The very conditions which have fuelled the students' radicalism had led the American working class overwhelmingly to vote for Richard Nixon" (p. 245). No comment.

ITEM: "Mannheim . . . had studied in Germany under Simmel and the Weber brothers . . . before returning to Budapest at the outbreak of war" (p. 181). Mannheim did not study under Max Weber for the simple reason that Max Weber did not teach in the years before World War I. Mannheim studied with Max's brother Alfred *after* the war.

ITEM: "[Scheler] distinguished three kinds of knowledge. The first was that understood in the west as 'scientific.' . . . This man shared with the animals" (p. 182). No comment.

ITEM: "Toennies . . . came from a provincial agricultural background remote from Prussia . . ." (p. 89). Toennies came from Slesvig, which was part of Prussia.

ITEM: ["The Reichstag] caused Bismarck's resignation . . ." (p. 140). It had no such powers, and this is precisely why Germany was not a parliamentary democracy before 1918. The Kaiser dismissed Bismarck.

ITEM: "Harriet Martineau . . . translated both [Comte's] *Course* and the *System* into English" (p. 79). Martineau published an abridgment of the *Course* and had nothing to do with the translation of the *System.*

ITEM: ". . . the ultramontane reactionaries like de Bonald and de Maistre who, *pace* Nisbet, were despised by Comte . . ." (p. 254). *Pace* Hawthorn, Comte had deep admiration for both. He refers, for example, to "the most eminent thinker of the modern Catholic school, the illustrious de Maistre," in the forty-sixth lecture of the *Course.*

Why continue? So much dribble pours from the presses these days that it seems almost pointless to concentrate on the shortcomings of just one volume. Yet it would seem important, be it only for reasons of intellectual hygiene, to raise a warning signal when shoddy goods appear on the market of ideas.

Far be it from me to advocate a kind of filial piety when it comes to the scholars on whose shoulders we all stand. We can pay them no greater tribute than to submit their ideas to relentless critical scrutiny; only in this way can an intellectual discipline develop and advance. But a precondition for such critical attempts to deal with the heritage of the past is to take it seriously and in a spirit of respect for its achievements.

Hawthorn, it would seem, lacks what Else Frenkel-Brunswick has aptly called tolerance for ambiguity. His is an either-or universe in which anybody who doesn't measure up to his exalted standards is cast into utter darkness. But such an attitude, I would suggest, is of no help when it comes to the uses of history for any academic discipline. Even the greatest

theorists, to be sure, have said many things that it would be foolish to defend today; but they have also made seminal contributions that still are indispensable for those of us who wish to work on the elucidation of human predicaments in our society. One may well agree that it is the task of the historian of knowledge to "recover the intentions" of the scholars under discussion, provided it is also recognized that for the practicing sociologist it is of the utmost importance to recognize what in the heritage of the past is still of contemporary significance. Through ongoing critical reading of the masters of the past, we must appropriate for our own uses what is still valid in our inheritance.

Hawthorn's condescending, sneering, disdainful approach to scholars who are immensely superior to him in their analytical capacities can only result in a kind of nihilistic antinomianism. This may go over well in introductory lectures for prematurely jaded Cambridge undergraduates, but it does not behoove intellectual historians. If, in addition, as in this book, a patronizing tone is joined to sloppy scholarship, one can only suggest that its use is likely to be detrimental to intellectual health.

Skeptical Sociology. By Dennis Wrong. New York: Columbia University Press, 1976. Pp. viii+322. $14.95.

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It is difficult to read these essays without being reminded of the diversity and contrasting styles which continue to characterize American sociology. It will simply not do to aver piously that empiricists and theorists, "hard" and "soft" sociologists, functionalists and Marxists, survey researchers and bibliophiles, arcane ethnomethodologists and those who compare entire societies, radicals exposing the iniquities of American society and those pursuing policy research (to make it work better)—that these and many other types are really all trying to do the same thing in their own way. Perhaps they are doing so, but if so, in a very abstract and barely perceptible way. I happen to believe that sociology remains a highly unintegrated, inherently diffuse discipline that lends itself to people "doing their own thing." In particular the consequential distinction is by no means obliterated between those who collect what they consider scientifically respectable "data" (or ingest what others have collected), pertaining to narrowly defined aspects of present-day life in the United States on the one hand, and those who reflect and write about major, perennial, or recurring issues of social existence dispensing with elaborate methodological devices and techniques on the other. In short, I believe that the archetypes of American sociological practices satirized by C. Wright Mills are still with us although they were neither exhaustive nor totally fair descriptions of the schools or trends in American sociology.

How is one to characterize the brand of sociology Dennis Wrong has been engaged in and which is so appealingly presented in this volume? The term proposed by him, "skeptical," does not quite do justice to it although it helps to differentiate it from other approaches or styles. Certainly neither the grand system builders, hoping to encompass all the laws, verities, and manifestations of social life in their theories, nor the dedicated data gatherers and manipulators entertaining heady visions of being scientists, nor the relentlessly committed indignant social critics can be accused of skepticism. Dennis Wrong's sociology is skeptical all right, but there is more to it. I am tempted to call it "intellectual sociology"—a term which unfortunately (but correctly) implies that not all sociology is intellectual. Wrong's sociology is intellectual in the best sense of the term since "intellectual," most would agree, entails skepticism, capacity for criticism, a concern with social justice and free expression, inquisitiveness, interest in ideas, in "big questions" and the core values and assumptions of society. Not all sociologists are intellectuals in this most flattering sense of the word; nor does it follow however that sneering at more humble mental tasks and activities and more narrowly defined professional concerns automatically transforms somebody into an intellectual. Dennis Wrong wisely disclaims the "humanistic" label since, as he puts it, " 'humanistic' has acquired a certain self congratulatory aura, a 'more-caring-about-people-than-thou' flavor" (p. 2). He does not claim the mantle of "critical" sociology either, partly because of the specific historical connotations of the term (Frankfurt School, revisionist Marxism), and partly because on closer inspection "critical" sociology often turns out to be quite uncritical or merely selectively critical.

Perhaps the manner in which a particular sociologist is characterized or classified has something to do with whether or not he feels affinity with literary (and literate) intellectuals or the world of science. In this context it is certainly significant that Wrong can also be characterized as one of a handful of American sociologists who minimize jargon and do not abuse the language. Lucidity is an interesting and important bond between such disparate figures as Wrong, Barrington Moore, Shils, C. Wright Mills, Nisbet, and a few others. Perhaps, after all, *how* people write has some bearing on *what* they say and what they find of professional interest.

It is obviously difficult to summarize a volume of 17 essays written over a period of close to 20 years. The author divides them into three parts: "Human Nature and the Perspective of Sociology," "Social Stratification and Inequality," and "Power and Politics." But these titles barely reflect the diversity of topics they accommodate. They include human nature, community, identity, ethnicity, economic development, Max Weber, the future, and the main concerns of political sociology (i.e., concepts such as power, force, authority, legitimation, electoral politics, etc.). These essays are wide ranging, somewhat polemical, somewhat philosophical, and many have an enduring, timeless quality best exemplified by the continued

impact (and relevance) of the critique of the oversocialized conception of man.

Many of these essays, and the more recent additions to them, represent a stocktaking of what happened in American sociology since the 1950s. They revive many of the debates, controversies, and preoccupations of times past. (Few, if any, of them have been resolved; rather, they have been by-passed, forgotten, and superseded by new concerns.) Dennis Wrong participated in and thoughtfully commented on several of them: On the functional necessity of stratification, on the Parsonian versus Weberian (or Marxist) views of American society, on the relationship between economic development and political democracy, and, of course, on the conceptions of human nature implicit in structural-functional theory. One of the ironies associated with the original critique of the oversocialized conception of man (or, of man, the acceptance seeker) is that when Wrong mounted his original attack (in 1961) it was rightfully aimed at Parsons and his followers (seen by the radicals as establishment sociologists), whereas in more recent times many sociologists unsympathetic to Parsons and to established institutions in American society have taken over the emphasis on man as the acceptance seeker while stressing and exaggerating the crucial part played by expectations in determining behavior—a major theme of the labeling theorists and kindred educational reformers-innovators. To be sure this perspective is usually applied exclusively to disadvantaged, or underdog, groups and sees them as helplessly in the grip of circumstances and situations (which include expectations). In any event, labeling theorists use a conception of human nature that has much in common with that of the oversocialized conception of man—a point Wrong does not develop.

This lucid and sophisticated volume makes the reader feel that the author has not forgotten the questions which prompted him to become a sociologist.

Sociology as an Art Form. By Robert Nisbet. New York: Oxford University Press, 1976. Pp. 145. $8.95 (cloth); $2.50 (paper).

John O'Neill
York University

Nisbet's book is nicely written. Indeed, it should be. This is the third time he has written the same book. In arguing today for the comparison between the practice of art and the practice of some classical sociological writers, Nisbet has forgotten that he began long ago with a literary version of the sociological tradition that makes it easy to find in it what he has already put there. Nisbet believes that much classical sociological

writing makes use of the categories of landscape, portrait, motion, anc metaphor. He takes these ideas from art and literature as though the were unproblematic and quite ignores contemporary art and literatur where they would be hard to find at all. He poses no questions for him self in reading the works of Marx, Weber, and Tocqueville as though the were essentially literary classics. He never attempts anything like a seri ous and welcome literary analysis of these texts. Nor does he conside rival readings such as those of Parsons or Aron which treat the sociologi cal tradition as stages toward an analytic sociology and political sociolog which can be abstracted from its literary and artistic settings.

Nisbet intends his book to be a defense of scientific imagination i opposition to the scientistic reduction of creative thought to routine meth od. In practice he is an ornamental writer, pleasing to the sociologica establishment he feigns to attack and quite unidentified with any of th practitioners of "creative" sociology. Nisbet's claim that sociology is a art form will not disturb the normal practice of sociology. To any on with a serious approach to contemporary art and literature, Nisbet' account is totally inadequate as an exemplification of the claim it makes At a time when the hermeneutics of reading and writing has become complex practice, Nisbet's essay appears sophomoric.

On Critical Theory. Edited by John O'Neill. New York: Seabury Press 1976. Pp. 263. $12.95.

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Ramapo College

John O'Neill prefaces his collection with the statement that he intend to foster the rebirth of critical theory. To that end he has assembled 1 essays that basically discuss the work of the Frankfurt School, includin the implications of Jürgen Habermas's "linguistic turn" in critical theory O'Neill's introduction is an explicit critique of Habermas's theory, whic also attempts to promote an alternative program of critical social phenom enology. O'Neill claims that his approach does not separate cognition an sensuous activity and is more capable of guiding socialist pedagogy. How ever, the collection he has assembled does not wholly support this con clusion but rather indicates the ongoing split within critical theory be tween the emancipatory ideals of rational self-reflection and aestheti experience.

Albrecht Wellmer's precise and comprehensive essay traces the origin o critical theory from the recognition of an implicit reductionist tendenc of Marx's philosophy of labor. Whereas Marx's notion of practical-critica activity was both an anthropological and epistemological category (on

that implied that man was both a tool-making and a symbolizing animal), in his philosophy of labor Marx subordinates the symbolic function to the productive one in order to save the primacy of material production. Hence the instrumental "interaction" of man and environment becomes the paradigm of sensuous activity in general. Thus an inherent reductionistic tendency was established that permitted the regression of Marxist epistemology and anthropology behind the active moment of idealism. This had the practical consequence of failing to prohibit the degeneration of socialist practice to technocratism and Stalinism. Without an adequate unification of the truth of both materialism and idealism, as the first thesis on Feuerbach indicates, Marx's program for a consistent naturalism or humanism is unrealizable. That is, without the categorical capacity to objectify the authoritarian domination of technical social control, critical theory cannot indicate how postliberal ideology (late capitalism and state socialism) severs the legitimation of just social forms of life from any normative regulation of interaction. The Frankfurt School's critique of "instrumental reason" was thus a recognition of the repression of ethics as a category of social life and the resulting depolitization of social reality.

Successful reconceptualization of Marxist critique was not achieved by either the Hegelian-Marxists of the 1920s (Lukacs, Korsch, Gramsci), nor was it realized by the first generation of the Frankfurt School (Horkheimer, Adorno, Marcuse) (as Paul Piccone's essay demonstrates). Christian Lenhardt's analysis of Horkheimer and Adorno's *Dialectic of Enlightenment* indicates how its "indeterminate negation" is a loss of historical continuity which turns the ideal of emancipation into a "gnostic" category. However, Jeremy Shapiro's imaginative analysis of "embeddedness in Nature" indicates the missing cultural moment of critique:

> Homo sapiens [also] evolves through the cultural transmission of information from one generation to the next by means of socialization into cultural systems, and these systems themselves learn, evolve, and become reflexive as the learning mechanisms enter awareness and become governable. . . . This would lead to the institutionalization of discourse, so that all claims to validity in human communication, including ethical and political norms, would be subject to systematic, as opposed to haphazard, rational examination; the establishment of political, social, and economic democracy as the foundation of the institutionalization of discourse; and an increase in personal autonomy within relations of increased interdependence, to guarantee the human capacity to participate in the two prior conditions." [Pp. 156–57]

In order to critique the distortion of authentic consensus by the undiscussed institutionalization of coercive technical controls, critical theory would have to reconstruct the evolution of the human species both in the dimension of the productive transformations of mankind's embeddedness in nature *and* in the reciprocal mediation of technical reproduction by

symbolically constituted truth claims of our social forms of life. To complete Marx's unfinished project of a unified naturalism it would be necessary to achieve a materialist "phenomenology of mind." As the excellent essays by Friedrich W. Sixel and Dieter Misgeld indicate, Habermas's attempt to realize this theoretical project has been advanced by an immanent critique of the systems theory of Niklas Luhmann and the hermeneutic philosophy of Hans-George Gadamer which retains significant elements of these contemporary restatements of materialism and idealism. However, that this highly analytic and forbiddingly abstract project does not persuade is indicated in the essay by H. T. Wilson, who traces Habermas's reformulation to Max Weber while arguing that science is neutral and does not become a social technology.

In contrast to the Habermasian project, Ben Agger asserts that critical theory can be reformulated only by refusing to concentrate on its philosophical inheritance and should instead follow the direction established by Herbert Marcuse and John O'Neill's cultural practice of a new sensibility. O'Neill claims that the nature of human solidarity is the appropriate focus for critical historical remembrance and utopian thought. He believes that Habermas's separation of cognitive and sensory interests is a "risk" that could be co-opted and that responsible socialist practice should be engaged in the education of the oppressed so that the tendency to identify with the oppressor can be overcome and the political relations between science, common sense, and values restored. For O'Neill, critical theory must deliver competent ethnographies of the practical exigencies of daily living in the modern world and only after a long period of such work climb once more to the heights of theory.

On Critical Theory thus fosters at least two forms of critical theory: an attempt to realize in contemporary terms a materialist phenomenology of mind on the one hand and an aesthetics of embodied solidarity on the other. These alternatives are, however, not really joined in discourse since they hardly recognize each other's ideal of emancipation. For example, Habermas asserts that the only rational criterion left in modernity is the universalization of truth claims in discourse; therefore, the rational reconstruction of social evolution is needed as a critical social science that can promote a further institutionalization of rational discourse and emancipatory self-reflection. O'Neill asserts that only the restoration of the historical memory of guilt and unredeemed suffering will provide the ground for an ethical materialism essential for the potential aesthetic unification of art and politics. Because of this opposition, Sherry M. Weber's essay, "Aesthetic Experience and Self-Reflection as Emancipatory Processes," may be the key essay of the collection in that it records the history of critical theories as *continuing* vacillation between these two models. Unfortunately, *On Critical Theory* adds very little to the clarification of the relationship between these two ongoing foci of emancipatory interest. However, it contains much that is of historical and philosophic interest.

Introduction to the Sociology of Music. By Theodor W. Adorno. Translated from the German by E. B. Ashton. New York: Seabury Press, 1976. Pp. xiv+233. $12.95.

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University of Missouri at St. Louis

This is an admirable translation of the second (1968) edition of the difficult 1962 German text which was based on a series of lectures delivered at Frankfurt University in 1961–62. Major portions of these lectures were broadcast over the North German Radio. T. W. Adorno states in his introduction that he intended to reach an audience through this book who "would flinch from more demanding texts" (p. vii). The book was to serve as an introduction to both musical sociology and the sociological conception of the Frankfurt School. Insofar as the author may easily be considered the most qualified spokesperson on musical matters of the Frankfurt School, this last objective is accomplished with authority. Adorno is less successful in providing a lucid text. As far as accessibility of the text is concerned, I did not find these lectures any less demanding than any other Adorno text. Fortunately, ease of access has been increased through the meticulous accomplishments of E. B. Ashton's translation. As far as providing an introduction to the sociology of music goes, omissions and selective emphases clearly identify its coverage as a partisan version of social philosophy at best.

In reading these 1961 lectures it will be helpful to remember that at that period in history only a small segment of the German population attended university. The university students were drawn from the 5 percent of the appropriate age group who graduated from German secondary schools. University students and professors represented a select circle. It would appear to be a reasonable assumption that there was a fair degree of homogeneity among Adorno's students in sharing musical experiences and attitudes characteristic of the German academic elite. Adorno's musical examples and his treatment of musical life typically reflect the sophistication of such a formally educated population.

The table of contents lists chapters dealing with institutionalized musical life. There are such substantive and analytic topics as: "Opera," "Chamber Music," "Conductor and Orchestra," "Avant-Garde," "Popular Music" (treated with condescension), "Types of Musical Conduct," "Function," "Musical Life," "Nations," "Mediation," and "Classes and Strata." Even this last chapter does not offer an introduction to or a summary of well-established research data on vernacular, folk, regional, ethnic, or work music, or discussions of musicians' organizations and unions, or data on the social placement of audiences without which music making

would take place in a social void. Adorno writes that "inquiries int
about the class aspect" (p. 56). Most of the chapter is instead de
to discussions of why Richard Strauss should be associated with such
social distribution and preferences of musical consumption tell us
as "heavy industry," "imperialism," "grand bourgeoisie;" why Felix
delssohn's compositions are slick, Chopin's music aristocratic, or
Johannes Brahms bears the mark of bourgeois society's individua
phase. After these discussions Adorno concludes toward the end o
chapter that "music has something to do with classes insofar as it re
the class relationships in toto" (p. 69). Unless the reader shar
Adorno's mostly implicit approach to the evaluation of the works of
composers he will find little guidance in how sociologists should pr
in empirically establishing criteria for these relationships.

In other chapters, Adorno's exposition is similarly restricted to m
examples or composers known from standard programs of routinized
cert life. Equally standardized concepts and approaches of academic
cology are employed in his analyses without his pausing to explain
these analytic techniques are adopted rather than others (such as
of ethnomusicology). Adorno consistently stays with these 19th-ce
modes of analysis even when he deals with contemporary music i
chapter, "Avant-Garde." That new cultural emanations might requir
ferent modes of analysis in keeping with different sound technologies,
of composition, and placement in social life, that an empirical soci
might need to develop methodological approaches appropriate to an
quate description of its data, most definitely is *not* the message cont
in these pages. Adorno abhors sociological and musical pluralism: "I
zither player and Bach have the same right, if individual taste is the
criterion, great music is deprived of the only thing that makes it grea
valid" (p. 120). What makes for artistic validity, however, Adorno's
leaves rather up to Adorno's personal preferences. And even less aes
cally shrouded topics, such as why it is that the New Music of the
is publicly received with less scandal than New Music of 40 years ea
are explained in personal terms: "Philistines gloat to see that eve
nonphilistines are philistines" (p. 183).

Throughout the book Adorno leads his readers through topics and
tions basic to the sociological study of music. Empirical sociolog
findings and methods, receive little attention and do not inform Adc
posture. As a list of topics to be treated by a sociology of music a
a theoretical attitude which could itself be the subject of sociological
ysis, Adorno's text serves as an intellectual challenge. To regard its
theory as an example of sociology as practiced today in Germany or
anywhere else is a mistake.

Social Exchange Theory. By J. K. Chadwick-Jones. New York: Academic Press, 1976. Pp. viii+431. $29.25.

Thomas Spence Smith
University of Rochester

A neighbor of mine admits quite freely to engaging in social exchanges. He owns houses and collects rents, and the economic theory of how he earns his living has become a way of thinking for him about what he does socially. When I learned this, it came as no surprise to discover that he is also a bird-watcher. I have observed him watching rock doves land under his seed feeder to cull the husks for a reward left behind by sloppy eaters among the finches and cardinals. Occasionally, a very rakish cat of my acquaintance will mouse around the corner, and the birds will take wing, scolding from the lilacs. The cat, who is given to taking his meals alfresco, will preen indifferently among the falling whistles, relaxed as electricity. The few successes he has had—indulged town cat that he is—are enough to bring him back when the birds least expect it. "Intermittent reinforcement," says my neighbor, who out of necessity has turned cat psychologist on top of being a practitioner of social exchanges.

There was a time in the sixties when social psychologists got to thinking along some of the same lines as my neighbor. Cats and pigeons and the experience of the market fell together for them in a pattern of proliferating analogies, predicating on one another in such a way as to persuade the city dweller of the truths in totemism. By way of the simplicities occasioned in thinking of social learning as a matter of stimulus-response psychology, the dynamics of bureauracy seemed for a time diminishable to models founded on the emissions of pigeons. People whose names my neighbor had never heard of—George Homans, John Thibaut and Harold Kelley, Peter Blau—began to publish influential books that all sought, while disagreeing over the fundamental relevance of pigeons, to expand the metaphor of the marketplace to human behavior, and even to support their claims by extrapolating from psychologists' observations on laboratory animals to observations on caged humans—office workers, subjects in Prisoner's Dilemma games, romantic dyads. Powerful books, they reached wide audiences, stimulated much discussion, and formed the basis for experimental research that has continued into the seventies. Enough of this derivative work had accumulated by 1975 or so, and sufficient measure had been taken of the major contributions to the theory of social exchange, to have persuaded J. K. Chadwick-Jones, in 1976, to publish a book which promised us a summary and critique of the views of the theory's major proponents and an assessment of where its influence will be felt in the next 10 years.

My neighbor holds the view that a person's opinion of the bird-cat problem is a fair indication of his philosophy of man. Chadwick-Jones

probably wouldn't hold with this, but he does devote a good part of his book to the analysis of how differing assumptions made by Thibaut and Kelley, Homans, and Blau—assumptions, for instance, about the relevance of pigeon psychology to human affairs—affect how they work up their theories and evaluate data. There is some benefit to the close juxtaposition of the ideas of these major contributors to exchange theory, especially when a common critical vocabulary is leveled against each, their assumptions are exposed, and the research indebted to them is discussed. That is what Chadwick-Jones attempts to do—to frame it all in a way that will allow us to know what sort of animals social exchange theory is talking about.

I am a cat man myself, so I appreciate (not to say agree with) the rather more feline sinuosities in Peter Blau's way of looking at things than I do the utter reductionisms in Homans or in Thibaut and Kelley. I also admit to having held this mild prejudice before reading Chadwick-Jones, though I suspect him of being a pigeon man himself. I have this suspicion because much of his book is poorly edited and badly written, and it is well known that pigeons are a redundant lot with little sense of the language. Bad sentences, obtuse paraphrasing (e.g., Homans's first proposition, pigeon-style: "If, in a particular situation activity has been rewarded in the past, then the more similar is a present stimulus-situation the more likely is the activity to recur" [p. 159]), sloppy punctuation, unnecessary redundancies—all detract from the usefulness of the book. For one thing, poor writing makes it unsuitable as a book for students. It is a bad model for them to imitate, and its exposition of others' arguments punishes the attention of the reader so cruelly that the extinction of learning is guaranteed from the start.

So far as I could make out, the critiques the book brings forth are neither novel nor daring. Standard-gauge ammunition that could shoot down any sociological or psychological theory—the expected bullet to its logical structure, the old rocketing of its ideological biases, the unkind shell on its analogies—is mixed with direct examination of questions peculiar to theories of social exchange, like the limits of exchange. There is much of interest in this; it is a good coverage of the critical literature; students should know of it. But to my way of reading, the critiques are presented too antiseptically. I found it difficult to work up a good sense of enmity against any side and was left with the impression that many of these quiet battles will drag on forever. Among the most tedious of these concerns reductionism, to which Chadwick-Jones devotes a chapter. At this point in what we know about things, arguments for any kind of reductionism strike me as having about the same interest as debates between astrologers. Chadwick-Jones thinks otherwise, since we are caused to witness again the usual Indian wrestling between the dead arms of old faiths.

Future work in the tradition of social exchange theory is not hard to map, once you have an idea of what past research has failed to settle.

Chadwick-Jones rests his judgments about these matters on the most thorough literature review of work in the social exchange tradition I know of. He argues that since exchange propositions "state the conditions under which different degrees of reciprocity occur, and the limitations to reciprocity in social behavior" (p. 397), we are likely to find that future work will seek to specify the "conditions of reciprocity, of equity, fair exchange, negotiation and bargaining in a variety of social situations" (p. 398). I suppose the same problems have been on the map since Simmel, but no enlightened social psychologist wants to buy into a neighborhood with problems other than these. Actually, as Chadwick-Jones's analysis hints, when all the convergences between notions of exchange and balance and symbolic interactionism, and so forth, are worked out, it probably won't make much difference where you buy. The market is bullish, especially for answers.

The Behavior of Law. By Donald Black. New York: Academic Press, 1976. Pp. xi+175. $12.50.

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The language of classic works in social analysis, the writing of Adam Smith or Alexis de Tocqueville, for example, often has a simplicity that is missing from the usual discourse in the social sciences. It is the result of unwavering concentration on what is essential, disregard for conventional wisdom and problem formulation, and lack of concern with the risks of being misunderstood. Donald Black's *The Behavior of Law* displays such classic simplicity not only of style but of substance, too; it stakes out a claim to the status of a classic formulation. It brilliantly integrates a vast range of ideas and empirical materials in the sociology and anthropology of law, boldly asserting general theorems which are related to rich and illuminating detail. Yet its attempt to provide a comprehensive explanatory theory of the law remains problematic because of unresolved basic questions about adequate theory building and its relation to empirical investigation.

The five major chapters of the book relate law, or "governmental social control" (p. 2), to stratification, differentiation, social distance, location in the center or periphery of social life, symbolic culture, organization, and nonlegal social control. Variations in the incidence and in the direction of legal social control are explained in terms of variations in these fundamental "aspects" of social life.

In a first drastic move of analytic strategy Black announces that the major phenomena under discussion are treated as quantitative variables.

"Law is a quantitative variable. It increases and decreases, and one setting has more than another. It is possible to measure the quantity of law in many ways. A complaint to a legal official, for example, is more law than no complaint. . . . So is the recognition of a complaint. . . . A trial or other hearing is itself an increase of law, and some outcomes are more law than others: a decision in behalf of the plaintiff is more law than a decision in behalf of the defendant, and conviction is more law than acquittal. . . . More generally, the quantity of law is known by the number and scope of prohibitions, obligations and other standards to which people are subject and by the rate of legislation, litigation, and adjudication" (p. 3). The other aspects of social life are similarly treated. Thus, culture, laconically defined as "the symbolic aspect [of social life]" (p. 1), "varies in its quantity from one setting to another. In some places it is so rich that a newcomer needs months or years to learn its many features, or he never does; others have only a few shreds of culture, readily known by anyone. . . . In short it is possible to count every kind of culture or all of it together. Compare a modern to a tribal society, a scientific laboratory to a bus station, a professor to his wife or children" (p. 63). Anthropologists and feminists are not the only ones who may have difficulty with this attempt to make such quantification plausible by illustration.

While it is true in principle that differences normally thought of as qualitative can be turned, at the discretion of the analyst, into quantitative ones and vice versa, there are problems in such transformation which must be solved if one wants to avoid severe loss of information, gross conceptual simplification, and distortion of the original questions. Difficult in any case and not necessarily solvable to satisfaction, these questions are never raised, let alone answered, by Black. Thus, he does not indicate how a contract between corporations compares with a traffic violation fine or both with the jailing of a convict, when it comes to aggregating different concrete events into the "quantity of law." Similarly, although he distinguishes four qualitatively different "styles" of law which correspond to different modes of social control in general (the penal, the compensatory, the therapeutic, and the conciliatory modes), he provides no formula by which major and minor incidents of legal social control in one mode can be aggregated with greater and lesser events in another. These are not mere problems of operationalization; rather the strategy of quantifying law, culture, stratification, and other major aspects of social life is not really carried through even at the nominal level of theoretical conceptualization. The quantitative rhetoric of the book is a draft on an uncertain future. For the present it amounts to little more than a metaphor, quantitative imagery.

Leaving major problems unsolved and moving on with the analysis as if they were solved is not an unprecedented procedure. Advancing with force even though the hinterland is not secured and the flanks are open may be a risky strategy and an unsound one for the long term, but it may also

bring breakthrough and decisive victory that elude a more cautious strategy. *The Behavior of the Law* can indeed be considered as such a breakthrough, a breakthrough in theoretical ordering and integration which at the same time goes decisively beyond the established stock of ideas, hypotheses, and empirical generalizations: it presents a host of new hypotheses derived from the general theorems that made the integration possible.

The first of these general theorems is: The greater the differences in wealth, the definition of stratification used here, the more law. This applies to different societies, across regions and communities of a single society, as well as to different settings within a community. Legal social control is greater in a downward direction within a stratification system than vice versa, and its varies with the vertical distance between the parties—in a downward direction distance makes for more law, in an upward, for less. These propositions are closely analogous to hypotheses about the relation of law to the degree of organization, to the amount of culture, and to the integration of individuals and groups with centers of social life. By contrast, the relations of law to structural differentiation, relational distance, and cultural distance are said to be curvilinear; and finally it is argued that law varies inversely with other social control. These and many related propositions are illustrated with a wealth of delightfully chosen examples ranging from the code of Hammurabi through historical materials to contemporary anthropological and sociological research. Each chapter has a section in which different theories of deviant behavior are confronted with propositions about legal social control which explain the same or overlapping phenomena. The last chapter explores the conditions of anarchy; it constitutes an inverted summary of the book since anarchy is defined as "social life without law" (p. 123).

It goes without saying that well-chosen illustrations do not prove general propositions. Given the unsolved conceptual problems of quantification it is hard to see how any procedure could achieve that end. Strictly speaking, this is a book of hypotheses, loosely formulated. The hypotheses have a certain unity because they are similar in form and because most sociologists will see them as implicitly interrelated in content, but Black does not explore whether and how the different factors affect one another as well as each other's impact on the law. Nor does he seek to explain the different propositions in terms of higher-order theorems—another way in which the hypotheses could have been more closely integrated with each other to form a more coherent theory. If these latter criticisms ask for much, they do so because the book aims high. Despite these criticisms and despite the reservations about the strategy of quantification expressed earlier, the book stands out as an important achievement. With daringly simplified theoretical tools it succeeds in integrating much that is known and surmised about law and society, it puts old ideas into a new light, and it develops a wealth of fresh ideas. Last, but not unimportant, the book is well written.

The Deviant Imagination: Psychiatry, Social Work and Social Change. By Geoffrey Pearson. New York: Holmes & Meier, 1975. Pp. xii+258. $18.75.

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Nowhere in the sociological enterprise are the ideological entanglements of theory more prominent and less assiduously examined than in the field of deviant behavior. It has been many years since C. Wright Mills pointed out the rural origins of the "social pathologists" by way of accounting for the wellsprings of social disorganization theory. The earliest indication of a substitute theory came in the early 1950s with Edwin Lemert's misleadingly titled *Social Pathology*. For more than a decade following, this initial statement of the role of the societal reaction perspective in accounting for the rise and course of deviant behavior remained little noticed by students of deviance. It was not until the mid-1960s that the labeling process central to Lemert's thought emerged as the dominant motif of contemporary deviance theory in the work of Howard Becker, John Kitsuse, Edwin Schur, and others of the symbolic interactionist persuasion.

Geoffrey Pearson in this book raises the question: Why then? His answer is given in a lengthy chapter on the "misfit sociologies," under whose rubric are included, in addition to labeling theory, theoretical perspectives drawn from phenomenology, ethnomethodology, existential psychology, Marxism, and Durkheimian ideas. Approaching the question as a sociology of knowledge issue, he contends that the emergence of misfit sociologies is of a piece with the rise of the counterculture movement of the period. Both had their source in the discontents created by the "second industrial revolution" with the flowering of bureaucratic rationality and the attendant dehumanization of the person. Given the contemporary structural supports for cultural pluralism, a contradiction arises between the conformity implicitly demanded by the Welfare State and the native diversity of expressive modes. Posed as a value issue, the question is whether the deviant is a social waste product, to be reprocessed in the hope of his salvation, or a creative pioneer in search of a reconstituted human identity and a crypto-critic of the prevailing culture.

While in full sympathy with the cultural criticism implicit in the misfit sociologies, the author is less than sanguine about their freedom from ideological bias and their failure to address coherently the political meaning of their theories. As for the first, after providing a competent review of the work of the labeling theorists, Pearson notes that their symbolic imagery of the deviant is restricted to "exotic, fringe, even 'bohemian' deviance" (p. 60), typically neglecting thieves, assaulters, rapists, and sundry other predatory deviants. He nonetheless finds virtue in such selective inattention. In their phenomenological, naturalistic, and "apprecia-

tive" orientation, these theories have brought into auditory range the voices of those whose humanity had been implicitly impugned by "mandarin" social science. However, in reading the message heard, Pearson later explores the ideologically constrained imagery of the deviant suggested by these theories. His contention is that not unlike the rural utopianism of their predecessor social disorganization theorists labeling theorists and phenomenologists entertain their own utopian images of reconstituted human communities in the urban milieu. These are marked by tolerance for diversity and plural moral worlds in each of which personal authenticity has a fighting chance. "In this *gemeinschaft*-like community . . . it seems as if social order gets along without skills, control, or know-how" (p. 177). And to this one may add, without the stubborn intrusion of hierarchy and stratification. In brief, he seems to be saying that while such ideological warp operates well as cultural criticism its utility as a basis for prescription is quite another matter.

Pearson's estimate of the political thrust of contemporary deviance theory is particularly enlightening. Although he notes its radical potential in specifying the role of power inequalities in defining deviance, he quotes with qualified approval Alvin Gouldner's faulting of the works of Erving Goffman and Howard Becker as lacking "erect partisanship" (p. 73). Labeling theory appears to offer a basis for a strategy of reform in social welfare by urging nonstigmatizing approaches, but attention to its programmatic embodiment is largely absent. This he does not attribute primarily to a failure of political nerve, although such failure may well serve career protection ends, as critics claim. Modern deviance theory, he asserts, represents instead the resolution of a problem that distinctively confronted a generation of social scientists exposed to and participating in the political eruptions of the 1960s. It enabled them "to put together politics, social science, and compassion; to effect some partial solution to the problem of the relationship between their lives as social scientists and their lives as men and women" (p. 77).

Finally, in addition to a perceptive critical review of the "misfit paradigm," the author presents his own account of how the symbolic imagery of deviance has figured in the economic, political, and social history of modern society. The specter of the "dangerous classes" that haunted the imagination of social commentators in 19th-century Europe provides a clue to the continuing link between politics and the imagery of deviance. In an exceptionally detailed and rich exposition. Pearson argues that the concept of deviance and the images and sentiments that cluster about it had and continue to have their source in the fear entertained by the more fortunate classes of an excluded and undomesticated underclass spawned by the industrial revolution. In its insight and eloquence this treatment of the policies of deviance stands out in a work that unfailingly illuminates every facet of the contemporary crisis in deviance theory.

'rime and Conflict: A Study of Law and Society. By Harold Pepinsky. Iew York: Academic Press, 1976. Pp. x+159. $14.50.

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ione are the years of quiet confidence and congeniality within the field of riminology. The clash between the social order and movements for social hange has politicized all the social sciences and especially criminology, nce it is increasingly the expanded and militarized police and prisons 'hich are called upon to contain dissent, alienation, and poverty. Along 'ith intensified repression, crime has grown beyond "deviance" to the pro-ortion of a cultural institution. These crises have unraveled consensus, and ie new research which follows controversy promises fresh insight into the istory, structure, and political economy of crime and justice.

Several competing criminologies seem emergent. Well-supported justice lanners and academic apologists for the state have adopted a cynical ragmatism, ignoring social inequality while developing the technology and rganization of repression. A radical or Marxist perspective has appeared ithin the academy and in the writing of prisoner-theoreticians like George ackson, together advancing a political-economic critique of justice under ipitalism and exploring the possibilities of a socialist reconstruction. The maining centrist liberals find their positions eroded; from the right by ie state's abandonment of ameliorative reforms, rehabilitation, and crime revention and from the left by liberation movements' demand for social istice and political accountability.

Harold Pepinsky's book stands on the troubled terrain of liberal re-rmism and belongs specifically to the "labeling" school of the conflict erspective. This theoretical treatise is not without passion in its criticisms id legislative proposals; but the author's frequent wordiness and reliance i formal legal and sociological language will doubtless limit readership. concern over both increases in crime and justice bias against poor and inority citizens is obvious in Pepinsky's critical approach to codification, cial values, stereotyping, discretion, and administrative prejudice. Un-rtunately, these immediate foreground issues are not linked to an anal-sis of social structure, state, and economy. We are left without a con-ncing accounting for the sources and nature of crime and repression.

The book's central thesis is ". . . that the influence of law on the behav-r of officials and private citizens is *negatively* correlated with the volume written law and the number of those who are employed in its adminis-ation. . . . as the influence of formal written law and its administrators our society grows, the rates of violations of the law can be expected to crease concomitantly" (p. 2). Startling words these to come from a wyer; but in repudiating law and the legal professionals, Pepinsky exalts

their importance as the basis for crime and justice policy. The seven supporting chapters which follow provide some useful information, particularly in the area of American police operations and Chinese conflict resolution; but the data are not nearly sufficient to support his legalistic thesis.

The author directly challenges the widely held Weberian equation of law and bureaucracy with modernization and civil liberty. Instead, Pepinsky asserts that the codification of offense categories and procedures only creates more issues to decide and more opportunities for the exercise of administrative discretion and bias. Antitrust cases are compared with routine criminal matters to show that the law favors corporate offenders over common criminals because it is more "difficult" to define legally "social injury" and "criminal responsibility" under the civil statutes. This argument that socioeconomic "bias inheres in the form of the law itself" (p. 25) also underlies a subsequent chapter on police decision making in a Minneapolis precinct. Routine police prejudice against poor and minority people is described as an administrative principle evolved to guide the exercise of discretion and also a self-fulfilling prophecy which contributes to high crime rates among those population groups.

The analysis and supporting data are hardly satisfying. If codification is a strategy which administrators adopt "to generate new areas of discretion . . . and to keep at least some areas of decision making safe from discovery of bias" (p. 54), why then do police, prison, and prosecuting officials so often bitterly oppose the formalization of rights and procedures for prisoners and defendants? If prosecution of corporate criminals under the Sherman antitrust statute is a technical problem of the law, how then have those same laws been used so consistently and effectively to punish severely union activists and radicals, most notably the IWW activists who were hanged, imprisoned, and deported by the hundreds? If police racism and class bias is but an administrative convenience and a hardened feature of police subculture, how do we account for shifts in the racial composition of those arrested, shot, indicted, and imprisoned by the justice system? Finally, how does the author's argument explain varieties in crime patterns over time and between different social classes? His causal linkages between law, administrative bias, and crime are tautologies which cannot explain historical and social patterns in crime and justice policy.

Two rather original chapters on justice and social philosophy in the People's Republic of China are introduced to explore the relation between social values, law, and social control from the citizen's perspective. These chapters, like the one on the Minneapolis police, are derived from substantial scholarship, and they are quite readable and informative. The author describes the Chinese reluctance to codify law or establish large legal bureaucracies and notes their emphasis on informal social control within local citizens' organizations. These arrangements are an outgrowth of their commitment to a "continuous revolution" in "the consciousness of

e masses" (p. 84); that is, to the struggle against individualism. This
olitical and legal emphasis on collectivity is effective because Chinese
ciety is relatively static, while the American system of formalize pro-
dures and individual rights is seen as appropriate to a society with great-
geographical and social mobility. Pepinsky is, nonetheless, troubled by
isting arrangements, because our competitive values and individualistic
gal system create an "egoism" which is the "seed of crime"; and this is
turn nourished within the soil of the "class hierarchy."

The author believes that many of our problems can be corrected, but,
keeping with liberal tradition, he pins his hopes for change on the
ntral government and has drafted a series of reform proposals for legis-
tive action. These include the payment of government subsidies to justice
ficials who report and process fewer crimes, the establishment of extra-
gal "social service agencies" to resolve conflicts through mediation and
lvising among the citizenry, and the extension of criminal responsibility
r corporate crimes to include all employees and shareholders of the of-
nding company. He also advocates the "attenuation of class hierarchy"
rough subsidizing those employers who increase wages and work rolls
hile taxing more those who do not.

The problems which are apparent in these proposals flow from the same
oblems of theory and analysis which so flaw this book throughout. Only
failing to understand the economic basis of crime is it possible to
lieve that the curtailment of codification and legal-bureaus' activities
ould reduce predation and violence. The author's failure to appreciate
storical trends and recent developments in the state allows for the rather
ive idea that the government would willingly dismantle part of its re-
essive apparatus, or, even more unlikely, would demand of the capitalist
ass that "they play the game of inter-class cooperation, or they could
andon their assets and refuse to be employers at all" (p. 130). The
thor insists on defining class in essentially static, depoliticized terms as
ocio-economic status" and so does not recognize the interface between
ass interests, class conflict, and the state. Thus he ignores the class com-
sition of Chinese citizens organizations, misses the role of these groups
overthrowing the feudal landlord and capitalist classes, and fails to note
at these groups influence as well as support the Chinese government and
ommunist party. He assumes that community organizations in this coun-
y could win popular support and reduce crime without any fundamental
ange in the economy and without any legal status or political power.

This book shows the mark of some serious research. It is ambitious,
peful, and in many ways a creative effort. The issues it raises are sig-
ficant; but it deserves a formulation which researches beyond "law
eakers and law enforcers" to critically examine the larger dimensions of
ate, class, and ideology as they bear on crime and justice policy.

Delinquency, Crime, and Society. Edited by James F. Short, Jr. Chicago: University of Chicago Press, 1976. Pp. 325. $15.00.

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This book is comprised of a dozen original articles prepared as a tribute to Henry D. McKay, most of which were presented at a symposium held in 1972 at the University of Chicago. The theme of this collection is a "reassessment" of the Shaw and McKay tradition in light of contemporary research and issues in the study of crime and delinquency. Hence, attention is paid to some of the more important empirical questions raised by this research tradition, and these questions are in turn viewed from a contemporary perspective.

In the introduction, Short (p. 2) explains that the contents of this book reflect two basic concerns: first, the explanation of crime and delinquency and second, the analysis of its social control. Since these concerns are not independent, the volume is not organized around them as such but around four headings: "Basic Theoretical and Empirical Issues" (pt. 1), "Studies of Youth Gangs" (pt. 2), "Attempts to Replicate Findings and Interpretations of Shaw and McKay in Two African Settings" (pt. 3), and "Studies Focused Primarily or in Major Degree on Social Policy Implications of Research and Theory" (pt. 4).

In part 1, H. Finestone traces the intellectual development of the theoretical perspective used by Shaw and McKay, depicting this tradition as a succession of efforts to develop a theory that would be comprehensive enough to encompass both the collective (epidemiological) and individual (social psychological) aspects of delinquency. Under the influence of such colleagues as Park and Thomas, Shaw and McKay attempted to blend aggregate statistical data with life-history data in order to relate delinquency to the process of social change in the inner city but soon adopted a functionalist perspective in order to account for the relatively stable structural features of these areas which led to a tradition of delinquency. Finestone suggests that this perspective has difficulty identifying the processes through which individuals become delinquents and that the Shaw and McKay tradition should be integrated with the interactionist approach in a manner that bridges micro and macro levels of analysis and provides for a more adequate explanation of delinquent behavior. In the second chapter of this section W. Simon, J. Puntil, and E. Peluso compare the early research of Shaw and McKay with current findings. They conclude that their current findings, despite improved methods of data reduction, are too fragmented to produce an integrated, coherent explanation of how the social structure generates deviant behavior. In the final article A. Reiss

examines the social class distribution of delinquency as indicated by official statistics, self-report studies, and victimization surveys and convincingly argues that unofficial measures assessing crime and delinquency rates do not refute two major conclusions which Shaw and McKay drew from official statistics: delinquency is endemic in certain neighborhoods, and the probability of becoming a delinquent is greater for persons in the lower income status groups than in the higher.

In part 2, W. Miller focuses on American youth gangs during the 1960s, examining the impact of social and cultural changes and the politicization of social issues as they affected gangs during an "era of urban crises." Miller examines media coverage of gangs and gang involvement in civil disorders, political activism, and drug usage, then forcefully argues that none of these developments had much of an impact on American youth gangs. Hence, publicized changes regarding the prevalence and behavior of gangs during this period are seen by Miller as being temporary or inaccurate. J. Short closes this section with a discussion of developments external to the gangs in the city of Chicago which served to change the "image" of the gang and were instrumental in shaping behavior toward the gang by elements of the larger society during the 1960s. In particular, Short notes the importance of the media and the police in determining the public's perception of the "gang problem" and in creating the atmosphere which enabled the emergence of a few "supergangs" such as the Black P. Stone Nation.

In part 3, S. K. Weinberg attempts to determine the extent to which Shaw and McKay's findings are replicated in the metropolitan area of Accra, Ghana, with respect to (1) the zonal distribution of urban delinquency, (2) the relationship between ecological succession of ethnic groups and the transmission of delinquent values, (3) peer group experiences and delinquent subcultures, and (4) family influence on delinquency. Weinberg's data indicate that despite differences there are considerable cross-cultural similarities in "the use and expression" of juvenile delinquency in America and Ghana. M. Clinard and D. Abbot conclude this section with an examination of property crime in two urban slum communities in Kampala, Uganda—one with a high, the other with a low, crime rate. Interviews with residents disclose that differential rates of property crime seem related to differential social organization—the low-crime slum had higher degrees of both cultural and communicative integration. The authors tentatively conclude that while urbanization and industrialization erode traditional authority (1) older people still play a vital role in determining the character of the community, (2) higher SES does not increase the community's ability to control behavior within its confines, and (3) rapid population change does not automatically destroy community cohesiveness.

In the final section, D. Cressey examines corporate crime and contends

that the pattern of criminal interaction in the business world and those observed in the slum as described by Shaw and McKay are very much alike. In both the corporate and the ghetto worlds there is a pervading ideology "conducive to widespread law violation," and neither world is effectively organized to combat crime. Cressey deplores the fact that American society is not organizing to enforce antitrust laws but is vigorously demanding the prosecution and punishment of street-crime felons. S. Kobrin next examines the contribution of labeling theory to the study of delinquency, particularly the establishment of diversion programs to reduce the effects of stigma. Kobrin tells us why he feels these programs are bound to fail, or at best achieve limited results, and argues that what is really needed to limit recidivistic deviance is an intervention strategy that is both more realistic and radical. D. Glaser follows with a discussion of the intellectual history and development behind the use of marginal workers—a role utilized by Shaw and McKay in their Chicago Area Projects in which persons were trained to bridge the gap between youth and adult worlds as detached workers, applying the ideas of social sciences to prevent delinquency. Glaser concludes by recommending ways for schools to control delinquency by reducing the social separation between generations. M. Wolfgang then reflects upon the birth-cohort studies conducted by himself and his colleagues and suggests relevant policies which might be implemented to make our juvenile justice system more effective. His data suggest that intervention programs should not be attempted prior to the age of 18, and until a youth receives three recorded offenses (unless the first or second act is very serious). Wolfgang further suggests that courts employ accumulated seriousness scores to be used "as a basis for determining the moment and gravity of the sanctioning intervention" (p. 281). In the final selection, Hans Mattick takes the American criminal justice system to task for arbitrariness, capriciousness, discrimination, exploitation, and promotion of recidivism. Mattick concentrates his attack on our prison system, pointing out why it did not work at its inception and why it continues to encourage the type of behavior it is supposed to discourage. He concludes his essay with a number of recommendations for reform of our criminal justice system—the major theme being "in every appropriate case and at every stage of the traditional criminal legal process, all forms of custody, and especially imprisonment, should be avoided whenever possible" (p. 314).

While the focus and cost of this volume limit its utility as a textbook for undergraduate courses, many of the essays presented are outstanding and contribute much to our knowledge of crime and delinquency and the critical issues related to its control. The essays by Reiss, Miller, and Wolfgang are particularly noteworthy and thought provoking and are just a few examples of why this book should be read by specialists in the field, particularly those who believe that the "Shaw and McKay tradition" has outlived its usefulness.

The Reformers: An Historical Survey of Pioneer Experiments in the Treatment of Criminals. By Torsten Eriksson. New York: Elsevier Scientific Publishing Co., 1976. Pp. 310. $17.50.

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In *The Reformers,* Torsten Eriksson, former director general of the National Swedish Correctional Administration and elder statesman in international correctional circles, provides the general reader with an overview of the better-known prison innovations and the reformers responsible for them. For those expecting some insight into the sociology of corrections or why the Swedish correctional system is so admired in the United States, this book is a disappointment.

To the casual observer the most puzzling aspect of the history of prisons is their early humanitarian justifications. In the United States the prison was a result of reform movements aimed at improving the offender, and the reformers who pioneered modern penal institutions were persuaded that incarceration was for the offender's own good. These and the other reformers Eriksson discusses were by and large decent representatives of the upper middle class who were appalled at the prison conditions they discovered. They all had occasion to visit prisons for some reason or other and wrote movingly of what they found. Because they were influential and highly placed, some of their recommendations led to change.

The best known of the reformers discussed by Eriksson is John Howard (1726–90), an independently wealthy Englishman. Howard's interest in prisons was initiated by a brief sojourn in several French jails as a prisoner of war. He embarked on a lifetime crusade to abolish inhumane prison conditions such as corporal punishment and to ensure proper sanitation, proper diet, and adequate vocational opportunities for those confined. His most enduring contribution to later correctional professionals seems to have been his investigative style. He favored visiting prisons all over Europe and the United States and writing about them. His modern counterparts organize international tours of prisons and give college credit for them.

Eriksson is very much part of that tradition. The jacket description of his background notes that he has visited 68 countries exploring correctional facilities. Sprinkled throughout his book are lengthy quotations from letters, journals, and diaries of the reformers and photographs of the places they visited.

None of Eriksson's reformers, however, was so rash as to propose the abolition of prisons: they all assumed the necessity of restraining some dangerous persons against their will. John Irwin's "Adaption to Being Corrected" in *Handbook of Criminology,* edited by Daniel Glaser (Chi-

cago: Rand McNally, 1974) points out that prison reformers were opposed to brutal punishment but animated by a mixture of hatred, fear, and revulsion concerning those who didn't share their life-styles or work ethic. The revulsion was disguised by a humanitarian concern for the health and well-being of "the dangerous classes." Imprisonment was a suitable response, perhaps a necessity, for the complicated and contradictory feelings of the reformers.

Eriksson's own judgment is less harsh. One of his reformers, however, made his views of prisoners quite explicit. Sir Walter Frederich Crofton, who became director of Irish prisons in 1855, believed in the existence of a criminal class "which had to be controlled and absorbed and gradually reinstated in society as a whole" (p. 92). His contribution was a system of "marks" which the prisoner could accumulate toward early release. Crofton's "criminal type" was easily identifiable. He generally was a lower-class Irish male who was unwilling or unable to adapt to the factory system. He needed to be placed in a structured environment where he could learn good work habits and respect for authority. A necessary condition for reentry into society was unconditional submission to English authority.

Although Eriksson does not deal with the prison's role in developing a docile work force under early capitalism, he does discuss the immediate forerunner of the modern prison, the English houses of correction set up to control the wandering unemployed. It combined the principles of poor house, workhouse, and penal institution with the aim of making the labor power of unwilling people useful. Eriksson reports that the houses of correction had to be self-supporting and were best utilized in situations of labor shortage where the lower class could withhold their labor. They were no longer required after the development of a surplus of labor and overpopulation.

But Eriksson does not systematically analyze the prison's relation to the economy. Nor does he look at the roles of the prison and the prison reformer in promoting adherence to prevailing cultural norms. As some critics have pointed out, the prison's attempt to induce cultural conformity in the United States in the 19th century bears a disturbing similarity to the policy of compulsory assimilation carried out by the American Indian Bureau with respect to the American Indians from 1849 onward.

Eriksson's history traces the introduction of work systems into the prison, the experiments with congregate and solitary living, and varied incentives to rehabilitation other than corporal punishment, for example, the indeterminate sentence, cottage living intended to simulate family life, self-government, short-term imprisonment, open institutions, therapeutic communities, and group counseling. He believes all of these efforts have made confinement more humane and represent genuine contributions by reformers. Critics might be more skeptical. The progressive model he concludes with is the Swedish one, in which institutions are smaller and more open,

inmates have their own cells resembling hotel rooms, each with its own key held by the inmate, and furloughs are available and vocational and therapeutic opportunities abundant.

A more penetrating analysis would challenge Eriksson's view. No serious student of the prison has been able to ignore the symbolic significance of the penitentiary since David Rothman's study of the asylum in 19th-century America: *The Discovery of the Asylum* (Boston: Little, Brown, 1971). Rothman documented the unshakable popular conviction that the fundamentals of proper social organization could be established in the penitentiary. Nor can one ignore the enthusiasm with which correctional personnel embraced the notion of criminal as "patient." Perhaps the most important innovation in recent corrections was the indeterminate sentence. The rehabilitation ideal assumed by it was taken over by prison administrators because of its effectiveness as a control device. Under a rehabilitative system, *the prisoner is held until he demonstrates that he has been* reformed. Dangerous criminals can be held indefinitely under this system. On the surface the goals of rehabilitation seemed at odds with a punitive philosophy, but they fitted it quite conveniently in practice. The key to understanding innovations in prison practice is not the ideas of the great pioneers but the role correctional personnel have played as an interest group. In short, behind the rhetoric of reformation and public protection we can discern the realities of a professional group on the ascent.

Eriksson notes that the prison reformers were all convinced of the negative impact of prisons on those incarcerated. Their dilemma, which Eriksson does not discuss, was that no matter how benign the new experiments they still required involuntary incarceration. An institution may be called a "school," a "mental hospital," a "correctional facility," or even a "campus," but if persons are involuntarily confined in them they are prisons. Reformer strategies to change the conditions of deprivation prior to the 1960s rarely involved the convicts themselves. Since then the latter have played a major role through the courts, aided by the corresponding growth of prisoners' unions. Eriksson does not document this development. His end point is the acceptance of the "new penology." The crucial development recently, however, is the shift from prisoner as patient to prisoner as plaintiff. This transformation has had a corresponding effect on the correctional official. He has been transformed from a treater to a defendant.

For the sociologist interested in total institutions, this book has little to offer. It is basically the kind of historical tour of which correctional personnel are so fond, focusing on the personalities of dedicated reformers rather than the conditions which made their innovations propitious. That sociological topic, using much of the same material as does Eriksson, was much better treated long ago in George Rusche and Otto Kirchheimer's classic work, *Punishment and Social Structure* (New York: Columbia University Press, 1939).

Communes, Sociology and Society. By Philip Abrams and Andrew McCulloch, with Sheila Abrams and Pat Gore. New York and London: Cambridge University Press, 1976. Pp. ix+239. $16.95 (cloth); $5.95 (paper).

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According to Abrams and McCulloch communes tap a fundamental dilemma of social life. They keep appearing and reappearing, but also they are fragile. The self-conscious separation of the self from the social is the cause of attempts to reintegrate human experiences communally. Periodically throughout history there have been outbursts of enthusiasm for communal life. One such period was the 1960s. The British communes of the sixties were what the authors call secular family communes. These attempted to cultivate friendships and were different from the utopian communities of an earlier period studied by Rosabeth Moss Kanter and Benjamin Zablocki. The main question in this book is whether friendships can be self-consciously institutionalized through communal living. The authors believe that friendships are difficult if not impossible in a capitalist society. Quoting Sartre, they state that the bourgeoisie have no friends, only competitors. How this relates to their study, which looks primarily at communal life, is unclear. Most of the communal members would be classified as workers. The question probably should be: Does communal life contribute to the development of working-class consciousness or does it provide a retreat from the oppression and alienation of industrial life? The authors consider their study to be in the tradition of C. W. Mills, but they have forgotten that Mills documented solidarity among the bourgeoisie, not competition. This is only one of several confusions and analytical ambiguities in the book. Because the theoretical perspective is disjointed and often contradictory, this is a difficult book to read (and therefore to review). However, it is valuable as a historical document, describing life and social arrangements in British communes.

A commune, according to the authors, is a relatively stable group, intentionally formed, containing between five and 25 adults with whatever children they may happen to have. The work in the common household or complex of households is shared. Ideally group life has primacy over all except possibly that of couples of mixed or the same sex. The group is bound together morally and economically. This book provides material comparing the nuclear family with communal life-styles. Chapters 3, 4, and 5 describe relationships, conflicts, and household arrangements. Analyzed as settings for the creation of individual personalities and the conduct of domestic personal relations, communes are seen as an alternative to either the single life or that of the traditional family.

The communes described are not utopias. Men and women are not equal, authoritarian leaders emerge, and individual creativity is not always en-

hanced. However, many persons receive more social support and are less lonely than they were when their living arrangements were different. The goals of commune members are to become social beings (and by implication happy) and to develop the habit of acting and thinking together. These goals are difficult to achieve in a culture where most people have been socialized to be individualistic and possessive.

Following the descriptive chapters there is analysis of why British communes are fragile and why so many fail. I found this theoretical analysis pompous, wordy, and irrelevant to the questions asked and the data collected. To describe communal life is worthwhile, but to apply macrotheory inappropriately to a case study can mislead students to whom the book is directed. After stating that all communes are fragile and that capitalism and communal life are polar forms of organization, the authors discuss success and survival in terms of Durkheim's concept of organic solidarity. I see no connection between Durkheim and the nature of friendship in a capitalist state. I agree with the authors in their interpretation of Durkheim that organic solidarity is probably impossible in advanced technological societies where individualism and anomie are endemic. For Durkheim, the solution to the problems of individualism and anomie was to provide intermediate (not primary) ties between the individual and the state through occupational guilds. Tönnies, who is not mentioned once, might have been more appropriately applied because of his emphasis on community solidarity.

Also, there is little in this book to suggest that communal living might be more successful under other kinds of economic and governmental forms. The only in-depth comparative example is the kibbutz in Israel, which the authors consider successful and which does exist in a capitalist state. From their analysis and theoretical orientation, it is not clear whether they seriously believe that the communal movement in the sixties was a protest against capitalism or a protest against the "existing social life" (the nuclear family). The communal movement as protest has existed at least since the Reformation, before industrial capitalism was a reality, and has persisted in a variety of forms since then. The nuclear family (also in various forms) seems to persist in socialist and communist nations. Industrial nations may be able to tolerate different forms of living arrangements which may or may not enhance social bonding.

The last chapter of the book discusses social policy implications. Because of the lack of clarity and the theoretical confusion, there is much to object to in this chapter, even though there is also much to commend. The data show a great variety of orientations and philosophies among communes. In chapter 4 the authors state that the structures developed neither enabled nor prevented more equality or freedom for women. Nowhere in their data is there any evidence for their conclusion that unsupported women would benefit from communal living more than others in the society. This might be acceptable folk wisdom, but clearly as a policy sug-

gestion it belongs elsewhere. The arrangements they suggest may be fine for unsupported mothers and their children. Possibly such communes should be state supported. However, it is strange that the authors who suggest in one part of the book that communes are doomed to failure under capitalism because of individual possessiveness should advocate elsewhere their formation as a solution to a problem so closely tied to the capitalist system. If friendships and social support are impossible under capitalism and if advanced capitalism means increased possessive individualism, then maybe the way to change is not more communal living but a more fundamental economic solution.

An Assembly of Good Fellows: Voluntary Associations in History. By Jack C. Ross. Westport, Conn.: Greenwood Press, 1976. Pp. xiii+327. $14.95.

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The study of voluntary associations is to an unfortunate degree limited to consideration of the United States and lacks cross-cultural and historical perspectives. With an impressive research effort, Jack Ross has made a notable contribution to the growing literature which seeks to expand the boundaries of the field. The significance of this volume lies in the consideration of a wide range of cultural systems, an eclectic approach to data collection, and, perhaps best of all, a rich historical view of the character of voluntary associations, especially in England.

The analysis is based on an organizational "model" which specifies the character of voluntary associations with regard to existence, prevalence, variety, purpose, and consequences. However, Ross concedes in the first chapter that the limited nature of the available data precludes an extensive analysis of any but the first two of these factors. With some critical comment, Ross adopts from David Smith nine variables to guide the exploration which follows. To call the theoretical basis of the book a model is perhaps a bit premature (as Ross himself suggests) because the interrelationships of the variables are only sketchily drawn. It would be more accurate to regard this work as a step toward a model through the investigation of the association of the variables with the existence of voluntary associations in various cultural settings.

In the first two of the data chapters, 10 cultural cases are considered. Chapter 2 considers five primitive societies, and in the following chapter an expanded version of an article appearing earlier in D. H. Smith's *Voluntary Action Research: 1973* (Lexington, Mass.: Lexington Books, 1973), five ancient societies are included.

The representativeness of the societies included in these chapters is left

largely undetermined, and the availability of data probably weighed heavily in the selection process. Ross draws on historical accounts of variable depth and sufficient to support only a limited examination of the model proposed. Consequently, instead of using the theoretical basis as a framework for his presentation, he is limited to a largely descriptive case-study approach that offers several interesting conclusions, although some variables are omitted entirely.

The problem of scanty data in the first part of the book is in contrast to an extensive description of the history of English voluntary associations found in two of the better chapters that follow. As Ross sifts through a much richer literature, interesting description is supplemented by more impressive theoretical analysis and awareness of social change.

The 800-year span of these two chapters is heuristically divided by the rule of Elizabeth I. Ross's discussion of the first era focuses on the religious fraternity and the guild; he presents the hedonistic club as the ideal-typical voluntary association of the second era. In addition to discussing the existence and prevalence of English voluntary associations, he pays a significant amount of attention to their variety, purpose, and consequences. These two chapters offer a broad look at the religious, economic, and political character of associations as well as the role of science, the arts, and charity in their history.

The concluding chapter reintroduces the variables presented at the outset and provides a discussion of their place in a theory of voluntary associations.

From a methodological perspective, Ross offers a worthwhile presentation of the strengths and difficulties of a multidisciplinary search, with an appendix to supplement comment in the text.

Overall, *An Assembly of Good Fellows* is a detailed excursion into a wide range of social history. Its breadth contributes to an expansion of associational research beyond both the recent American scene and the somewhat arbitrary division of disciplines. Yet the cultural range is not achieved without penality. First, the limitation of available data makes the objective cross-cultural comparison highly tentative. Second, these data do not support rigorous analysis of the associational model. Perhaps most important, Ross moves so rapidly through each of the cultural cases of primitive and ancient societies that the reader gains little sense of voluntary associations as part of a broader cultural system. The two-chapter account of English associational history is far more carefully done and contributes substantially to the value of the study.

Certainly some of the shortcomings noted above are inevitable in an endeavor of this kind, and the author does a creditable job of sensitizing the reader to the limitations of the study rather than sidestepping them. This methodological concern and the rich discussion of associations in English history are perhaps the greatest assets of the book.

The Social Psychology of Military Service. Edited by Nancy L. Goldman and David R. Segal. Beverly Hills, Calif.: Sage Publications, 1976. Pp. 299. $17.50 (cloth); $7.50 (paper).

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This volume is a collection of papers presented at a conference with the same name held at the University of Chicago in 1975. It brings together several reports of past and present research with important implications for contemporary military manpower policy, processes of socialization and adjustment to training and operational environments, changes in the structure of the military family, and attitudes toward the military institution.

The first section deals with behavior in operational environments. John Faris discusses the increasingly significant role of the drill sergeant in basic training and how it is being transformed by the transition to an all-volunteer force. Jonathan Borus reports on a study of the adjustment of combat veterans to routine garrison life and an unsuccessful attempt to introduce a preventive intervention program to facilitate the adjustment process. Jay Lifton reports on his experience with "rap groups" of antiwar veterans with special reference to their attitudes toward military chaplains and psychiatrists. Erik Gunderson presents data which indicate that shipboard environmental conditions are correlated with health, satisfaction, performance, and retention of naval personnel. Paul Nelso demonstrates that biographical data can serve as predictors of naval adjustment. David Bowers compares work-related attitudes of naval personnel with those of a national sample of civilians.

Several of the papers deal with larger institutional features of military organization, such as the family. Nancy Goldman traces the evolution of familism in American military organization and the implications of the greater prevalence of married personnel in the modern armed forces. M. Duncan Stanton surveys the literature on the military family and suggests that the family should be the primary unit for management decisions. Ellwyn Stoddard and Claude Cabanillas discuss the stresses encountered by the army wife in her "complementary role" and report that they encountered an increasing level of role stress through the first half of the officer's career but thereafter—especially after the decision to retire—a rapid decrease in role stress.

A final section deals with the transition to civilian life and the changing image of military service. M. Kent Jennings and Gregory Markus present a longitudinal study of political participation by Vietnam veterans. David Segal and Mady Segal use data from the Detroit Area Study for a report on veterans' attitudes toward trust in government and isolationism-interventionism. John Blair and Jerald Bachman compare the attitudes of veterans and nonveterans toward the military and Vietnam policy in one

article; in another, they consider ideological implications of an all-volunteer force. Finally, Ronald Inglehart presents cross-national data comparing a values indicator with attitudes toward military service.

The volume is a useful addition to the literature of military sociology.

Entry into the American Labor Force. By Michael D. Ornstein. New York: Academic Press, 1976. Pp. vii+220. $14.50.

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This study does a commendable job of exploring entry into the labor force, both as an occupational stratification process and in terms that ". . . speak to the concerns of labor economists" (p. 22). It can be expected to take its place in the literature in the tradition of Blau and Duncan on the one side and the Parnes Career Threshold study on the other.

This is an empirical work whose data base is an NORC survey of noninstitutional U.S. resident men between the ages of 30 and 39 in 1968. The occupation, industry, wages at start and end of job tenure, extent of on-the-job training, job separation information, and much more were collected retrospectively for every job (including part-time and military) held from age 14 to the time of the interview in 1968 for some 1,600 respondents. This highly impressive data source comes out of the Social Accounts Project at Johns Hopkins. While Ornstein does not spend much time describing the sample, it appears to be an excellent resource for studying entry or any other early career process. Blacks form 46% of the sample, and race is a primary classification variable throughout the book.

Chapter 2, "Entering the Labor Force," concentrates on describing the job obtained at entry as a function of individual characteristics ("leaves full-time schooling and participates in the labor force for a period of more than 16 months without returning to full-time education during that time" [p. 24]). Multiple regression is used to explore the effects (for blacks and whites separately) of family background, age, education, and work experience before entry on both the quality of the first job (as measured by a prestige scale) and the starting wage.

Results here generally confirm earlier observations by other researchers. For example: "Blacks start out from poorer backgrounds, acquire less education, hold fewer and lower quality jobs before entry, and obtain first jobs with lower wages and prestige scores than whites. This process does not simply pass on critical inequities of background, but adds to them at every step" (p. 74). By comparing the prestige and wage regressions it is also shown that the determinants of these two elements of job quality are quite distinct.

Chapters 3 and 4 deal with the mobility of individuals during the first two years following entry. Analysis by broad group indicates that mobility is generally productive. Chapter 4 probes accordingly the reasons for job change and the prestige and wage changes that follow. The regression of the number of jobs held during the first two years on a wide variety of supposedly relevant characteristics leads Ornstein to conclude that "neither education, family background, extent of early work experience, or [*sic*] the nature of the job held bear much of a relationship to the number of jobs held" (p. 109).

Much better results are obtained for a regression with duration of first job as the dependent variable. It will come as no surprise to economists that ". . . the predominant factor in determining first job duration is the amount of wage increase during the job" (p. 124). The effect of discrimination in reducing job opportunities is also indicated by the greater efficacy of wage increases in holding black employees.

Chapter 5, "The Consequences of Entry," traces the respondents to the point eight years after entry. A surprising amount of educational mobility is revealed in the sample. Military service is shown to have positive prestige effects for individuals with low educational attainment and negative effects for educated men. The wage analysis shows that military service tends to lower white earnings but to raise black earnings.

Finally, the prestige and wage of the job held eight years after entry are regressed on a set of 32 variables representing family background, education, characteristics of the entry job, and characteristics of the current job. There may be some colinearity problems in these regressions since prestige and wage of starting job are included. In fact, the entry wage turns out to be the best predictor of the wage eight years after entry. Generally speaking, the analysis in this chapter does not bridge the sociology-economics gap so well. This is reflected in Ornstein's statement, "The clear implication is that somewhat different strategies will be necessary to develop wage, income and perhaps wealth stratification models to accompany or be integrated with the models that now use socioeconomic or prestige rankings of occupations as the major variables" (p. 159). We can only hope that further transdisciplinary efforts of this sort are forthcoming.

Public Places and Private Spaces: The Psychology of Work, Play, and Living Environments. By Albert Mehrabian. New York: Basic Books, 1976. Pp. xii+354. $15.95.

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Holistic research on everyday settings in modern society is growing in both volume and sophistication. Contributors to this line of study include soci-

ologists who use ethnographic methods, anthropologists who follow Laura Nader's advice to study their own society, and environmental psychologists such as Robert Sommer and Albert Mehrabian. The vocabulary of such research is rapidly becoming the *lingua franca* of the applied behavioral sciences. Sociologists will find Mehrabian's book accessible conceptually and interesting in the way he has selected and handled topics.

What Mehrabian sets out to do is to criticize and redesign almost every detail of our sociocultural environment. His book contains specific advice on how to reconstruct factories, offices, prisons, schools, libraries, mental hospitals, middle-class homes, spas, movie houses, museums, theaters, sports arenas, retirement communities, suburbs, restaurants, and many other things. In fact, it would be possible to build an entire middle-class community following Mehrabian's advice, a community which, I think, on the whole, would be better than the communities we now have, but also more expensive.

The most imaginative theoretical move in the book is the introduction of the general adaptive syndrome (GAS) into environmental studies; it is an adaptation to conditions where an animal must constantly ready itself for flight or fight only to discover that most alarms are false. The GAS reaction is a systemic adjustment which permits rapid neutralization of extreme stimulation following a false alarm. It changes the blood chemistry. Human beings have the vestiges of a GAS reaction which we can abuse with drugs and by forcing ourselves to work beyond our limits. For example, if a person smokes to the point of initiating a GAS reaction, the reaction neutralizes the effect of the nicotine and the person progresses from a state of stimulation to a state of depression. If the individual smokes, drinks, or works beyond the point of a GAS reaction, he or she does so not for stimulation but only to maintain a normal level of arousal. This also keeps the GAS reaction going. Apparently human beings were not designed to live long lives while constantly overriding the GAS response by self-stimulation.

Mehrabian's conceptual framework is composed of relatively few easy to understand variables relating to the GAS response, psychological arousal levels of the individual, and what he terms the "load-level" of the man-made environment. He suggests that droning noises, dull colors, square rooms, hard chairs, etc. (these are all aspects of low-load environments) do not arouse the individual as do differentiated settings, bright colors, irregularly shaped rooms, soft furniture, music, etc. But the arousal-load equation works only up to a point, at which the load becomes unbearable and the individual begins to GAS himself. Mehrabian seems to have an excellent practical anthropological sense of what constitutes stimulation and too much stimulation in environmental design, but one wonders whether this is the result of his environmental psychology or his own specific gift.

The quality of the specific sociological content of the book is uneven. Mehrabian's description of the social organization of theatrical performances (p. 220) is the best I have read, as is his analysis of the differences between the organization of television viewing and that of motion pictures and theatrical performances (pp. 218–19). However, some of his other explanations of social phenomena are too simple: "In essence, a suburban way of life . . . helps to reduce feelings of anxiety, and this explains the tremendous exodus in recent years to the suburbs" (p. 314).

Most of the advice that Mehrabian proffers is on the side of moderate self-indulgence. One often hears the voice of a generalized mother in psychological writings, and it is not absent from this book. Mehrabian gives advice on how to dress, bathe, smile, and make friends (p. 70); when to come home from a date (p. 271); how to decorate a living room (pp. 97 ff.); etc. And he goes beyond conventional wisdom to give advice that few mothers would provide, such as how to avoid premature ejaculation (p. 76).

In short, the book provides more than a design for a new community, it also supplies a design for behavior and attitudes that are adapted to modern life. In this regard, it is interesting to note that Mehrabian's prison and mental hospital are no longer "total institutions" but are open to the community and are designed so as not to be sharply different from their surroundings. But a new kind of total institution emerges in the book: an enormous recreational complex containing gourmet restaurants, discotheques, junk-food restaurants, pornographic and other movie houses, diverse live entertainment, and finally sleeping rooms, all under a single roof —a kind of urban resort that pleasure seekers would never have to leave.

Mehrabian's study, and others like it, represents a recent tendency to get the social scientists out of poverty areas and into research on the international middle class. The economic basis for such research is a need for a humanistic applied behavioral science in the design of new communities. Its political base is a growing concern that much social research from the past decades was the scientific front for a political program of domination even if this dimension of their work was not necessarily on the minds of the social scientists involved. Now, in universities, think tanks, and research laboratories, the scientists are rethinking the relation the international middle class (which they serve) has with the rest of the world and are suggesting new models for the relationship among the rich nations and the poor, the social classes, the city and the country, etc. The Western middle class, as a matter of necessity, is becoming more reflective, even more self-critical, and the emerging new holistic paradigms for the analysis of modern communities are the current scientific version of this new collective self-consciousness. An important aspect of this thought and change is an energetic movement to redesign everyday social settings to make them more energy efficient, socially accountable, and livable. Mehrabian has made a strong, readable contribution to this movement.

The Quality of Working Life. Vol. 1: *Problems, Prospects, and the State of the Art.* Vol. 2: *Cases and Commentary.* Edited by Louis E. Davis and Albert B. Cherns. New York: Free Press, 1975. Pp. 450; 387. $12.95 (cloth); $4.95 (paper) per volume.

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"The quality of working life" (QWL) has become the central organizing concept for studies of organizational behavior in industry in the 1970s. This two-volume work, growing out of an international conference held in 1972, provides a good opportunity to assess the state of knowledge in this currently popular line of research.

Although the books aim at comprehensive coverage, and although the "task force on assessment of substantive knowledge" (2:357) recognizes the existence of a variety of forms of participation, including worker control, these books make no effort to explore the full range of decision-making systems and in fact focus primarily on the man-boss relationship and on collective bargaining. Within these limitations, the two books reviewed here seem to me the best source of information now available regarding the problems and processes of introducing changes in socio-technical systems, with the aim of improving the quality of working life.

Book 1 begins with essays stating the humanistic goals of improvement of the quality of working life and then moves on to detailed definitions and measurement problems and procedures. In addition to the editors, this section includes contributions by such leaders in this new line of research as Herrick, Maccoby, Walton, Seashore, Lawler, and Trist—though Trist, who pioneered in this type of research with his coal-mining studies in the 1950s, is here represented only by a slight and very preliminary treatment of a government-imposed participation program in Peru.

There is nothing very novel about the discussions of definition and measurement, but they do broaden the base from earlier studies of job satisfaction to focus less on passive acceptance of the job situation and more on opportunities for initiative, responsibility, and growth in competence. The focus on technology differs markedly from major studies of the human aspects of technology of the 1950s. The studies of that era by Walker, Guest, Turner, and others amply demonstrated the oppressive nature of automotive assembly-line technology, but most of us at the time assumed that this technology was essential for efficient production of automobiles and therefore the technology had to be accepted as given, subject to only minor modifications. As Swedish auto manufacturers found it increasingly difficult to get enough Swedes to man the assembly lines, they introduced drastic changes in technology to provide for an assembly process based on work teams having substantial control over their own activities. More recently (and therefore not reported in these volumes), U.S. auto companies have begun to experiment with their own ways of break-

ing away from the rigid assembly-line model. As we find that such drastic changes in technology are economically possible and may even be economically advantageous, this discovery is leading social scientists to an active examination of the impact of different technological solutions to the manufacture and assembly of the same product as they bear upon the quality of working life for the workers.

A prominent feature of developments in this field has been the growth of semiautonomous work groups, where workers take over many of the functions and responsibilities previously carried by supervisors. Some of the most important work on this new pattern of job design and supervision has been done by Gerald Susman, who is represented here by the essay, "Technological Prerequisites for Delegation of Decision Making to Work Groups," and who has developed this theme more fully and systematically in a recent book, *Autonomy at Work* (New York: Praeger, 1976).

Part 3 of book 1 examines the role of consultant or change agent to the firm in cases where job and organizational redesign projects are undertaken. Here Einar Thorsrud, one of the leaders of QWL research and intervention in Scandinavia, presents a valuable analysis of the intervention process based on his own rich experience. The authors in this part outline a participatory intervention strategy in which the consultant stimulates the labor and management people to work together toward restructuring of jobs.

Parts 6 and 7 of book 1 deal particularly with QWL interventions in the context of union-management relations, with the emphasis on European experience. I find particularly valuable the chapter by Yves Delamotte entitled "Union Attitudes toward Quality of Working Life." He points out that in Scandinavia, where QWL interventions have moved rapidly with union collaboration, unions have in general accepted the capitalist system, whereas in France, where QWL interventions have made little headway, union opposition has been based on ideological commitment to social revolution. This might lead one to explain the difference in terms of ideology, but Delamotte goes on to point out that in Italy where, as in France, the strongest unions are Communist dominated, QWL interventions have nevertheless made significant progress. He argues that the difference between France and Italy in this regard is based on the structure of industrial relations: French unions have no influence at the plant level and therefore would have no way of coping with management QWL initiatives, whereas Italian unions have come to share power with management in some major industries in making decisions at the plant level, so that union leaders can share with management the responsibility of developing such intervention projects.

The second volume of cases and commentary presents a mixed bag of task-force reports, which would be more meaningful to the conference participants than to the rest of us, but it also contains some case reports of importance. As the far-reaching QWL innovations in the Topeka dog food plant of General Foods continue to be the subject of discussion in

the mass media, it is useful to have here "A Case Study of Diffusion" by Lyman Ketchum, the leader of that important project. Ketchum not only describes the project and its process of introduction but also indicates clearly some of the problems of fitting a radical innovation in organizational structure and managerial processes into a large corporate bureaucracy—problems that seem to have led Ketchum and others associated with the innovation to leave the company later. Apparently innovations of this type are likely to be more acceptable to workers than they are to managers with standard business school educations and standard corporation experience.

Since the Italian experience in this field is much less well known than the Scandinavian, it is especially useful to have Federico Butera's contribution, "Environmental Factors in Job and Organizational Design: The Case of Olivetti." Butera not only gives us a systematic description of major projects carried out in this large company but also provides the theoretical basis for predicting that similar innovations will become more common in the future: in companies characterized by complex technology and a dynamic research and development program, changes in products and technology will come so rapidly that the Tayloristic style of management with rigid controls held at the top of the plant and company simply does not permit the flexibility and initiative required by managers, supervisors, and workers to cope with modern industrial production.

Living and Dying at Murray Manor. By Jaber F. Gubrium. New York: St. Martin's Press, 1975. Pp. 221. $8.95.

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This book is perhaps best described as sociologically informed description. Gubrium has the "sociological eye"—the sociologist's particular and peculiar perspective—and he uses it to shape the raw materials of his participant observation study of a Catholic nursing home for the aged. He does not use it, however, to transform perception into conception, to move from description to analysis. If one asks, then, whether this book advances sociology, the answer is no. But if one asks whether, accepted as description, it does its work with sensitivity, honesty, and craft, whether it advances our knowledge of the living and dying of the institutionalized aged, the answer is a resounding yes.

Living and Dying at Murray Manor is good and fruitful description, I think, precisely because it is sociologically informed. Gubrium begins not with the standard lay questions emerging out of the clichés of moral outrage, not with the questions of should or should not. He begins, rather, with the scholar's dispassionate questions of what and how. In his own words, from the preface, the book "examines the social organization of

care in a single nursing home. . . . My working question . . . was, 'How is care in a nursing home accomplished by those people who participate in its everyday life?' " This is in contrast to the perspective from which life in a nursing home automatically means "decay, cruelty and dehumanization. . . . Although it is true that some aspects of life in nursing homes match these stereotypes, life at Murray Manor is filled with intimate social ties, the celebration of small accomplishments, agonizing losses, boredom, conspiracies, anger, pride, humiliation, trust, love, hope, despair—in short, all the complexities that occur when a group of people spend their daily lives together."

As Gubrium describes them, these people who are "spending their lives together" actually constitute three separate groups, three separate worlds, segregated often by place, primarily by meanings. There is the world of top staff—the administrator, medical director, social worker, and so forth —with its focus on "patient care" (chap. 2). There is the world of floor staff—registered nurses, licensed practical nurses, and aides—with its focus on "bed and body work" (chap. 4). And there is the world of the clientele—both residents, for whom Murray Manor functions more as a "home," and patients, for whom it functions as a hospital—with its focus on social ties (chap. 3) and the various strategies for "passing time" (chap. 5). It is the ongoing separate existence of these worlds as well as their occasional physical and symbolic collisions (chaps. 1 and 6, especially) which constitute the prime motif and the major dynamic of living and dying at Murray Manor.

I have said that the author's stance is predominantly a dispassionate one. This is a generally true statement, but not entirely so. The moral impulse dies hard, especially when it confronts hierarchy. In recent years, sociologists as a group have shown themselves to be almost constitutionally incapable of truly empathetic portrayals of persons in superordinate positions. An apparently very strong identification with the presumed underdog inevitably transforms top dogs into cardboard villains. Gubrium has not escaped the "curse" of his colleague group. Top staff are clearly the "bad guys" here. Their world and their world only is viewed through the screen of mockery. Mild mockery, to be sure, but mockery nonetheless. Only in describing top staff does the author block the reader from "taking the role of the other," from seeing Murray Manor through their eyes. As a consequence, the descriptive power of the book is weakened, although certainly not inordinately so.

Despite this flaw, *Living and Dying at Murray Manor* is, as I suggested above, a good and fruitful descriptive account. It is good because it is largely dispassionate and sociologically informed. It is fruitful because it is good. Together with the descriptive as well as more analytic work of such scholars as Kathy Charmaz, Barney Glaser and Anselm Strauss, Elizabeth Gustafson, Arlie Hochschild, Jerry Jacobs, Victor Marshall, and David Sudnow, this book makes increasingly possible the emergence of a truly comparative, analytically sophisticated, and empirically grounded under-

standing of health-care institutions and of the dynamics of aging in advanced industrial societies.

A Nation of Behavers. By Martin E. Marty. Chicago: University of Chicago Press, 1976. Pp. xi+239. $8.95.

Richard Quebedeaux
United Church Board for Homeland Ministries

By the time this review is published, Martin E. Marty's latest book, *A Nation of Behavers,* will have received numerous enthusiastic reviews in the popular religious and secular media. The author, an eminent professor of the history of modern Christianity at the University of Chicago, and no stranger to the academic community at large, probably is better informed about the American religious scene than any of his colleagues in the field.

In *A Nation of Behavers,* Marty constructs a new "map" of American religion, a sociocultural "geography of religion," so to speak. He argues convincingly that what most distinguishes one religious grouping from another is what people *do* (their social behavior), not primarily what they say or believe. We are a nation of behavers more than believers. In the midst of a secular, pluralistic society, most Americans still use religion as an important means of establishing identity and social location.

Insisting that "observers of American religion regularly need to map the terrain" (p. 18), Marty locates and elaborates six clearly defined religious groupings perdominant in American life. In presenting his taxonomy, the author defends his right to write "contemporary history"—noting the objections raised by traditional historians—because the mapping of today's religion, he feels, must not be relegated to "journalists, feature writers, television communicators, friends and neighbors, pollsters, anthropologists, and sociologists" alone (p. 19). And in so doing, he admits that the exercise of "describing the landscape and its landmarks will not save souls or make sad hearts glad. But it can contribute to a sorting out process that will be helpful to both participants and observers" (pp. 18, 19).

Marty first discusses "Mainline Religion" as the central, but now very bland, landmark in American religious life—the Protestant-Catholic-Jewish reminiscence of an older establishment mapped most effectively in Will Herberg's classic study, *Protestant-Catholic-Jew* (Garden City, N.Y.: Doubleday, 1955). Mainline religion has been thought of as "the traditional, inherited, normative, or median style of American spirituality and organization, over against the 'marginal' or 'fringe' or 'curious' groups that drew so much attention in the story-telling of the late 1960s and early 1970s" (p. 53). Marty accepts the contention of Dean M. Kelley in his *Why Conservative Churches Are Growing* (New York: Harper & Row, 1972) that mainline liberal churches, marked by relativism, diversity, tolerance, lukewarmness, and individualism, are losing ground to conservative (i.e., "strict") churches, characterized by absolutism, conformity, dis-

cipline, commitment, and missionary zeal. Mainline religion provides few reasons for adherence today; thus, Americans have begun "to take their religious quests to other locales on the map" (p. 79)—associations that do offer meaning in the midst of affluent boredom, strictness in a permissive society, and a sense of "belonging" ("community") amidst widespread social alienation. These religious groupings are the subject of much of the rest of the book.

The author's second religious cluster is "Evangelicalism and Fundamentalism," two closely related schools of Protestant orthodoxy grounded in an affirmation of the final authority of Scripture in all matters of faith and conduct, the necessity of conversion to Christ, and the mandate for evangelism (recruitment). Of the two schools, fundamentalism is the stricter and the more "cultic," anti-intellectual, and culture rejecting—in a word, less middle class in its constituency and orientation. Evangelicalism, fundamentalism's more moderate derivative and counterpart, has had particular success of late, because evangelicals offer their "converts" the best of Christ *and* culture. "On the one hand, they provide meaning, belonging, and identity apparently *over against* other Americans while on the other hand they are taught to *fit in with other Americans,* to be the real and true citizens" (p. 105).

Marty's third religious grouping is "Pentecostal-Charismatic Religion." Pentecostalism is an enthusiastic form of Christianity centered on an experience, "Spirit-baptism," and the present-day operation of "spiritual gifts" (Greek *charismata*)—including speaking in tongues, divine healing, and prophecy—in the life of the church. Since the early years of this century, and until quite recently, Pentecostals have generally been confined to their own "sectarian" (and working-class) denominations and independent churches. But since 1960, the experience of spirit-baptism has spread to the "historic" mainline denominations as "charismatic renewal" in the context of their distinctive middle-class forms of worship and life-styles. Thus Pentecostals still tend to be the "fundamentalists" of the grouping, while Charismatics are the more middle-class "evangelicals." The religious experience is the same, but the subcultures are different.

The author's fourth grouping, "The New Religions"—from Jacob Needleman's book of the same title (Garden City, N.Y.: Doubleday, 1970)—encompasses the non-Judeo-Christian, non-Western religious traditions that borrow freely from Asian, African, metaphysical, and occult sources (e.g., Hinduism, Buddhism, Sufism, Baba-lovers, Subud, Transcendental Meditation, astrology, Scientology, and neopaganism). His fifth cluster is "Ethnic Religion," the direct result of a new sense of "peoplehood" among numerous ethnic and racial minorities. Peoplehood may be understood as "the quality or state of constituting a people; *also:* awareness of the underlying unity that makes the individual a part of a people" (p. 169). Ethnic religion elevates the symbols of a people's racial or ethnic heritage to the level of "ultimate concern." Marty's final grouping is "Civil Religion," made famous by Robert N. Bellah—the integrative efforts at producing

a faith for the whole American society. Unlike evangelicalism and fundamentalism, Pentecostal-Charismatic religion, the new religions, and ethnic religion, however, civil religion (in the author's opinion) has little real importance on the map. It is a debate largely among the intellectual elite for whom this book is written (which is why it gets a whole chapter). "It plays little part in social or political history" (p. 190). And the controversy surrounding Bellah continues.

A Nation of Behavers is well conceived, well written, timely, and even provocative. Marty gives more than adequate attention to the best historians, sociologists, psychologists, and theologians whose works bear upon his general topic and specific religious groupings. Although a new map will, no doubt, have to be constructed in the not-too-distant future, this one will surely suffice for the present. It is a brilliant essay and must reading for all (including sociologists) who wish to understand the nature and function of religion in contemporary American society.

Division in the Protestant House. By Dean R. Hoge. Philadelphia: Westminster Press, 1976. Pp. 166. $3.95 (paper).

Hans Mol
McMaster University

The "division" of the title of this book is the theological separation between Evangelicals and liberals in the American churches. Dean Hoge draws the dubious analogy between this division and a historian's distinction between private (soul saving) and public (social witness) Protestantism, as though personal salvation does not have considerable social effects (responsibility? reliability? humility?) and a theology dealing with social amelioration does not have consequences for personality integration. He is on much more solid ground when he bases his distinction on his own research (three rather disparate studies: one in the Philadelphia area in 1970, one of New Jersey United Presbyterians in 1972, and one of a 1973 national sample, again of United Presbyterians). From the last he checked by means of regression analysis the relative importance of theological factors as compared with group interest, institutional interest, psychological, and age factors and discovered that the "theological factors turned out to be by far the most important for all categories of respondents" (p. 70). In other words, the theological position of Hoge's respondents predicted their choices about other matters more closely than any other set of factors. That is of course the way it should be. Any meaning system worth its salt should have a decisive effect on perception, interpretation, and attitude, whether it be Christian theology, Marxist sociology, or scientific humanism.

So far so good. Yet in his analysis of the New Jersey data Hoge seems

to come close to saying the opposite: "Persons who feel that their station and style of life are either actually or potentially threatened by hostile social forces want the church to provide a safe haven and to stay away from the types of social action that they assume would bring further change. Persons who feel more secure are more likely to advocate church social action" (p. 73). Does this then mean that theological position is after all a response to feeling threatened? No, says Hoge: "For the most part, theological statements are not simply rationalizations for psychological or group interest factors but are independent according to our analyses" (p. 89).

The problem seems to be that whereas in the 1973 research, theological views were quite independent of social class and occupational and educational factors, in the 1972 New Jersey study 18% of the variance could be explained by status concern, feeling of social threat, occupational level, and education. And yet is resistance to the church being engaged in social action "directly related to whether the particular action seems to uphold or disrupt what might be called a middle-class way of life[?] Our finding is that this is indeed a basic factor, regardless of theological or other influences" (p. 88).

In his conclusion Hoge puts it even more strongly: ". . . the strongest middle-class commitments are to family, career, and standard of living, plus health whenever it appears in danger. For the typical Protestant church member these interests are so strong that church commitment is largely instrumental to them and contingent on whether the church appears to serve them" (p. 120). There was little, if anything, in even Hoge's New Jersey survey which could have tested and correlated these various commitments, but the author bases the observation on a review of the literature.

How do we reconcile this strong independence of the theological variable with church commitment being instrumental to middle-class interests? Simple: "The Protestant house is divided in two ways. At times the main conflict seems to be along one line, and at times along the other. Since an effective argument can be made more easily on theological grounds than on the grounds of class interests, the theological arguments have been the most used in public, and they have sometimes been used to defend, or to camouflage, class interest considerations" (p. 121). So Marx is right after all: behind theology are class interests! And I must be wrong in assuming that Americans are good at camouflaging both theological and class interests!

But what about the *independence* of the theological variable in the research? I am afraid I have to leave the answer to this exciting question to those readers who have by now become interested in the book. If the latter has anything more to say about the intricate interaction between theology and society (not just class interest) it has escaped me twice.

The clue to my rising dissatisfaction with the book is to be found in

the penultimate paragraph of the book where the author describes his sociological analysis as couched "in one-story abstractions hovering above concrete events" (p. 133). The analogy here with the hovering closeness of the mythical dream world of the Australian aborigines to their actual situation is striking. So is the analogy between a comprehensive, abstract, sophisticated, sociological frame of reference in which latent factors are sensitively intertwined with, for instance, Thomas Aquinas's *Summa Theologica*. The trouble is that for us the primitive is less relevant.

Yet Hoge has some interesting hunches. In his last sentence he mentions that neo-Evangelicalism could be creative and redemptive for the divided Protestant house. He is probably right. Yet he does not provide the reader with a good sociological reason why this should be so. It may be that there is an interdependence between theology and situation. It may be that an independent/traditional theology still has a surprisingly good fit with a pluralistic society. It may be that a theology which has a fixed point of reference in the past (revelation) and a fixed point of reference in the future (the parousia) balances an unhinged present. It may be that a theology which revolves entirely around the sin/salvation or crucifixion/resurrection dialectic dramatizes for moderns their actual situation of oscillation between change and stability or breakdown and wholeness. It may be that a fundamental commitment to this dialectic has equally fundamental, but latent, consequences for personal, group, or social identity. It may be that neo-Evangelicals combine two basic religious functions: the maintenance of a straddling meaning system and the sensitive reconciling of social injustice. Perhaps sectarians/Evangelicals do make good use of the low level of integration of a pluralistic society and form buffers between social anomie and personal alienation.

But Hoge does not deal with any of these possibilities because he hovers so closely above his data that he shortchanges his analysis and therefore cannot see the forest for the trees. The tragedy is that this kind of analysis of surveys (which do not hang together to start with) has no fructifying potential. It does not open new paths for research. A good sociological or theological imagination could have compensated somewhat for this defect, but Hoge does not give evidence of having much of either. And yet his style is bright enough to make me want to read his next book, provided it starts where this one leaves off.

As it is, I found this book immature, almost as bad as my own work 15 years ago. Hoge has rushed into print instead of presenting us with a well-thought-out, well-rounded piece of research or theorizing. If a theologian were to ask me to recommend to him a good study in the American sociology of Protestantism, I certainly would not recommend this one. And come to think of it, I cannot find much else to recommend it. The American variety of the sociology of religion in its present state seems to me to contain a lot of bright-eyed mediocrity, appalling ethnocentricity (few people nowadays seem to read French or German, with the result that the

significant European contributions are disregarded completely), latent inconsistency, Marxist superficialities, hopeless insensitivity to sophisticated theological questions, a-theoretical naivité, and last, but not least, much too much intrinsic questionnaire bias to be of much use to theologians, or even the clergy. And I am afraid Hoge's *Division in the Protestant House* is not an exception.

Contemporary Transformations of Religion. The Riddell Memorial Lectures Forty-fifth Series, Delivered at the University of Newcastle upon Tyne on 2, 3, 4 December, 1974. By Bryan Wilson. London and New York: Oxford University Press, 1976. Pp. ix+116. £2.95.

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University of Waterloo

Listeners at Newcastle to these 1974 Riddell Memorial Lectures must surely have felt the cold darkness fall. Bryan Wilson's account of Western civilization's plight is harrowing beyond words. His presentation, altogether without dramatic effect, is dramatic in all it conveys. Secularization, he tells us, has banished the benefits that only religion can give, and all we have in lieu is the unencumbered youth playing in cults at the most serious thing in life.

Bryan Wilson's long immersion in the study of religion provides the point of view and knowledge for this penetrating diagnosis. Others have told of the radical transformation under way in society that can be glimpsed through the study of the economy. It may be a new experience for some to do their viewing through a study of religion. Yet, as Bryan Wilson shows it, the transformation in society at large to be glimpsed through that window is very much the same.

Secularization is the dominant change upon us, in Bryan Wilson's view. This change represents the infusion of life with rationality and is most patently seen in the elaboration of technology, in bureaucratic organization, and in the network of impersonal roles through which our everyday encounters now unfold. Entrapment in a milieu like this is itself alienating. Besides this, traditional religion is not capable of legitimating what is done there. For such reasons, received religion has been losing its hold in the Western world over a very long period. In the first of the three lectures Bryan Wilson reviews some of the data on this decline that he has been collecting for a number of years.

For the sake of eventually measuring their significance for religion's survival, the new religious movements in both the West and the Third World are examined in the second lecture. A curious balance of trade in religion is brought to light here. Christian sects deriving from the West are thriving in the Third World and are producing the kind of transformation of personal character that modernization requires. Kimbanguism

in Zaïre, Pentecostalism in Chile, and the Jehovah's Witnesses in Kenya are considered in illustration. Meanwhile, Westerners alienated from Christianity are drawing inspiration from primitive or oriental practice in many of their new cults.

But the third lecture shows these cults to be far too superficial and ephemeral to supply the dynamic for new religion. They are, rather, somewhat inconsequential protests against the violence done to our humanity by secularization. The individualism and hedonism of certain ones, for instance, constitute a protest against this process of dehumanization. Three types of cult are distinguishable: gnostic-type cults in which salvation comes through acquiring particular knowledge (illustrated by Scientology), cults such as Human Potential which seek the release of powers within the self, and cults which impose a virtuoso discipline in a retreatist community (illustrated by the Divine Light Mission of Maharaj Ji). Notwithstanding their great variety, none gives evidence of being able to integrate a whole culture and social order in the way a religion does. For this reason Bryan Wilson concludes that it is unclear whether religion has any prospect of rebirth in the modern world.

This book exhibits the qualities we have come to expect in Bryan Wilson's work: honesty, breadth of view, sympathy with religious sentiment, richness of illustration, responsible and careful reasoning, and clear and fluent writing. Yet one thing I found jarring is the author's tendency to use the terms "sect" and "cult" interchangeably, particularly since this assignment gave special opportunity to make discriminations by means of them that we find the author making without them. He says, for instance, that the movements examined from the Third World might be classified with early Methodism, but not those examined from the West. But is this not because the former, like early Methodism, are sects, whereas the latter are cults? The distinctively cultic character of these is something he belabors in his conclusion anyway. He points to the fact that in making their respective goods ultimate they give exaggerated importance to elements that have had a more proportionate place in our culture.

Concerning religion's future, it seems fitting that Bryan Wilson, in keeping with his usual caution, should be skeptical. Nevertheless, he does seem to imply a bleak rather than a hopeful prospect. This comes from leaving us to assume both that capitulation to secularization will progress without limit and that transient cults are all that the vacuum will breed. But are there not more forces at work than are shown in this black-and-white sketch? Is it not possible to admit the battering taken by the traditional churches and still concede that some of them are making effective adaptations that diminishing support could not index? Do we not have still to reckon with the thriving Christian sects as much in the West as in the Third World? Is it not possible that among the ephemeral cults a more durable religion might arise, similar to the way Christianity emerged from a sea of ephemeral cults, some of which it resembled?

The New Religious Consciousness. Edited by Charles Y. Glock and Robert N. Bellah. Berkeley: University of California Press, 1976. Pp. xvii+391. $14.95.

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Marquette University

This 16-chapter report of a 1970–74 project focuses on investigations of the youth counterculture in the San Francisco Bay area. The book includes ethnographic studies of three new religious movements in the Asian tradition—the Healthy-Happy-Holy Organization (by Alan Tobey), Hare Krishna (Gregory Johnson), and Guru Maharaj Ji and the Divine Light Mission (Jeanne Messer); three new quasi-religious movements—the New Consciousness and the Berkeley New Left (Robert N. Bellah), the Human Potential Movement (Donald Stone), and Synanon (Richard Ofshe); and three new religious movements in the Western tradition—the Christian World Liberation Front (Donald Heinz), Catholic Charismatic Renewal (Ralph Lane, Jr.), and the Church of Satan (Randall H. Alfred). Additional chapters deal with church-sponsored student ministries (Barbara Hargrove), reactions of three church congregations (James Wolfe), Jewish identity in relationship to the counterculture (Thomas Piazza), survey data on the new religions in social context (Robert Wuthnow), historical analysis of religious change in 19th-century America (Linda K. Pritchard), and conclusions by each editor.

It is assumed that "the place to read the future . . . is where it is beginning," in other words, the bay area; the religious dimension was chosen "as the strategic point of entry into the question of contemporary cultural transformation because we thought it potentially the most profound level of change" (p. xiii).

Each of the nine ethnographic studies was done to determine what appeals to youth in alternative approaches to reality, the alternative states of consciousness sought, and prospects for the group's having a significant and permanent place in American culture. No rigorous scientific procedure was used in selecting the groups for study, nor was any particular set of guidelines followed in the fieldwork and interpretive analyses. Most of the studies include some participant observation besides exploration of literature, interviews of leaders, examination of published materials about the group, and other forms of data collection. Descriptive information and interpretive perspectives emerge which can be of help to advanced scholars of the respective groups and also serve as an introduction for persons who have no previous knowledge of them.

Conclusions generally were adopted cautiously as tentative and subject to correction upon gaining the perspective of additional experience and historical events. An effort was made to adopt a nonexploitative attitude toward those who were studied, neither debunking the groups nor subjecting them to sensational publicity. Therefore, subtle, nonrational messages

of sympathy and antipathy unintentionally communicated by the authors are less evident than in most studies of religion.

The 1973 survey data reported by Wuthnow from a sample of 1,000 persons age 16 and over in the bay area focus on 13 contemporary religious and quasi-religious movements. Only a small proportion of the population had been attracted to and taken part in each. Countercultural groups (TM, Hare Krishna, Zen, yoga, and Satanism) were the most widely known, yet only about one in three persons professed to know something about them. Of the personal growth movements, Synanon was the best known with 8% professing to know at lot and 44% a little about it. It is followed by Scientology with almost one-fourth professing to know something and *est* (Erhard Seminars Training) with only 6% claiming to have some knowledge. The neo-Christian groups (Christian World Liberation Front, Campus Crusade, Jews for Jesus, Children of God, and those that speak in tongues) were even less widely known; only about a fourth claimed to have knowledge of the tongues groups; all others were lower. Except for *est*, most groups overselected persons under the age of 30, but Campus Crusade and Jews for Jesus overselected both the young and persons past age 50.

Whether these groups are mainly youthful experiments that will be abandoned as their participants become more mature or a phenomenon of a new consciousness that will remain for a long time is not firmly predicted. Wuthnow believes that the present religious experimenters may gradually lose interest in these groups as they grow older, get married, take jobs, and settle down. In opting for that conclusion he has adopted an assumption about factors which generate interest in such groups. Pritchard indicates remarkable similarities between the current scene and 19th-century America in which a large number of diverse sectarian groups emerged, many of them subsequently becoming established denominations, yet she concludes that we are possibly witnessing a new phase in the history of American religion and certainly not its demise. Her assumption that "established religion of the 20th century divides primarily over political and social controversies that emerged outside of religious institutions" (p. 328) fails to recognize the important role played by theological orientation (see Dean Hoge, *Division in the Protestant House* [Philadelphia: Westminster, 1976]).

Bellah's concluding chapter, "New Religious Consciousness and the Crisis in Modernity," indicates that the American disturbances in the 1960s were hardly unique, but for the first time mass disaffection from common understandings of American culture and society began to occur with erosion of the legitimacy of major American institutions, including the church. Thus, the crisis of the 1960s was above all a crisis of meaning, a religious crisis. He concludes that of the three most likely outcomes for the future, the revolutionary alternative offers more hope than the liberal or traditional authoritarian. If there can be a major shift in established biblical religions away from alliance with utilitarian individualism and toward reappropriation of their own religious roots, together with open-

ness to needs of the contemporary world, this would provide the mass base for "a holistic reason that unites subjectivity and objectivity" and will make human life worth living in the 21st century (p. 352).

Glock's conclusion, "Consciousness among Contemporary Youth: An Interpretation," presents the thesis that the youth counterculture of the 1960s did not initiate any significant change in American society; rather, it was a highly visible sign of fundamental changes already under way and, most of all, the surfacing of a mode of consciousness that was multicountercultural, not merely countercultural. Old cognitions of the world were being replaced by new cognitions inspired by science, particularly the social sciences. Criticism of existing social arrangements brought also a call for social reform, but efforts at reform delivered much less than promised, "partly because the new cognition was ambiguous about what effective reform would constitute" (p. 363). Because "the end of the open rebellion did not mark the end of the process" of searching for alternative realities, the crucial question is whether any society other than a dictatorship can result when there is no consensus about what constitutes a meaningful life and when so many people have found no answer to the question of meaning (p. 366).

Scholars and researchers will find several paradigms for the analysis of cults, sects, and religious movements in this volume. They will do well, however, to watch carefully for the implicit assumptions and presuppositions that inform the research, whether these pertain to the question of generalizing from the bay area to the entire nation; accepting the frustration hypothesis about religiosity; lumping together countercultural, personal growth, and neo-Christian movements; believing that social change triggers religious reorganization; or other topics.

Only a few casual hints are given of the possibility that the groups studied may not, after all, be bellwethers of the future of American religion. One wonders what this book would have been like if historical continuities in the new religious consciousness had played a significant role as a guiding hypothesis. Is reaffirmation of ancient central values, rather than accentuation of allegedly "new" contradictions of them, the path to personal happiness and peace, the route to group survival and strength, and the road to consensus and order in society? If so, the rapid growth of conservative churches may be a better sign of hope for the future than the survival of countercultural groups.

Inevitably, since final preparation of the manuscript, changes have occurred in the volatile groups reported, so many details presented must be accepted as time bound. For example, the Christian World Liberation Front recently experienced a friendly division from which at least two significant groups have emerged to replace the old coalition.

Errors are inevitable in such a complex, wide-ranging volume. Thus, the Protestant Pentecostal movement initially did not attempt to convert people to a new sect (p. 176); the Westminster Confession anchored the Presbyterian but not the Anglican church (p. 302), and it is misleading to

talk about a "biblical arrogance toward nature" (p. 340) except from the perspective of a few groups with warped biblical interpretations.

Along with other significant recent works, like Richard Quebedeaux's *The New Charismatics* (Garden City, N.Y.: Doubleday, 1976), Ronald Enroth's *Youth, Brainwashing, and the Extremist Cults* (Grand Rapids, Mich.: Zondervan, 1977), Irving I. Zaretsky and Mark P. Leone's (eds.) *Religious Movements in Contemporary America* (Princeton, N.J.: Princeton University Press, 1974), and J. Gordon Melton's *A Directory of Religious Bodies in the United States* (New York: Garland, 1977), this study provides a wealth of knowledge, insights, and understanding of the new religious movements and their relationship to ongoing churches, denominations, and sects in the United States.

Hunger for Experience: Vital Religious Communities. By John E. Biersdorf. New York: Seabury Press, 1975. Pp. xiii+174. $7.95.

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University of Wisconsin—Madison

Hunger for Experience presents the findings of an ambitious research undertaking, rather portentously named "Insearch: The Future of Religion in America." The topics studied are broad and massive in their ramifications; unfortunately, the techniques employed are inadequate to address them. John E. Biersdorf begins by noting the contemporary importance of religious movements which seek to experience and be guided by the immediate presence of God. His goal is to "define and analyze religion of experience, illustrating it from the groups in the study, and . . . to suggest what religion of experience implies for the future of institutional religion" (p. 10). The author's perspective is that of a mainstream Protestant church official; indeed, he was executive director of the Department of Ministry of the National Council of Churches at the time of the project's inception.

Noting that the larger, more "liberal" churches have shown stasis or even loss of membership and gift income, while the reverse has been true of many smaller (often "conservative") denominations, Biersdorf suggests that an examination of the "growing edge of religion in American life" is in order. Before looking specifically at some of the groups constituting this "growing edge," he outlines the operation of the "modern individuality boom" which provides the impetus for experience-seeking religious participation.

While the description of this "boom" will be familiar to most sociologists, it is potentially valuable to those without sociological background. The author summarizes the pluralist-voluntary nature of American religious enterprise, offers some social psychological discussion of change and of the "consumption of meaning symbols," and looks at values in terms of

their content and the process of their construction. He suggests that, as we anticipate the social and cultural changes that will confront us in the future, we can expect that "the groups which are addressing the problems raised by the projected changes will probably have a corner on the future of religion" (p. 31). At this point we get a sense of the intended strategic value of the upcoming empirical presentation.

The "promising religious communities" studied by Insearch were nominated by a panel of ministers, scholars, and religious executives and leaders. Organizationally, each group was classified as a congregation, an alternative community, or a special agency. Thirty-six groups participated in the study; there was an attempt to stratify the selection by taking into account mission focus, social composition, and region. While Biersdorf admits that his groups do not amount to a random sample, he is unable to offer an adequate conceptual justification for the use of this particular aggregate.

A cluster analysis is then performed on each group's "record of goals, meaningful experiences, and major activities" (p. 39) as reported by individual members to yield a series of dimensions representing the shared emphases of groups in each of six clusters. For each dimension, such as "transforming society through faith" or "the development of personality," the author presents a description of a relevant group. These vignettes and the many others presented in the book are somewhat interesting but offer little systematic insight.

Even more disappointing than the surfeit of anecdotal material is the effort to impose statistical techniques on information that does not seem well suited for such analysis. Biersdorf's use of factor analysis cannot but be confusing to the general reader as well as unsatisfying to those familiar with the technique. Group vignettes are interspersed among tables which sort into factors the values of individual group members concerning intimacy, social issues, religious discipline, and interpersonal relations.

The final chapters address the future of organized religion. The author presents a concise exposition and then a critique of Dean Kelley's *Why Conservative Churches Are Growing* (New York: Harper & Row, 1972). Kelley analyzed membership and financial giving as indicators of church strength; Biersdorf notes that religious participation can involve more than organizational activity. Interpersonal relationships and personal religious experience and discipline must be examined as well. Conservative churches are growing, the author concedes, but "other groups are also having an important impact, although it is hard to prove this statistically because they offer individuated meaning-laden experiences" (p. 108).

Hunger for Experience, though designed as an empirical study, is unsuccessful as a research effort. The redundancy of the descriptive material, the confusing use of statistical measures, and the lack of an index limit the usefulness of the book, despite the presence of some well-articulated theoretical presentations.

Realities of Social Research: An Empirical Study of British Sociologists. By Jennifer Platt. London: Sussex University Press, 1976. Pp. 223. £6.00.

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This study of the sociology of social research aims to go beyond the simplistic accounts normally provided in methodological textbooks and to place the social research process in its systematic context. The author's purpose is described as threefold: "to satisfy a simple ethnographic curiosity; to provide data and suggest interpretations that may help to advance theory in such areas as the sociology of work and of education; and to suggest sociologically realistic prescriptions for the conduct of successful research" (p. 10). Only the first of these objectives has been achieved satisfactorily. The study makes virtually no contribution to theory. Interpretations of the data are largely ad hoc and sometimes little more than speculative. In her conclusion, the author admits that giving colleagues the benefit of her recommendations for the conduct of future social research no longer seemed realistic because the crucial features of the social organization of sociological research in Britain were beyond the control of the individual research director (p. 178).

Among the factors which appeared to influence the conduct of research were the state of the labor market for sociology graduates, the grant policies of universities and funding agencies, the norms of the university system, and a number of other factors which may have been unique to British sociology in the late 1960s. After years of neglect, there was a rapid increase in the number of departments of sociology at British universities and in the number of students. Many of those appointed to both senior and junior positions were relatively inexperienced in empirical research.

In an Appendix, the author gives an account of how the study was initiated and carried out. Altogether, 121 unstructured interviews were conducted concerning 55 research projects, of which 11 were not university based. Although not systematically selected, the sample of projects appears to have been quite representative of those being undertaken in Britain in the period from 1965 to 1972. Among the topics covered in the book are the influence of external granting agencies on the nature and conduct of research; university organization and the conflicting demands of teaching and administration, particularly of research directors and other senior personnel; variations in team organization; the division of labor and structure of authority; the relationship between research and careers; and the interaction of the research project and the private lives of those involved. Each of these topics is covered with some quantitative but mainly qualitative evidence derived from the interviews, from which extensive quotations are taken.

Among the conclusions drawn by the author is that bureaucratic forms of organization are more compatible with survey research, while partici-

pant observation and unstructured interviews go better with a relatively egalitarian division of labor. She draws attention to the sources of friction which may arise under a variety of conditions and emphasizes that not all conflict is necessarily dysfunctional in the research situation. She notes that many of the projects took an excessive amount of time and some remained unfinished. The more successful ones were generally those directed by full-time researchers and were conducted outside universities or had only a small number of people involved. Only systematic comparative studies in other countries would enable one to determine whether this is a universal pattern, but my experience suggests that the same would probably apply in Canada. One conclusion not drawn by the author, but justifiable in the light of the evidence, is that the contemporary multiversity, dedicated to the mass education of undergraduates, may no longer be the appropriate vehicle for the conduct of research.

Transitions over the Life Course: Lessons from Age-Set Societies[1]

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This study examines processes of life-course transitions in 21 African age-set societies, a group of preliterate societies where age is a major organizing principle, and compares processes of transition there with those in the United States today. The analysis challenges some long-held views about the putative smooth course of passage through the life course in "simple" societies and troubled transitions in the United States. Despite the contrasting societal contexts, in both types of society transitions constitute immanent sources of tension: for the individual, conflicts between old and new roles and for the society, competition for rewarding roles. But in both contexts mechanisms emerge that serve to mitigate some transition difficulties. Even the processes of transition themselves are subject to change as a result of individual and societal adaptations to transitional problems and to social and environmental change. Some theoretical implications of these findings are explored.

The life course of individuals everywhere is punctuated by transition points—the relinquishment of familiar roles and the assumption of new ones. Much sociological analysis of these life-course transitions has emphasized consequences for the individual, especially the difficulties of moving from one set of roles to another within the context of contemporary Western societies (e.g., Benedict 1938; Brim 1976; Rossi 1968). Yet life-course transitions are more than an individual matter; they involve an interplay between the individual and what he or she confronts as society or the social structure. The historical and cross-cultural variability of transitions supplies ample evidence that they are not a simple result of developmental processes but, instead, reflect specific kinds of

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institutional structures. In this paper, we seek to develop this broader approach to life-course transitions as a framework for reexamining some of the familiar notions about transition processes. Our analysis utilizes the conceptual framework of age stratification. Within this perspective, age and aging have significance not only as properties of the individual but also as essential components of social structure and change.

We consider three questions: How do life-course transitions affect the society as well as the individual? What adaptations are made to meet the societal and individual problems generated by transitions? What are the sources and nature of changes in transition processes? These questions have already been raised by age-stratification theorists with respect to Western industrial societies (e.g., Riley 1976; Riley et al. 1969; Riley, Johnson, and Foner 1972; Foner 1975; Riley and Waring 1976). We seek to elaborate the general understanding of life-course transitions suggested by these writings through systematic analysis of contrasting societal settings. In these pages, then, we explore the questions raised above by focusing on a sample of preliterate African age-set societies and by comparing transitions there with those of contemporary American society.

We have chosen a sample of African age-set societies because, in a number of key aspects, they present a striking contrast with contemporary society in the United States. They are relatively small scale, technologically undeveloped, and preliterate. Because they utilize age as a major element in their social organization, their age-grade systems are particularly explicit. Just because age norms are so definitely institutionalized and stand out clearly in these societies, their age-stratification systems are better documented than those found in most other preliterate populations. Thus they provide us with some of the most complete data on life-course transitions available for preliterate societies.

Our findings both extend previous analyses and challenge some long-held views: Life-course transitions are neither as simple in age-set societies nor as intractable in our own society as stereotypes suggest. At the societal level, transitions represent an inherent source of tension and conflict in both complex industrial and "simple," age-set societies. And in both types of societies, individuals face problems in making transitions. Finally, both individual and societal responses to the problems of life-course transitions tend to change the very nature of the transitions.

SAMPLE AND DATA

Our criteria for designating an age-set society as such include the following: named groupings of males[2] based on age or generation; publicly rec-

[2] In most societies the organized age system involves only males; formal age systems spanning the entire adult lives of females are rare. The implications of distinct systems

ognized membership in these sets; duration of these groups, once they are formed, for the bulk of the life course; membership entailing the allocation of significant social roles; joint movement of age-set members from one age grade to the next; and differential social rewards accorded to the various age sets, based on age grade occupied.

Formal age-set systems that meet these criteria are relatively rare in human societies. Of the 547 cultures surveyed in the *World Ethnographic Sample,* only 23 were found to contain age-set systems, most of them in Africa (Coult and Habenstein 1965, p. 27). Our analysis is based on 21 African societies having age-set systems, 14 of which are based entirely on age and seven of which have an important generational[3] element in the determinaton of set membership.[4] These were selected as the societies in Africa having adequate documentation of their age-set systems. The existence of age-set systems in a number of other African societies has been noted in the ethnographic literature. None of these reports, however, provides sufficiently detailed data for our analysis.

Our analysis of age-set societies is based on the original ethnographic reports.[5] In most instances we have had to rely on the description of an age-set system provided by a single ethnographer. In the majority of these cases, however, reports by others have provided useful background

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of transitions for females and the way the male and female age systems mesh are discussed in an unpublished paper by one of the authors (Kertzer).

[3] A person may be assigned to set membership by virtue of his age (whether based on some social, physical, or chronological criterion), or on the basis of genealogical factors. In the latter case, a common rule is that a boy should belong to the set following that of his father. Few societies follow such generational principles strictly. Most societies having generational components to their age-stratification systems compromise the generation principle to a greater or lesser extent in order to achieve some age homogeneity of sets. It is for this reason that generation-set systems are included in our analysis.

[4] The ethnographic present is used throughout. Many of the age-set systems referred to have, in recent years, been severely modified in response to political and economic pressures.

[5] The 14 age-set systems and the primary sources relied on for their documentation are: Afikpo Ibo (Ottenberg 1971); Arusha (Gulliver 1963); Kikuyu (Kenyatta 1938; Lambert 1956; Middleton 1953; Prins [1953] 1970); Kipsigis (Peristiany 1939; Prins [1953] 1970); Latuka (Seligman and Seligman 1925; Seligman 1932; and unpublished field notes by Oker B. B. Madison); Masai (Jacobs 1958; Bernardi 1955; Fosbrooke 1948); Meru (Holding 1942; Lambert 1947); Nandi (Huntingford 1953); Nuer (Evans-Pritchard 1936, 1940); Nyakyusa (Wilson [1941] 1963, 1949); Rendille (Spencer 1973); Samburu (Spencer 1965, 1973); Tiriki (Sangree 1965, 1966); and Turkana (Gulliver 1958). The seven generation-set systems are: Borana Galla (Legesse 1973; Prins [1953] 1970); Jie (Gulliver 1953); Karimojong (Dyson-Hudson 1963, 1966); Konso (Hallpike 1972); Kuria (Ruel 1958); So (Laughlin and Laughlin 1974); and Zanaki (Bischofberger 1972). All but one of the 21 societies are found in East Africa: nine in Kenya, four in Tanzania, three in Uganda, two in Sudan, and one in Nigeria. Half (11) are pastoral, primarily engaged in cattle herding, while eight have a primarily agricultural economy and two have mixed agricultural herding economies.

material on the society in question. It is also of interest that studies of about half the societies in our sample have been published since the last major sociological analysis of age-set societies was undertaken (Eisenstadt 1956).[6]

THE AGE SYSTEMS OF AGE-SET SOCIETIES

All societies have some form of age stratification: that is, age is one basis for allocating social roles and for drawing important social divisions. Age-set societies are distinctive in the extent to which their age-stratification systems are formalized and the degree to which age provides a major basis for role allocation in the society.

The Structure of Age Systems in Age-Set Societies

When we speak of a system of age stratification as a general phenomenon, we are referring to a societal structure of socially recognized divisions based on age—that is, age strata. Members of each age stratum are alike in chronological age or life stage and have roles in the society that are in some sense age specific. What from a dynamic point of view are the major transition points over the individual's life course, are from a structural perspective the boundaries that distinguish one age stratum in the society from another. In our society we tend to recognize several strata: childhood, youth, middle age, and old age;[7] in age-set societies the number of socially defined age grades (age strata) varies from as few as two to as many as eleven. For example, the Latuka of Sudan demarcate five age grades: children, youths, rulers of the village, retired elders, and the extremely aged. Whereas in our society the occupants of an age grade do not have a well-defined organization, in age-set societies individuals occupying the major age grades tend to act jointly, often as corporate groups. It is important to distinguish here between "age set" and "age grade." The latter term refers to socially recognized life stages (e.g., youth, warrior, elder), each associated with a variety of appropriate roles. It is a term we shall reserve for describing age-set societies rather than the more general term "age strata." The term "age sets" refers to named groups of people who are assigned joint group membership as a result of their similarity in age and who proceed together at culturally prescribed intervals of transition from one age grade to another. As we

[6] Since the present article was written, a comparative study of age-set systems has been published (Stewart 1977).

[7] Of course, the age-stratification system in the contemporary United States is a good deal more complex than is suggested here. For a more detailed discussion, see Riley et al. (1972, esp. chap. 10).

shall note below, age sets constitute a special case of the more general term "cohort."

The term age "stratification" refers to the fact that age is a basis of structured social inequality. The age strata are differentially rewarded. In our society, for example, most old and young do not have access to the most highly rewarded economic roles, and the middle aged have more political power than the young. Age-based inequalities seem more apparent in age-set societies. In many age-set societies greater rewards are bestowed on members of each successively older age grade. But not all age-set societies accord the members of the older age grades higher prestige (or power); in some, it is relatively young men who receive the highest social rewards. Among the Kipsigis of Kenya, for instance, of their three age grades, boyhood, warriorhood, and elderhood, the warriors have the greatest prestige and personal freedom (Peristiany 1939, pp. 31–36).

In modern Western societies a person's chronological age is but a partial clue to his social location—the roles she/he is likely to fill and the rewards she/he receives. For example, class and ethnic stratification crosscut lines of age stratification; they are a source of variations in age norms and provide other important sources of identity for the individual. In age-set societies, although bases of social inequality other than age are operative, the age system is more definitive. Individuals become part of the formal age-stratification system as members of an age set. Thus to know which age grade an individual occupies (together with the other members of his age set) is to have a fairly clear idea of his socially defined roles and tasks, his rights and responsibilities, his relations with age peers and age dissimilars.

Dynamic Elements in Age-Set Systems

Individual aging from birth to death involves physiological, psychological, and social changes to which the individual must adapt. Life-course transitions are but key points in the aging process, entailing marked changes for the individual. They are the socially defined stages (although often distinguished also by physiological changes) at which individuals move from one age stratum to another, give up one set of roles and take on a new one.[8] In age-set societies the dramatic changes individuals undergo are more obvious than in our society because transitions are generally marked by formalized procedures and accompanied by considerable public

[8] For a classic account of the rituals associated with this process, see van Gennep (1960). For a fuller discussion of life-course transitions and some evidence on specific transitions in the United States, see, for example, Riley and Waring (1976, pp. 379–96), Riley et al. (1969), and Uhlenberg (1969).

ceremony. Individuals typically make these transitions as members of a named group. That is, there is a collective shifting of people from one age grade to another.[9]

An important aspect of transitions everywhere is that they constitute a form of social mobility. Because members of the various age strata have differential access to valued roles, transitions represent a movement within a social hierachy. In effect, as individuals proceed from one age stratum to the next, they are upwardly or downwardly mobile. Thus in all societies life-course transitions involve individual adjustments not only to new roles, and everything these may entail, but also to new positions in the social hierarchy. Reactions to transitions in age-set societies are clearly influenced by the fact that transitions there involve changes in social rewards. In most of the age-set societies examined here, prestige and power generally increase with the passage through to the most senior age grade. Thus most people in age-set societies look forward with eagerness to transition to the next age grade, at least up to the final age grade. In 10 of the 21 age-set societies under study, some loss of prerogatives accompanies transition to the final age grade, something analogous to retirement in our society. In some, political power may be lost, yet economic well-being and religious authority are increased. In five of the societies (Arusha, Karimojong, Konso, Latuka, and Nyakyusa), loss in status on passage to the final age grade is more marked.

The second dynamic feature of age systems, cohort succession, impinges on the whole age system and can have wide ramifications in the society (Riley et al. 1972; Ryder 1965; Waring 1975). It concerns us here because it influences the aging process and life-course transitions. Because the world around us is constantly changing, each cohort (persons born in the same time interval or entering a social system at the same time and aging together) is unique. And because each cohort is exposed to a unique segment of history, the process of aging and therefore the nature of life-course transitions are never quite the same for any two cohorts. The consequences of these different historical and environmental contexts for the careers of successive cohorts are readily perceived in modern, everchanging societies. For example, not only the nature of the transition itself, but the whole life course of cohorts who made the transition to adulthood during the depression were different from these experiences of cohorts who came of age during affluent times. Age-set societies may seem, in contrast, to be relatively unchanging.[10] But even there, suc-

[9] It is true that entry into the younger sets may occur on an individual basis in some societal groups, but even in these groups subsequent transitions are usually made formally and collectively by all members of that age set.

[10] It should be noted, though, that a number of African age-set societies (e.g., Tiriki, So, Sidamo) are known to have adopted the age-set structure within the past century or two. The notion of "simple" societies as unchanging is itself suspect.

cessive age sets (cohorts) differ in life-course patterns. Cohorts can vary in size, in exposure to epidemics, in experience with natural disasters, or in contact with other societal groups, and, as we shall see, such differences can affect when and how transitions are made.

Variations among Age-Set Societies

We have suggested that in some general sense age systems in age-set societies have elements in common with age systems everywhere, but that they are distinctive in the degree to which they elevate age as a major basis of social organization. However, the way they use age as an organizing principle varies among and within societies. Societies differ in the number of age grades that are recognized, the number of years an age grade spans, the degree of internal organization of an age set, the point in the life course at which individuals become members of an age set, the frequency of the formation of new age sets, and whether or not members can be initiated throughout the period of formation (see table 1, which summarizes some of these variations). Even within societies there are variations among age grades in the span of years covered and among age sets in the degree of joint and organized activity. And finally, successive age sets (cohorts) confront different physical and social environments. Underlying the diversity, however, are some common problems that help us to understand the overall nature of the transition process in these societies and begin to suggest analogies to the situation in the United States today.

PROBLEMS OF LIFE-COURSE TRANSITIONS IN AGE-SET SOCIETIES

Despite the fact that life-course transitions in age-set societies are organized and ritualized, these transitions are neither simple nor smooth. We have found three types of problems which seem to be intrinsic to transitions in these societies. Our discussion focuses on the nature of these problems and on some of the ways the societies deal with them.

The Timing of Transitions

While transitions seem structured and formal in age-set societies, not all aspects of the transition process are firmly fixed. One set of transition problems in age-set societies arises from the fact that the timing of transitions is often uncertain, and this can have repercussions throughout the society.

According to an abstract model of the process, transitions in age-set societies occur at fixed points—say, every X years. In few societies, how-

ever, is this actually the case. Typically the timing of transitions is a product of deliberation rather than of simple chronological determination. The decision to initiate a group of young men into an age set is never made by the prospective initiates themselves. Similarly, the transition of the age set from the first grade in the system to the second grade is not decreed by the individuals making the transition. While the precise group formally controlling the decision to hold transition ceremonies differs from society to society, in 19 of the 21 societies this is the prerogative of the elders, however defined; in two societies (Borana Galla, Latuka) it is the prerogative of the younger age set, which occupies the position of greatest political power in the society.

Conflicts over timing of transitions.—The consequences of the prac-

TABLE 1

VARIATIONS IN AGE-SET SYSTEMS

	Age Set Only	Generation Set
Frequency of age-set formation (yr):		
1	Kikuyu, Meru	...
3	Afikpo Ibo	...
5	Latuka, Masai, Nyakyusa (5–8 yr)	...
6	Arusha, Turkana	...
8	...	Borana Galla
10	Nuer	...
14	Rendille, Samburu (12–14 yr)	...
15	Kipsigis, Nandi, Tiriki	...
18	...	Konso
20–30	...	Jie
25–30	...	Karimojong
25	...	Kuria, So, Zanaki
Number of age grades:*		
2	Nuer, Turkana	Karimojong
3	Kipsigis, Nandi, Nyakyusa, Rendille, Samburu	Jie, So
4	Latuka	Kuria, Konso, Zanaki
5	Arusha, Afikpo Ibo, Masai, Meru, Tiriki	...
7	Kikuyu	...
11	...	Borana Galla
Point in life cycle at which individual becomes member of an age set:		
From birth	Nandi	Borana Galla, Konso, Kuria, Zanaki
From birth, but official induction later	...	Jie, So
10–11 yr	Nyakyusa	...
Adolescence	Kikuyu, Meru, Latuka, Masai, Arusha, Turkana, Nuer, Rendille, Samburu, Kipsigis, Tiriki	Karimojong
30–35 yr	Afikpo Ibo†	...

* A statement of the number of age grades which can be distinguished is beset with difficulties, for the exact number of age grades stated in some cases depends on judgment in distinguishing age grades from sub-age grades and on collapsing analytically two or more separate emically recognized age grades into one age grade.

† But informal age grouping around wrestling competition begins earlier.

tices described above are not difficult to predict. Given the fact (1) that the exact timing of transitions is often uncertain, (2) that those most concerned often have least to do with determining the time of transition, and (3) that there are inequalities among age grades in power and prestige, "setting the date" frequently leads to tensions and conflicts. Age sets which stand to lose prestige and power through the transition often do what they can to delay the ceremonies; those who stand to gain exert pressure to expedite the rites. Since the ensuing struggles concern all the men in several age sets, these conflicts have a potentially great impact on the whole society. Such conflicts are apparently widespread throughout age-set societies. In our sample there are just 10 societies for which sufficient data on the relevant variable are available. Of these, all societies having indeterminate timing (these include seven age-set societies and two generation-set societies) are reported to have conflicts over the timing of transitions.[11] The only society having determinate timing of transitions, the Nandi, is the only one for which it is specifically claimed that transitions engender no social conflict (Huntingford 1953, p. 68) (see table 2).

To be sure, the battle lines as well as the intensity and openness of the conflict vary from society to society. Among the Kipsigis, for example,

TABLE 2

INDETERMINACY OF TIMING OF TRANSITION AND CONFLICT OVER TIMING IN AFRICAN AGE-SET SOCIETIES

	Timing of Transition		Conflict	
	Determinate	Indeterminate	Yes	No
Age-set societies:				
Afikpo Ibo		x	x	
Kipsigis		x	x	
Arusha		x	x	
Masai		x	x	
Meru		x	x	
Nandi	x			x
Samburu		x	x	
Nyakyusa		x	x	
Generation-set societies:				
Zanaki		x	x	
Karimojong		x	x	

[11] There has had to be some oversimplification in categorizing a society as having indeterminate timing of transitions. The degree of indeterminacy may range from months to years. Further, three of the societies having indeterminate timing and the one having determinate timing have prerite transitions. Some of the others may also have prerite transitions, but they are not reported. These kinds of problems are involved in all attempts at classification and tabulation found in this paper, and are compounded by scanty ethnographic data in many cases.

there is struggle over the timing of initiation, with the current warriors beating up those who would replace them and the aspiring warriors, on their part, mobilizing to the point where they attack the current warriors (Peristiany 1939, pp. 31–32). Among the Samburu the expression of conflict is somewhat different. Here young men during their 20s were traditionally held in an extended period of delayed adolescence and were denied the highly valued marriage role. One response was a series of youthful deviant activities such as theft of stock, disobedience, and affrays (Spencer 1965, pp. 156–65).

Resolution of conflicts over timing of transitions.—It is the nature of the aging process that transitions must eventually be arranged. There are various mechanisms by which the different societies resolve the issues pertaining to the timing of these rites. In some societies conflicts are institutionalized in transition rituals. For example, the Meru stage a mock battle between age sets in which novitiate warriors and novitiate elders are aligned against the incumbent warriors and elders. These latter are defeated and concede to their conquerors, who then come into power (Holding 1942, p. 59). Sometimes, those not directly affected by transitions intervene. Among the Kipsigis, where conflicts were quite intense, the elders who had already retired and who wanted their sons to become social adults with the prerogatives of manhood tried to persuade the warriors to retire (Peristiany 1939). Here, and in the case of the Meru, there is a suggestion that a tacit coalition among the less privileged against those in privileged grades eventually forces resolution of the conflict.

In other societies demographic factors become crucial. Among the Karimojong the decision to hold the transition is in the hands of members of the senior set, who occupy the most prestigious position and who are not eager to relinquish their roles. But as many of them die or become senile, it becomes impossible for them to carry out their social duties. At the same time, the shrinking size of the senior set and the deteriorating faculties of senior-set members embolden the adult uninitiated to make public protests. It is at this point that "succession takes place for it is seen as unopposable by the retiring elders themselves" (Dyson-Hudson 1966, p. 188).

Age Discrepancies—Chronological Age and Age-Set Membership

Another problem associated with the timing of transitions arises because members making transitions together are often at different stages in their respective life courses (or, alternatively, because individuals are at the same stage in their life course but may not make transitions together). Such age discrepancies are mentioned in reports on several of the age-set

and all seven of the generation-set societies under study. These discrepancies, as the figures suggest, are most dramatic in those societies in which age-set membership is based on generation rather than simply on age. For example, among the Galla of Ethiopia (Legesse 1973), many children may "retire" alongside elderly men who also belong to the same generation class.[12] Some men may not marry and have children until they are in their 40s, while others in their generation set will encounter the same role transfer in their 20s. And among the Karimojong, where only two generation sets—one junior and one senior—are recognized as being in existence at any one time, while a boy's father belongs to the junior generation set, the boy himself may not be initiated (Dyson-Hudson 1966, p. 163). Thus young boys may join an age set and enjoy the concomitant privileges while older boys may be excluded.

In age-set societies having no generational component such age discrepancies may occur for different reasons. It is often the case that initiation into an age set depends not so much on chronological age—traditionally, few age-set societies calculated the age of individuals in years—but on such factors as social maturity or family wealth (where initiation requires a substantial offering) (Evans-Pritchard 1936, p. 237). However, once the recruitment of the age set's membership is completed, subsequent role transfers will be made by all members of the age set together at the same point in time. For example, where individual A was one of the oldest recruits to the age set and individual B was one of the youngest, they would both "retire" at the same time, but individual A might be 48 and individual B just 38 at retirement.

The workings of the age system can in these various ways bring hardship to some members of an age set and create problems for the society. The rules of the age system may disregard the needs of certain individuals; they may violate other societal norms for what is age-appropriate behavior; or they may be at odds with societal requirements for qualified personnel such as warriors of fighting age. (See Prins [1953/70] for a discussion of adverse consequences for society of such rules among the Galla, and Nadel [1952] for a discussion of the relationship between age discrepancies and witchcraft.) In some societies, at least, such contradictions do not go unrecognized. For example, among the Zanaki the discrepancies are alleviated somewhat by modes of transition distinct from the generation system. Males of approximately the same age undergo circumcision as a rite of passage into adulthood and form a group which is independent of generation. The generation-set system still operates,

[12] Because a boy's generation-set membership is specified by a fixed number of sets following that of the father, over time such sets will be composed of individuals of widely different ages, with many members of more senior sets being much younger than members of more junior sets.

however, in that those boys whose fathers belong to the generation class which is still active cannot assume all the roles of adulthood. They are regarded as adults, but are excluded from some specific activities of the generation class.[13]

Role Discontinuities

The problems discussed so far pertain to matters of timing—when whole age sets are to make transitions or when individuals are permitted to or required to make these transitions. Role discontinuities involve the content of roles, the marked contrast between pre- and posttransition role definitions as well as problems of pacing, that is, the abruptness with which these transfers are made.

Nature and extent of role discontinuities in age-set societies.—There is evidence of sharp changes in the individual's roles at the time of transition in many age-set systems. Perhaps the most dramatic transition commonly found is that occurring when boys are initiated into an age set and occupy the first grade of an age-set cycle. (In the 21 societies under study such a dramatic change was reported in nine of the societies.)[14] Here individuals move abruptly from the roles of children to those of adults. The Tiriki are a case in point (Sangree 1966). Uninitiated boys may not engage in sexual intercourse, they must eat with other children and with women, and they are free to play in the women's section of the hut. After initiation, they may engage in sexual intercourse, are expected to eat with other men, and are forbidden from entering the women's section of the huts. They are thus abruptly cut off from the world of women and children and catapulted into the company of adult men. In addition, they undergo a dramatic change in prestige. This suddenness of role transfer and the sharp contrast between pre- and posttransition roles, however, are not duplicated in subsequent transitions among the Tiriki. Similarly, in other societies not all transitions are characterized by sharp and sudden changes. For example, sudden and dramatic role discontinuity at the onset of old age is reported only for two societies (Latuka and Nyakyusa). In a number of societies, "elder" status, the final age grade of the life course, is reached well before old age and thus there is no formal institutionalization in the age-grade system of roles associated with old age.

Moderating mechanisms.—Role discontinuities have long been recog-

[13] Comparable mechanisms for alleviating the stresses inherent in age discrepancies are described for the Karimojong (Dyson-Hudson 1966, p. 202) and the Kuria (Ruel 1958, p. 12).

[14] These nine are the Kipsigis, Latuka, Masai, Nuer, Nyakyusa, Samburu, So, Tiriki, and Turkana. This does not imply that all the other societies lacked such dramatic change, for in nine cases the evidence was not complete on this point.

nized as a source of strain to the individual making the transition. If there are tensions involved in role transfers, a variety of procedures have evolved in many age-set societies which serve to offset or alleviate the difficulties which would otherwise accompany abrupt changes in roles. First, in some societies there is a gradual period of transition which mitigates the abruptness of role changes and allows a period of anticipatory socialization. For example, among the Afikpo Ibo, members of the junior elder age grade are permitted to attend and to take part in the elders' meetings (Ottenberg 1971, p. 86). (It should be emphasized that taking part does not make them "full partners" with the elders.) And among the Karimojong, the senior generation about to retire abandons its prerogatives stage by stage "until at the end it has abdicated its superior status, relinquished its corporate control of public affairs and reduced itself to a collection of separate, aged individuals" (Dyson-Hudson 1966, p. 193).

Further, in many societies, a degree of individual flexibility is allowed in the actual operation of the age-grade system. For example, among the Samburu (Spencer 1965, p. 90), "those who wish to become elders as soon as possible generally try to marry early and start to acquire the dignity of elders; and those who prefer still to remain as moran retain the accoutrements and behavior of moranhood." Such flexibility not only tempers the impact of role discontinuities on the individual but helps to minimize the discrepancies between the formal rules of the age-grade system on the one hand and individual inclinations on the other.

Thus flexibility and informal rules in the age system do seem to blunt some of the potential difficulties individuals face in making transitions in age-set societies and to lessen some of the individual deprivations engendered by the rules of transitions. This does not mean that the disruptive potential involved in conflicts over the timing of transitions is thereby extinguished. There are societies which are reported to have both conflicts over the timing of transitions and a degree of flexibility in the operation of their age systems. What seems to be at stake in these conflict situations is the assignment of full rights and privileges to age-set members and the public recognition of these rights.

Many of these problematic aspects of transitions in age-set societies seem far removed from the issues of transitions in large-scale, modern societies. Yet, as we discuss in the following pages, there are some underlying parallels in the two kinds of societies.

LIFE-COURSE TRANSITIONS: DILEMMAS, ADAPTATIONS, AND CHANGE

Life-course transitions in age-set societies seem complex and paradoxical. While they generate social and individual stress, they are frequently

accompanied by mechanisms which reduce some of these stresses. These elements take on fresh clarity when we juxtapose transition processes in small-scale, age-set societies to those in an urban, industrial society like the United States. A comparison between these two diverse types of societies begins to suggest some generalizations about the impact of life-course transitions on the society and the individual.

Life-Course Transitions and the Society

On the surface, transitions in age-set societies provide an orderly method of role allocation and reallocation. But, as we have seen, the process is often not so orderly; rather, it is frequently the occasion of societal discord. These conflicts are essentially a version of time-honored struggles between the "ins" and the "outs." They tend to arise because there are no clear societal rules mandating a specific point at which transitions are to be made. The conflict of interest is between those who wish to assume age-graded roles of power and prestige and those who are reluctant to be "kicked upstairs."

While the absence of clear rules of transition can be a source of age-related antagonisms in age-set societies, the operation of formal rules distinctly establishing points of life-course transition can also create problems for the society. We have noted that in these societies strict adherence to formal rules can create age discrepancies which bring hardship to only some individuals of a given age—for example, delaying their marriage or forcing premature "retirement." Because these rules do not affect all individuals of a similar chronological age, the potential for group protest and society-wide discord over the operation of these rules is reduced. Nevertheless, conformity to such rules can have a society-wide impact by subverting societal requirements for a sufficient number of role players of a given chronological age.

Thus in age-set societies, both clearly defined rules on the one hand and indeterminate rules of transition on the other have society-wide repercussions. We suggest that both explicit and indeterminate rules of transition also have far-reaching consequences in American society.

Consider first the indeterminacy of the timing of transitions. In our society, the assumption of full adult status depends a good deal on having full-time work roles, especially for males and increasingly for females. Yet unemployment rates are higher for the young than for older workers, especially at times of economic downturns. When full-time work is not available; the transition of young people to effective adulthood becomes problematic. The uncertainty about when they will assume full adult

status can be a source of youthful deviance and hostility toward their elders.[15]

Alternatively, the operation of explicit rules of transition can also lead to problems in American society because such rules are not necessarily pegged to the individual's readiness either to assume or to leave given roles. Further, because these clearly defined rules do cover a considerable number of individuals in an age stratum, the potential difficulties for the society are magnified. Two examples will illustrate the point. For young people seeking to enter the work force as full-time workers, formal job requirements typically stipulate that the young person have a high school diploma and, in many cases, a college degree. These binding age-related norms about the appropriate number of years of schooling needed to qualify for work roles result in an extended period of adolescence or pre-adulthood for a substantial number of young people. One reaction of many young people to prolonged dependency has been individual and collective action—such as struggles for student power or against parietal rules—in order to claim some of the rights and privileges of adulthood even before adult roles are assumed (see Coleman [1974] on youth-culture pressures for autonomy; see also Matza [1964]). For older people, formal rules mandating retirement frequently operate to hasten their exit from the work force. While older people have not often demonstrated their dissatisfactions with "rolelessness" by collective protests, the enforced role ousters have led to various types of social withdrawal among them, an outcome creating problems for the family and community (Riley et al. 1969).

These examples from two widely different types of societies suggest that there are immanent sources of conflict and tension in the very process of life-course transitions. Because aging is inevitable and continuous, the society must accommodate the perpetual flow of one cohort (or age set) after another to fit into the array of social roles that is available. One possible solution is to allow "natural" forces to operate: younger people assume adult roles when they are ready to do so; older people give up roles when they are ready or when they become ill or die. Another solution is to regulate this continuous flow of people by establishing firm age norms for entering roles and leaving them: that is, by establishing clearly defined points of life-course transition. Both solutions are problematic and can have adverse consequences for the society: the first, because there is no guarantee that the readiness of those who must give up roles fits in with the readiness of those who are waiting in the wings;

[15] The indeterminacy of transition to adulthood in 19th-century America is found to have a similar potential for creating discord in the family (see Foner [1978]).

the second, because the age norms may not be in accord with individual needs or societal requirements.[16]

This statement is an abstraction of what really goes on in various types of societies, of course, but it provides a basis for generalizations about some sources of age conflicts and stresses in age systems. In essence, the problems summarized above parallel several forms of the pathological division of labor which Durkheim (1964) proposed as causes of class conflicts. On the one hand, we find insufficient (in the Durkheimian sense) regulation (e.g., no clear norms for the timing of transitions)—the analogue of the anomic situation. On the other hand, there is a set of rules, but they are not in accord with individual capacities, as in the extrusion of the young and healthy old from the work force in our society, or the long delay in marriage in some age-set societies—a situation corresponding to Durkheim's forced division of labor. But whereas Durkheim saw these pathological forms as temporary aberrations, the examples from vastly different societies suggest to us that, in the case of age stratification, age conflicts and tensions in the society are inherent in the process of life-course transitions. (See Mannheim [1952] for seminal work on another key source of age conflicts, the succession of generations [cohorts].)

Flexibility in Transition Processes

If matters of timing of transitions produce individual frustration and precipitate societal discord, the actual transition subjects the individual to conflicts between the demands of old and new roles—that is, to sharp role discontinuities. Many decades ago, Benedict (1938, pp. 165–66) asserted that in age-graded societies the trauma of role discontinuities is alleviated by peer support as individuals make these transitions as part of a group. We have found that this is only part of the story: as we have discussed previously, there are a variety of practices in these societies which appear to minimize the potential stresses for the individual in life-course transitions. There is frequently a period of role rehearsal which permits individuals to learn new roles before they are official incumbents. Sometimes the role changes are made by stages, thereby easing the abruptness of transfers. Sometimes the actual practices permit individuals to make some role transfers, such as marriage, on an individual basis and others with their age-set peers. Such practices may mitigate the strains of making multiple transfers simultaneously. In general, there is a good deal of individual flexibility in following the rules, a tendency which serves

[16] In addition, in changing times age norms may not mesh with changes in supply of and demand for role players of given ages, a point discussed below (see also Waring [1975]).

not only to alleviate role discontinuities but also to mitigate age discrepancies.

Flexibility in actual practices does not mean that collective transition rituals are of little significance. In some cases the rituals confer the full range of privileges on age-set members, only some of which had been granted informally. In addition, they serve as points around which informal norms can vary (Gulliver 1963). Finally, in some societies the formal transition may be especially significant in compelling individuals to leave highly valued roles associated with a particular age grade.[17] In short, rituals serve to legitimize age-grade status.

Can parallels be drawn between these aspects of transitions in age-set societies and those in American society? In American society there are no collective rituals marking the passage from one age grade to another. (High school and college graduation ceremonies may constitute an exception, though not for all individuals in the cohort.) Moreover, in our society the problems of role transitions for the individual seem more acute than in age-set societies. Discontinuities between pre- and posttransition roles involve not only sharp changes in role definitions but changes in physical settings, in role partners, and role relationships. Yet, just as we have noted a relatively wide range of practices in age-set societies that serve to ease these role passages for the individual, we can identify some of these very same strategies operating in the contemporary United States.

Consider the transition from youth to adulthood. Role rehearsal for major adult roles is widespread. For example, part-time and/or summer jobs are common for school youths 16–19 (Waldman 1969). In 1971 about 25% of 14–16-year-old males in school (97% were in school) were working, mostly part time (Griliches 1974). And studies indicate that child work—defined as working part time for strangers for pay—was more prevalent among fourth- to eighth-grade boys than is generally believed (Engel, as reported in Skolnick 1973, pp. 352–54). And role rehearsal for marriage includes dating, going steady, and, currently, living together. (More than a quarter of a million young men and women under 25 were reported to be sharing bachelor quarters with an unrelated partner of the opposite sex in 1976 [U.S. Bureau of the Census 1977, p. 43]). At least for some people, such role rehearsal represents a self-conscious effort to adjust to prospective roles.

[17] An example of this is found among the Latuka, who have a large-scale transition rite every 22 years. Individuals pass from the youth age grade to the highly valued "village owners" age grade throughout the interrite period, their status being officially validated at the next major transition ceremony. However, exit from this grade to the less powerful grade of elders takes place only through the formal rite. Thus, over time, the population of "village owners" grows as new members are inducted, only to sharply fall when the new rite takes place, forcing the retirement of all "village owners" who had been formally inducted into that age grade at the previous rite.

Measures to ease the shock of retirement seem less prevalent. Gradual retirement is possible for only certain segments of the labor force, primarily those in the independent professions and those having their own farms or businesses. Reports indicate that a substantial minority of retired workers do work, many at part-time jobs, most in the same broad occupational categories as their previous jobs (Riley and Foner 1968, p. 452; Lingg 1975). Most adults do little definite planning for this late stage in their lives (Riley and Foner 1968, p. 446), although there may be a degree of unwitting preparation (Merton 1957; Foner 1969).

Finally, there is some flexibility in the operation of the age system that permits individuals to make transitions at their own pace and in terms of their own needs. Children skip grades in elementary school; bright youths enter college after only three years of high school; there is flexible retirement in some companies and occupations. And college students now do not have to wait until they finish their schooling before they can get married.

Thus in the United States there is evidence that age norms are bent somewhat to make transitions a less difficult process (see Clausen [1972, pp. 504–6] for a discussion of the lessening of some role discontinuities in the United States today). However, this does not resolve all problems associated with role transfers. Young people may be socialized for but not assigned to valued roles (see Riley et al. [1972, p. 571] for a discussion of socialization without role assignment), and on their part older people may be preparing for retirement even though they are reluctant to relinquish their current, more rewarding roles.

In summary, in both small-scale, age-set societies and in large-scale, industrial societies like the United States, individuals are not necessarily at the mercy of the inexorable workings of the age system. In both types of societies there are intrinsic sources of strain in transitions for the individual; but the actual operation of the age system departs from the formal system in many ways, often making transitions less onerous for the individual. Further, flexibility in the system also serves to offset some problems that transitions pose for the society. That is, insofar as flexible transition practices reduce individual frustration which might otherwise be expressed in deviant acts or directed against other age groups in the society, these practices serve to reduce strain in the society.

We would argue, however, that such "give" in the age system can have different consequences for the individual and the society. Consider two sets of major transition issues discussed above: for the individual, to resolve conflicts between the demands of the old and of the new roles; for the society, to resolve the competing demands of different age groups for rewarding roles. The mechanisms we have mentioned can help the person

adjust to and fit into new roles. By alleviating certain individual problems, flexible transition practices can also help deflect tensions in the society. However, with regard to the problems on the societal level noted above, one dilemma remains. These mechanisms are no guarantee that individuals will be assigned to or permitted to keep highly rewarded roles. As long as there are a limited number of these rewarding roles to be allocated among people of different ages, the gains of one age group must be at the expense of other age groups.[18] It is competition among members of different age strata for rewarding roles that leads to recurrent problems for the society. Thus, flexibility in the system may be important in supporting the individual without resolving some fundamental transition problems for the society.

In a broader sense, whether individual or societal problems engendered by transitions are fully resolved or not, such problems do have far-reaching implications for the society. Both the problems and the measures devised to cope with them can be a source of change in the nature of transitions, a point to which we now turn.

The Potential for Change in Transition Processes

Not only do transitions mark individual transformation, but transition processes themselves are subject to change. These changes may be almost imperceptible or dramatic; they may involve only some aspects of transition or many aspects. Among the many features that may be altered are: matters of timing, formality of the transition, the extent of role discontinuities, whether transitions are made on a group or individual basis, and whether they involve more than one role. Our focus here is on two sources of change in transitions which are key aspects of age-stratification systems: the hierarchical nature of age-stratification systems and the succession of cohorts.

Consider first the implications of the age-related hierarchy of roles. We have suggested that life-course transitions represent a form of social mobility as individuals proceed from one set of roles to another, with the new roles providing either greater or fewer rewards than the old ones. It is the desire to gain access to or hold on to these rewards that provides an important motivation for social conflict or individual grievances. (Incidentally, this also helps to account for the fact that there are two transition points which seem most often to be involved: the move to adulthood and the transition to old age. These are frequently the points which entail appreciable changes in the individual's power and prestige.) One outcome of real or threatened conflicts may be a change in the transition process.

[18] It is recognized that where there is a shortage of role players to fill rewarding roles, as in war time, flexible age norms can operate to ease societal difficulties.

A recent example in the United States is the lowering of the voting age, which, in part at least, can be traced to the youth-adult conflict of the period in which the change was enacted.

Perhaps a more significant source of change in transition processes is related to the succession of cohorts. No matter how unchanging a society may seem to be, no two cohorts are identical nor do they confront exactly the same social and physical environment. Such changes in the context in which transitions occur are likely to force changes in the processes themselves. Recall the case of the Karimojong where the relative sizes of the age set seeking power and the age set (cohort) in power were crucial in setting the time of transition. It seems clear that any exogenous events such as epidemics or natural disasters that affect the size of these successive age sets would influence the timing of the transition and the relations among age sets. Size of successive cohorts has been an important influence on transition points in our society as well. Waring (1975) shows how transition points are pushed up or pushed back to adjust to successive cohorts of different size. For example, the transition of unusually large youth cohorts to adulthood may be delayed because there are not enough jobs for them. Or, the transition of a small middle-aged cohort to retirement may also be delayed in order to maintain the levels of experienced workers in the labor force.

The unique experiences of each cohort may also engender specific adaptive mechanisms that serve to deal with the problems of transitions. This may be a gradual process as successive cohorts learn from the experience of their predecessors. For example, we suggested above that in the transition to retirement in the United States, relatively few adaptations had been institutionalized to deal with the abruptness of this role transition. But retirement as a firmly established norm is a relatively recent phenomenon. In 1920 there were 60% and in 1950 there were 46% of the men 65 and older in the labor force as compared to less than 25% in 1974 (Riley and Foner 1968, p. 42; Bixby 1976, p. 4). Much of the data about adjustments to retirement come from the first cohorts to confront retirement as a mass phenomenon (Riley and Foner 1968). Among more recent cohorts, we can expect more advanced planning and social support to ease the discontinuities involved in the transition. The recent increase in early retirement seems to herald new attitudes about retirement among people in their 50s and early 60s (Bixby 1976, p. 3). Clearly, the mass nature of retirement today provides the basis for peer support to help retirees "manage" this change in their lives. It is this mass market which has facilitated the development of leisure programs and residential havens for the retired. And these, in turn, provide a supportive environment to the retirees. Thus, as experience with retirement and its problems grows, and

measures are developed to meet these problems, the nature of the transition process may be expected to change.[19]

Similar processes are likely to have occurred in age-set societies. Consider some of the events that have "hit" these societies: the borrowing of the age-set system from another group, the banning of warfare by colonial powers, or the contact with more complex political and economic systems. Two examples suggest how such events have affected transition processes in African age-set societies. Among the Nigerian Afikpo Ibo, a new age set was traditionally initiated every three years. It is reported that in a number of villages now, it takes a longer period of time to form a new age set for "many young men are away at work or at school, and it is difficult to organize into an effective group and to collect the necessary funds for an initiation, even under pressure from the elders" (Ottenberg 1971, p. 56). Among the Sidamo, where once the elders had a monopoly on political power, they recently have had to compete with the Ethiopian court system. Youths are thus less deferential toward the elders and also, presumably, less concerned about making the transition to the elder grade (Hamer 1970, p. 68). Here, apparently, the nature of the transition is changing as an age grade once associated with great rewards of power and wealth begins to lose these privileges. It is likely that the adjustments made by early cohorts to confront such social and economic changes will differ from those made by later cohorts. If we are correct, then as these adjustments accumulate, transitions and the age-set system are subject to further changes.

Even if the societal context in which transitions take place does not change perceptibly, successive cohorts may still have unique transition experiences. For the difficulties engendered by transition processes seem to generate the impulse for change. As members of a given cohort find ways to deal with transition problems, the transitions themselves become modified. Later cohorts making these transitions confront a situation on which earlier cohorts have left their mark.

Thus while it is tempting to emphasize the constancy of the ever-recurring cycles of transitions and the continuous flow of cohorts, we see these processes themselves as ever changing. Within the life-course transition processes are the seeds of change as transitions give rise to social conflict and individual adaptations. And the process of cohort flow, as it interweaves with social and environmental change, acts on the age system and transition processes in the society.

[19] Even as some "old" problems associated with retirement are being resolved, new issues (e.g., the elimination of mandatory retirement and the introduction of flexible age criteria for retirement) are facing those on the threshold of retirement.

CONCLUSION

Our analysis of transition processes in two vastly different types of society has challenged some received notions about transitions in these societies:

Whereas many theories of life-course transitions have emphasized the impact of transitions on the individual, we have pointed to their effect on the society as well. In both age-set societies and the United States today there is a disruptive potential in life-course transitions. Whereas there has been a tendency to stress smooth transitions for the individual in preliterate societies and traumatic ones for the individual in Western societies, we suggest that the difficulties encountered in life-course passages can be formidable wherever they occur. At the same time, in both types of societies means are found to mitigate the tyranny of the rules of the system. Finally, transition processes are constantly subject to change as individuals and societies deal with the exigencies of the processes themselves and of social and environmental change.

These findings highlight two important features of age-stratification systems. First, just as Marx looked for inherent sources of conflict in the class system and saw class conflicts as a basis of change in the class structure, we propose that age conflicts and tensions are intrinsic to transition processes and that these conflicts are likely to be a source of change in the age systems (see Foner [1975] for a discussion of this general point). Second, each cohort facing a given transition does so in a unique historical context and deals with the problems of this transition in its own way. Thus generalizations about transitions depend both on cross-cultural study and on the study of these passages among successive cohorts within each society.

We recognize that it is not the same the world over, but we suggest that there may well be underlying similarities in transition processes wherever they occur, just as there are such parallels in the two contrasting types of society we examined.

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"Anticritical Last Word on *The Spirit of Capitalism*," by Max Weber[1]

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The "Anticritical Last Word on *The Spirit of Capitalism*" was written by Weber in 1910 as a reply to his most prominent and trenchant contemporary critic, the historian Felix Rachfahl. The issues raised by Rachfahl constitute the basis of many later well-known critiques of *The Protestant Ethic*. Thus this essay offers an outline of how Weber would have replied to his later critics; in addition, it is a significant reiteration and clarification of the arguments found in *The Protestant Ethic*. The introduction summarizes Rachfahl's objections and points to the reasons why his critique and later ones misconstrue the nature of Weber's arguments.

INTRODUCTION

Since its publication in 1904–5, *The Protestant Ethic and the Spirit of Capitalism* has been the object of both reverential praise and harsh criticism. The debate surrounding this compressed and exploratory essay comprises countless articles and numerous books and spans over half a century. Yet despite the imposing extent of this debate (see Eisenstadt 1968; Nelson 1973; Green 1973) the controversy between Weber and his contemporary critics has been largely neglected, except for some passing comments found in literature reviews. Ephraim Fischoff ([1944] 1968, p. 68) has noted Weber's exchange with his critics and the regrettable fact that Weber's rebuttals have remained unavailable. Kurt Samuelsson, in his stinging critique, *Religion and Economic Action* (1964), offers a brief summary of Weber's most trenchant contemporary critic, Felix Rachfahl. However, Samuelsson entirely omits mention of Weber's replies to Rachfahl, and this surprising disregard leads him to overinterpret and in some cases simply to misstate Weber's arguments.

The significance of this early dispute rests wholly on the fact that Weber himself participated in it, capping the debate with his final reply to Rachfahl, "Anticritical Last Word on *The Spirit of Capitalism*." The

[1] I am deeply indebted to the late Professor Benjamin Nelson and to Professor Guenther Roth, both of whom examined this translation in detail and offered extensive critiques; their assistance greatly improved the translation. I would also like to thank Professors Lewis Coser, Kurt Lang, and Ned Polsky for their careful reading of the text and helpful suggestions.

general inattention to this early controversy is particularly unfortunate since Rachfahl's arguments against Weber formed the basis of many of the later well-known critiques of *The Protestant Ethic*. H. M. Robertson (1935), Lujo Brentano (1923), Herbert Luethy ([1964] 1968), R. H. Tawney ([1926] 1962), and most recently H. Trevor-Roper (1967) are a few of the more prominent commentators on *The Protestant Ethic* whose criticisms are closely related to the issues raised by Rachfahl. Thus the "Anticritical Last Word" offers at least an outline of what would have been Weber's reply to his later critics.

The exchange between Rachfahl and Weber consisted of four essays appearing between 1909 and 1910 (Rachfahl [1909] 1972*a*, [1910] 1972*b*; Weber [1910] 1972*a*, [1910] 1972*b*). At this time Rachfahl, a professor of history at the University of Kiel, was best known for a highly respected, three-volume history on William of Orange. Later, in 1915, he accepted a chair at the prestigious university in Freiburg, where he published his last book, *Staat, Kultur und Gesellschaft*, shortly before his death in 1925. Rachfahl's essays on Weber first appeared in the *Internationale Wochenschrift für Wissenschaft, Kunst und Technik*, a popular intellectual periodical of the time. They were directed largely at Weber but also included criticism of Weber's colleague at Heidelberg, the theologian Ernest Troeltsch. Weber would have preferred to answer Rachfahl in the same periodical because of its wide readership; however, because Troeltsch was requested to submit a reply to an essay that was in great measure directed at Weber and because Weber found the editorial policies of the *Internationale Wochenschrift* objectionable, he chose to answer Rachfahl in the more academic *Archiv für Sozialwissenschaft und Sozialpolitik*, of which he was one of the editors.

The exchange with Rachfahl quickly dissolved into a bookish and pedantic polemic. Nonetheless, Rachfahl offered a serious critique of Weber that centered on five issues:

1. Rachfahl argued that Weber's notion of the "spirit of capitalism" was misconceived since it rests upon a tenuous distinction between "traditional" and "capitalistic" economic systems. He contended that Weber drew this distinction in terms of absolute opposites when in fact it consists merely of relative differences. This was the result, Rachfahl thought, of Weber's use of ideal types in his analysis instead of examining actual historical events. Rachfahl pointed out that there have always been men satisfied with a style of work and life dictated by custom as well as those who find their needs, ambitions, and actions extending beyond the boundaries defined by tradition. He argued that the appearance of the latter type of man certainly preceded Calvin and therefore did not require a dramatic historical event such as the Protestant Reformation to induce

its development. As a consequence, Rachfahl considered Weber's notion of the "spirit of capitalism" "too narrow on the one hand and too broad on the other . . ." (Rachfahl [1909] 1972*a*, p. 75). It is too broad because it implies that any attempt to overstep traditional economic activities is a manifestation of a "capitalistic spirit." It is too narrow because it includes as "capitalistic" only economic activity motivated by the desire for capital accumulation and excludes economic activity elicited by such other motives as power, honor, or security for succeeding generations. Rachfahl maintained that motivations such as these are much more significant for the analysis of economic behavior than any religiously induced ones. Instead, he asserted that, whenever individuals find religion and profitable economic activity in conflict, religion is quickly dismissed. He took this to mean not that religious considerations are of no significance but merely that they must be weighed within the context of wider social relations.

2. Rachfahl considered Weber's notion of "inner-worldly asceticism" misleading since Weber meant by this simply a methodically organized and rationally controlled life-style oriented to the greater glory of the Lord. Rachfahl insisted that precisely such a life-style was also demanded of Catholic laity and was not at all unique to the Protestant sects. Furthermore, he noted that such a notion has little to do with the Catholic conception of asceticism, the essence of which, he argued, is the rejection of the world, regardless of whether this be a rational or irrational form of renunciation. Such forms of world-renouncing asceticism constituted a particular type of piety explicitly rejected by Protestantism, which demanded instead that a general Christian ethic permeate all aspects of an individual's life. However, this demand, Rachfahl insisted, was thoroughly incorporated in Roman Catholic theology. Thus for Rachfahl the reality of "inner-worldly asceticism" lies in the neologism and not in historical fact.

3. Rachfahl maintained that the empirical relationship between ascetic Protestant sects and the rise of modern capitalism is a very weak one, even if the doubtful existence of something like "inner-worldly asceticism" should be admitted. According to him, the capitalism of Holland has almost nothing to do with such sects; in the 17th and 18th centuries the United States, where this ascetic impact allegedly exerted such a great influence, was largely an agrarian society with no great fortunes. In England, he argues, capitalist enterprise antedated the Protestant sects that, according to Weber, would have introduced such a "capitalist spirit."

4. Rachfahl asserted that the criticisms mentioned above might have been avoided had Weber viewed the great capitalists rather than the petite bourgeoisie as the carriers of a "capitalist spirit." He protested that the

great capitalists, such as Jacob Fugger, were hardly shy about ostentatious living; on the contrary, Rachfahl thought that in their lives the significance of nonreligious factors such as pleasure, power, or honor becomes most apparent. He argued that, whatever spirit may be found among the petite bourgeoisie, it is not a capitalist one.

5. Finally, Rachfahl did not flatly deny the role of the Reformation in shaping the modern industrial economic system. However, he contended that its significance lies not in the notion of a "calling," which induced an "inner-worldly asceticism," but in the general tolerance for new forms of social behavior which emerged from the Reformation. It transformed Europe both politically and economically and thus eliminated traditional impediments to new forms of economic activities.

Weber's two essays in reply to Rachfahl's critique appeared in 1910. In these replies Weber, whose explosive temperament was not unknown, fell upon Rachfahl in a particularly rough manner. However, his stance toward Rachfahl later softened, and in the notes to the revised version of *The Protestant Ethic* he expressed a general admiration of Rachfahl's work, although he noted that in this instance Rachfahl had "ventured into a field which he [had] not thoroughly mastered" (Weber [1920] 1958, p. 186).

Rachfahl's criticisms, while at points not without acuity, reveal a basic misunderstanding of Weber's argument. Weber's analysis sought to examine specifically religious and sociocultural forces which would favor the development of the modern capitalist industrial system. Its object was not to analyze the economic behavior of individual persons. As Weber points out in his "Author's Introduction," written shortly before his death in 1920 for the *Gesammelte Aufsaetze zur Religionssoziologie* (1920; see also Nelson 1974), he was attempting to identify a potential relationship between a social ethos and the rise of the "rational capitalistic organization of (formally) free labour" (Weber [1920] 1958, p. 21). The essence of Rachfahl's critique is his objection to the isolated analysis of religiously induced motivations for economic gain and the exclusion of other types of motivations. As at least one writer has noted, this forced Rachfahl to rest his objections on the analysis of the economic behavior of individuals, a quite different level of analysis from the one proposed by Weber (see Michael Sukale's chapter in Baumgarten 1964, pp. 445–48). Weber would in no way contest Rachfahl's argument that in every epoch entrepreneurs and economic innovators may be found. Indeed such an argument might have struck Weber as entirely fatuous since he was himself a noted economic historian of this period who in fact had written his doctoral dissertation in 1889 on the structure of trading companies in the Middle Ages (Weber [1889] 1924).

Neither varieties of individual economic behavior nor, for that matter, the personal characteristics of particular entrepreneurs constituted a central issue in Weber's argument. This is a point which Rachfahl and many later critics have frequently missed. Rather, the question for Weber was how a specific historical transformation in the character of occidental society, namely, the emergence of the modern industrial system from earlier social forms of the Catholic Middle Ages, was supported by particular sociohistorical events. His distinct interest was the penetration of an entire society by a new practical ethos, an ethos nurtured through placing novel religious and hence psychological premia upon the systematic and methodical organization of conduct in one's vocation. It was an ethos not limited simply to the entrepreneurs and middle strata; rather, it colored the organization of conduct in vocations of both high and low degree. Weber of course located the impetus for this ethos in the "inner-worldly asceticism" fostered by certain forms of Protestantism; however, he was well aware that this original impetus was no longer necessary to sustain contemporary capitalism. Indeed in this essay Weber himself intimates that sentiments current in his own time were recoiling against the "vocational personality" of early modern capitalism. Yet in its beginnings, Weber thought the practical ethos cultivated by "inner-worldly asceticism" provoked a new organization of enterprise as well as a methodical conduct of self which attached new meanings to social life in general and occupational activity in particular.

Even as recent and acute a commentator as Trevor-Roper has missed this point. In his essay entitled "Religion, the Reformation and Social Change" (1967), he examines the characteristics of some powerful entrepreneurs. Trevor-Roper maintains that, while Calvinist, they possessed the Erasmian detachment of an enlightened bourgeoisie, and thus their religious roots were of little significance. Trevor-Roper contends that what they shared in common was not religion but the fact of being primarily immigrants from thriving centers of capitalism who later lived in alien social milieus. The evidence that he offers for their Erasmian world view is less than persuasive; however, the entire issue, while not uninteresting, is not as significant as some imagine. It does not truly address Weber's argument. The personal characteristics of specific wealthy individuals are at most peripheral elements in the sociohistorical forces that Weber sought to examine. They shed little light on how the practical ethos of life in the early modern era became one that could support and legitimate the rational organization of capitalist labor and capitalist enterprise.

All this is, of course, more apparent with hindsight. The essay which Rachfahl criticized was prolix, made increasingly difficult by lengthy foot-

notes, and therefore easily subject to misunderstanding. *The Protestant Ethic* was intended mainly as an exploratory foray whose full meaning would not become clear until Weber's later work in the comparative sociology of religion. Only in the context of these later essays and his monumental *Wirtschaft und Gesellschaft* do the intent and arguments of this early essay become more readily understood.

Weber did not return to *The Protestant Ethic* until shortly before his death. Following the publication of his "Anticritical Last Word," he began to work on the monograph known as *The Sociology of Religion,* which appeared in *Wirtschaft und Gesellschaft,* and later on *The Economic Ethic of the World Religions*. It seems that, rather than continue a fruitless controversy, he chose to let his later work inform his critics.

As we plunge toward the 21st century, the issue of Protestantism and capitalism no longer seems to possess the same intellectual urgency as it did to a generation still experiencing the tension and anxieties of the drive toward "modernity." Nevertheless, the problem posed by Weber remains one of undiminished import. Is it still possible to speak, in our modern "postindustrial" societies, of a practical ethos, whatever its origin, which attaches such importance to occupational activity? Or has our society undergone dramatic transformations equal to those of the early modern era, transformations that evoke a different ethos and different affinities?

The following translation consists of the second half of the "Anticritical Last Word." The first half consists of a rancorous polemic against Rachfahl which adds little to the substance of Weber's argument. However, a taste of this polemic may be found in the references to Rachfahl and particularly in the last paragraph of this translation.

ANTICRITICAL LAST WORD ON *THE SPIRIT OF CAPITALISM*

After so much polemic, finally enough. I am entirely forgetting the exceeding kindliness Rachfahl has shown in giving me a lesson on how I might have done it better. Namely, I should have said, "Under the influence of the Reformation vocational ethic, a particular variation of the capitalist spirit developed in modern times. I will ascertain the source of this ethic and the boundaries of its expansion as well as examine the question of its qualitative impact. That is, I will attempt to determine if the capitalist spirit, which has produced the contemporary capitalist economic system (*sic*), has acquired particular characteristics from this source which are of constitutive significance for its essence." In other words,

> 1. I should have made an assumption that Rachfahl himself denounces in other places. This is the idea that some sort of "capitalist spirit," regardless of how defined, singularly created out of itself the capitalist

economic system—a purely spiritual construction that I have clearly rejected in my essays.

2. Furthermore, I should have, if I understand correctly, *assumed* that which, among other things, I was seeking to *prove:* namely, that the vocational ethic emanating from the Reformation (R.'s *pars pro toto* will be ignored here) exerted a decisive influence on the formation of a "variation of the capitalist spirit." We will let this expression pass without comment.

3. Then I should have determined the boundaries of its expansion. This is something I could not explicitly pursue in that essay and something that, due to its conceptual framework, is in part not possible to pursue at all and in part not yet possible, at least not in the segments that have been previously published.

4. On the basis of this, I might have finally discussed the distortedly posed question (see no. 1) of its "qualitative impact."

5. Thus I would have defined the problem in a manner that in no way corresponded to my intentions. The progress of an expanding capitalism was not my central interest; rather, it was the development of humankind as it was produced through the confluence of religiously and economically determined factors. This was clearly articulated at the end of my essay.

It appears, however, that in order to execute such a plan, insofar as it is sensible, I would have had to place at the forefront of the discussion a definition of the many characteristics contained in the complex concept of "spirit of capitalism." Without this it is not possible to establish the existence of a "variation." Yet I have explained in my essay why this was not done and why, if I were not to rape history, it could not be done. A specific historical structure such as that which is at first only ambiguously conceived under this name can only be imbued with conceptual clarity through a synthesis of its individual components as they are revealed in the reality of history. (I notice no attempt is made in the criticism to refute these assertions.) Thus in the reality of the historically given we find particular individual characteristics of a variously mediated, refracted sort, more or less logical and complete, more or less mixed with other heterogeneous characteristics. The most prominent and consequential of these features are selected and combined according to their compatibility. Thus an "ideal type" concept is constructed, an analytical category which the content of an actual cross-section of history *approaches* only in varying degrees. In fact, every historian employs, consciously or more frequently unconsciously, continuous concepts of this type whenever precise concepts are used. I have repeatedly discussed this point outside of these essays without, at least up to now, encountering any disagreement—without, however, somehow deluding myself that through these methodological attempts this not so simple problem is conclusively settled. I have certainly much stronger cause to think quite modestly about my work in this direction up to now.

In any event, the case in question is a very complex historical phenomenon. Thus it is possible to begin only with the apparent givens and gradually attempt, by sorting and abstracting, to grasp the concept through the exclusion of that which is nonessential. Accordingly,

1. I proceeded to draw attention through illustrations to the up to now uncontested fact of the remarkably strong congruence between Protestantism and modern capitalism (capitalistically oriented occupational choice and the blossoming of capitalism).
2. To illustrate this, I presented some examples of such ethical life maxims (Franklin) which would undoubtedly be considered manifestations of a "capitalist spirit." I then posed the question of how these life maxims are differentiated from divergent ones, especially those of the Middle Ages.
3. Then I sought, once again through examples, to illustrate how such spiritual attitudes are *causally* related to the economic system of modern capitalism.
4. In so doing, I came upon the notion of "vocation" and pointed out the long noted (especially by Gothein)[2] affinity[3] between Calvinism and capitalism—an affinity also observed of the Quakers and similar sects.
5. At the same time, I attempted to demonstrate that the contemporary concept of vocation was somehow religiously rooted.

This then raised a problem not for the entire series of essays as originally planned (as was noted at their end) but clearly for the succeeding expositions that appeared in due course in the *Archiv für Sozialwissenschaft.* This was the relation of Protestantism in its various shadings to the development of the idea of a vocation through its specific impact on the development of those ethical qualities of the individual which would influence a propensity toward capitalism. This question of course only makes sense when in fact such religiously determined specific ethical qualities exist. What kind they might be was at first possible to indicate only generally through illustrations. In conjunction with the statement of the problem itself, it was necessary, in sharpening the evidence and supplementing what was already stated in the development of the problem, to demonstrate that such qualities in fact are found in particular aspects of the Protestant ethic, what they might be, what kinds of Protestantism would develop them to a specifically high degree, and in what way they

[2] Weber is referring to Gothein 1892.—Trans.

[3] Normally, *Wahlverwandschaft* is translated as "elective affinity." However, this is a term of doubtful meaning in English. It is an archaic technical form stemming from the term *affinitas electiva,* used by Albertus Magnus and Galileo to refer to what chemists now call strong and weak bonds. While it also had this technical meaning in German, after Goethe published *Die Wahlverwandschaften* in 1809, the term took on a wider and less technical meaning that corresponds to the English word "affinity." Since, in contrast to the German, the term "elective affinity" never acquired this less technical connotation in English, *Wahlverwandschaft* has been translated here simply as "affinity."—Trans.

are differentiated from those which were in part instilled or merely tolerated by the medieval church and other forms of Protestantism.

The actual treatment of the problem itself demanded, where possible within the purview of a theological layman, an attempt to identify theoretical and dogmatic anchorings of the ethic among individual variations of Protestantism. This was necessary to show that what are at issue are not merely incidental characteristics that bear little relation to the conceptual content of religion. Yet some very different things had also to be considered. These were the *practical-psychological* motives with which a particular religion imbues actual ethical conduct. Setting aside all his distortions and superficiality for the moment, Rachfahl was not even able to understand that these two questions deal with *quite disparate phenomena.* It is certainly a very important practical and interesting question to inquire into the similarities and contradictions of the ethical ideals implied by the church doctrine of Catholicism, Lutheranism, Calvinism, or others—whether certain types of behavior, practically and psychologically cultivated by ascetic Protestantism, are "required" or are "valid" in church theory, as Rachfahl claims, "also for Catholic lay persons" and not merely for monks. However, this assertion has unfortunately absolutely nothing to do with the question of whether a particular type of religiosity produced a *psychological vehicle* that *tended to create a typical conduct*—conduct compatible with the church doctrine, or representing exaggerated forms of it, or perhaps even actually quite different from it. As I myself have noted, it is evident that conscientious work has been praised and recommended to the lay person of the world in every epoch. In the Middle Ages theoreticians of ethics as well as preachers did this, and Berthold von Regensburg[4] and others are prominent examples of it. In contrast, however, as Harnack once demonstrated in a short essay, early Christendom shared the Hellenic perspective toward work.[5] Luther's pronouncements about this

[4] Also known as Berthold of Ratisbon (ca. 1210–72). He was the most popular Franciscan preacher of the 13th century in Germany and particularly noted for his sermons in German to the common people. After the publication of his sermons in 1824, he was viewed as an important figure in the development of the modern German language.—TRANS.

[5] Adolf V. Harnack, in "Der Wert der Arbeit nach Urchristlichen Anschauung," in *Mitteilungen des Evangelisch-Sozialen Kongresses*, 14 Folge, 1905, Nr. 3/4, S-48. Harnack has strikingly emphasized the fact that the specifically ethical significance of work and its dignity was not originally a Christian notion or even peculiar to it. In addition, see Friedrich Hauck (editor), *Die Stellung des Urchristentums zu Arbeit und Geld*, 1921.

The precept "Who does not work shall not eat" is directed against a particular type of parasitical missionary that has appeared in every epoch. Today it is represented in classical form by Booker Washington's amusing description of the divine "call" common among Negroes when they prefer being saints to being workers. Other instances of this are in part segments of parables or in part eschatologically determined.

topic are well known, and there has been no dearth of teaching outside of ascetic Protestantism on the blessing of worldly endeavors.

But what impact do they have when, as in Lutheranism, no psychological premia are placed upon methodical adherence to the theoretical teachings? Or when, as in Catholicism, much more significant premia are placed upon entirely different types of behavior? Furthermore, the institution of confession in Catholicism permits a person time and again to begin anew and be spiritually cleansed of all transgressions against church precepts.[6] In contrast, in its development since the latter part of the 16th century, Calvinism—and similarly the Baptist sects—caused very specific psychological premia to be placed upon the ascetic regulation of life which Calvinism demanded; the importance of these premia in this area

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Positive connotations of work are found much more strongly among the Cynics and in some pagan Hellenic epitaphs of petty bourgeois circles than among the early Christians. In light of the statements I have made in my essays about the influence of the Old Testament spirit on the Puritan vocational ethic, it is certainly grotesque when Rachfahl holds these things against me—that he knows about such things only from precisely these statements is demonstrated by his vacuous and gratuitous comments. I have also pointed out the well-known manner in which this renaissance of the Old Testament is related to the specific characteristics of Puritan religiosity described by me. Rachfahl also has forgotten this.

[6] This should not be viewed as a statement about the general pedagogical value of the confession. Yet if instructions for confession are examined or information gathered about the questions actually asked in confession, it was and is entirely different issues from those of interest to us. In addition, the history of the prohibition against usury is a good example of the relation, in practice, between Catholic doctrine and economic activities. It is well known that even today it [the prohibition] has not been rescinded, and according to the established maxims of church government, it could not be rescinded. This is because the prohibition against usury is explicitly contained in the Decretals. However, it is a result of false reading from the Greek, which consequently caused a completely mistaken translation in the "inspired" Vulgate. But in practice this prohibition has ceased to be enforced—first definitely so only less than a century ago, when the directives of the Congregation of the Holy Office instructed confessors not to inquire about *usuraria pravitas* through questions about loans. It was assumed that the penitent would remain obedient to the church should the church find it opportune to enforce the prohibition again. [For a definitive discussion of usury and its sociohistorical implications, see Nelson 1969.—TRANS.] This is similar to the public discussions in French Catholic circles which express the desire that confessors no longer need inquire about *onanismus matrimonialis*, a form of birth control used to support the two-child system. Despite the biblical curse against this practice, there has been, to my knowledge, no censure of this discussion by the church authorities.

This type of compromise is typical of the Catholic church. It tolerates this practice *temporum ratione habita*, just as it tolerated in the Middle Ages the de facto existence of certain forms of capitalist activity, though they were in no way positively approved. Exceptions to this were the denunciation of certain forms of capitalism—of which the church made use—and now it even tolerates the use of these forms. [Weber is here referring to usury.—TRANS.] Protestant asceticism, in contrast, created for capitalism the positive ethic, the soul, which those activities demanded in order for "spirit" and "form" to be fused.

can hardly be exaggerated. They resulted from the notion that proof through ascetic conduct is necessary, not only in life in general but particularly in vocational life, as the subjective guarantee of *certitudo salutis*. It is not, however, the cause but, rather, one of the most important manifestations of an individual's predestination for salvation.

It was this phenomenon that I examined in my essay. I had to present the resulting methodical regulation of life, according to the purpose of this kind of analysis, in its specific features and thus in its internal ramifications, as they certainly did not exist consciously in every individual's mind in such absolute consistency and intellectual awareness. Rather, the individual grew up in the atmosphere created by these religious forces.

I sought in part to indicate what powerful support these motives found in church institutions and other social institutions influenced by the church and sects in my essays in the *Archiv für Sozialwissenschaft;* in part, I tried to illustrate this topic somewhat more clearly in the often mentioned sketches that have appeared in *Die Christliche Welt.*[7] To recapitulate: first, the central ritual of the Lord's Supper acquired a very particular accent in "ascetic" Protestantism. This was the idea that whoever takes part in this act and yet does not belong to God's invisible church "eats and drinks himself to damnation." This notion is one of somber intensity, the great burden of which has disappeared today almost completely, even among most Christians. Yet a feeling for it may be recaptured from a waning generation's memories of its youth and from the residues of the religiously linked gravity of life which has been, so to speak, shunted off to religious backwaters. There is nothing similar in ascetic Protestantism, by no means incidentally, to the institution of confession which accords a Catholic relief from the pressure of the sobering question of his individual qualification for salvation. In this respect also and in general the problem of whether a person belongs to the elect is unraveled in Protestantism quite differently from the medieval Catholic manner of adding up and balancing transgressions against good deeds, with the result of a more or less approximate adequacy which then might be supplemented through the use of church sacraments and devotions. Rather, as I have described, the question of salvation is answered most especially in ascetic Protestantism in terms of an inflexible either/or of the total personality, which is manifested in the entirety of the ethical conduct of life. On the ground of ascetic Protestantism, the individual confronts his God in an immensely harsher manner than on that of Lutheranism, thrown back solely upon himself and his state of grace, as it can only be recognized in the totality of his conduct of life. On the other hand, on this ground the external aspects of the organization of life

[7] Weber is referring to Weber 1906*c*, an expanded version of Weber 1906*a*, 1906*b*, translated in Weber 1946.—TRANS.

are much more controlled by an individual's peers, namely, the members of the congregation. In Catholicism, as in Lutheranism, it is in the last analysis only the representative of the "office" who must determine with the individual communicant if the latter is qualified for communion. In Calvinism, the entire social life is unambiguously directed toward the exaltation of "God's glory," a burden which is in this way alien to the other great churches. The responsibility of preventing its desecration by the participation in communion of a bearer of obvious signs of damnation belongs to every individual member of the congregation. Just a short generation ago, the laity provoked the Kuyper schism, named for the church elder Kuyper.[8] In this case, the exclusion from communion of confirmants judged unacceptable by the members of the congregation was demanded, although these confirmants had been examined by clergy from outside the local church and found acceptable. Underlying this protest was the principle that no authority, whatever it might be, which did not reside concretely in the communion community and whose probity was not controlled by the congregation itself could interfere in this question which directly affected each individual member of the congregation. The powerful social significance of this notion appeared most sharply in the New England churches. There the demand for the *ecclesia pura* and the purity especially of the communion community produced "class differences" in the strictest sense. It was the occasion of struggles and compromises over the position of the "near Christians" and their right, for example, to present their children for baptism and to represent them within the congregation and other similar issues.

When we review the church precepts of Protestantism and their development and, insofar as it is possible, examine their actual implementation and think through their consequences, it becomes clear that a significant part of the moral regulation of life had been assumed by the churches, in very different degrees of course. Earlier, during the Carolingian era, this power was held by ecclesiastical courts (*Sendgerichte*). In the late Middle Ages, it was largely in the hands of the city; and in the time of the territorial state, in the hands of the royal police. Generally this control was weaker in Lutheran than in Calvinist areas, where, as I have earlier indicated, the express submission to church discipline, after a believer was admitted to the congregation, increased exactly after Calvin's lifetime. Yet, as I noted previously, what was even stronger and more effective—and its residue may

[8] Abraham Kuyper was a conservative minister in the Dutch Reformed Church and the events Weber referred to occurred in 1886. They led to the formation of the Gereformeerde Kirk, sometimes called the Dutch Calvinist church. Kuyper later became a representative in the Dutch Lower House and leader of the anti-revolutionary party. He was also professor of dogmatics at the Free University of Amsterdam.—Trans.

still be found today—was the kind of ethical training that the ascetic sects imposed upon their members. I have described it on the basis of recent observations in the United States in the already cited essay published in *Die Christliche Welt*. However, the contemporary process of secularization in American life, combined with the enormous immigration of heterogeneous elements, is rapidly sweeping this residue away. In addition, the ruthless pursuit of "spiritual booty" among competing denominations has weakened the intensity of their educational achievements. Nevertheless, with a bit of attentiveness it is possible to recognize even in the residues the importance which ethical training once had.

I recall what I have said in other places about the function of sects in economic life, a function that nowadays is gradually being taken away from them by purely secular organizations. An excellent example of it, in lieu of numerous similar experiences, is how a young man's membership in a Baptist church in North Carolina was motivated by his intention to open a bank. Closer questioning revealed that this was not done to gain advantage with potential Baptist clients; rather, it was done with precisely the non-Baptists in mind who made up the vast majority of potential clients. The reason: any person desiring to be baptized had to submit to an astoundingly thorough inquiry about his past by the congregation during his catechumenate. Included were such questions as, Ever visit bars? Ever been drunk? Ever play cards? Ever "live an unclean life"? Questions were also raised about wastefulness, whether checks and other bills were paid on time and whether there were traces that indicated economic unreliability of any sort, etc. The applicant had to expect that inquiries would be made at all previous places of residence. Once the young man was accepted, his credit and business qualifications would be guaranteed, and he would be able to best every competitor not so legitimized. On the other hand, a possible exclusion because of unacceptable behavior would mean, as always with such sects, his social excommunication.[9] We can find the same

[9] I have compared the credit worthiness created in this manner with the particular borrowing power of members of German student clubs. In my time it was possible for a student to live almost for free in Heidelberg after receiving the "ribbon" of a student fraternity; and in the case of Fuchses, the novice members, creditors recorded debts of the students with the university registrar. The credit accorded clergy in the Middle Ages, over whom excommunication hung as a means of pressure, was also similar. Likewise, the often hazardous credit attributed to the modern young officer, over whom the threat of dismissal hangs, belongs to this sort. Yet there is a significant sociological difference between these instances and that of religious sects in which good credit is perceived as a subjective quality of the personality through the selection and acceptance by the sect after suitable instruction. In contrast, the other examples represent only a reinforcement of the creditors' objective guarantees of repayment. This was also a secondary effect in the case of the sects. Psychologically, an entirely different situation from the previous ones was created by the characteristic institution of Methodist youth training and the equally characteristic use

state of affairs 200 years earlier. For example, the Quakers have always prided themselves on their introduction of the fixed-price system, which is of great importance to capitalism, in place of the oriental custom of bargaining. In fact, historical analysis shows that 200 years ago the blossoming of Quaker retailing may be traced to the fact that customers felt more secure because of this basic policy, more secure than any of the price regulation of the Middle Ages or the present day could make them. Also the Quaker congregation stepped in with assistance if someone began a business but did not possess the necessary capital or experience for its management.

The literature of all these sects, only shortly after their emergence, manifests the exultation that the Lord has clearly blessed them because the "children of the world" would entrust their money with them, either as deposit, investment, or in any other form, rather than with others even of their own confessions. This was because with members of such sects they were more certain of the prerequisite personal ethical guarantees.

For further similar individual examples, I refer to my sketch in "The Protestant Sects and the Spirit of Capitalism" and limit myself to one more observation. Everyone knows that even up to the last decades a Yankee of the old cut, and in particular the businessman, simply could not understand—and occasionally today does not understand—how a person could not belong to some denomination. It makes no difference what denomination, and in this respect there is complete tolerance. Such a religious outlaw would be suspicious, both socially and with respect to business, because of his deficient ethical legitimation. Similar tendencies are discernible now and then in Scotland and in English bourgeois circles. At least until 15 years ago even the tourist could see this, especially on Sundays. Where it has been possible to escape the earlier overpowering pressure for religious legitimation, the businessman of the American middle class has instead at his disposal other rapidly growing organizations. For purposes of legitimation, he often wears the badge of such an organization in his lapel, demonstrating that he has all the qualities of a "gentleman" by virtue of his admission by ballot. Great numbers of such insignia, which are reminiscent of the Legion of Honor rosette, can be observed if attention is paid to them.

Insofar and as long as the genuine Yankee spirit was dominant, the American democracy, even without all the trusts and unions, was never a simple sand heap of isolated individuals. On the contrary, it was to a great extent a maze of exclusive associations, the archetype of which is the sect.

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of small group meetings for the regular examination of spiritual states, a limited open confession. In contrast to the Catholic confession, which occurred behind closed windows, these meetings were conducted in front of a number of personal equals. This situation has disappeared, but earlier it meant very much.

They demanded and induced as a self-evident condition of membership those qualities in participants which defined the gentleman of affairs as capitalism needed him. Of course a man in the position of Mr. J. Pierpont Morgan does not require this legitimation in order to wield the power of his economic position; and, in addition, things are quite different today. However, the penetration of the entire life by the distinct spirit which was furthered by these associations was a particularly significant prerequisite for modern capitalism to take root. In the broad strata of the middle class and finally also among the masses which the capitalist mechanism incorporated, modern capitalism encountered an adequate life-style and through it gained massive control over life in the manner that it has. Understandably, historians of Rachfahl's sort have no idea of the enormous amount of reeducation necessary to make this possible.[10]

Yet perhaps someone might offer an "obvious" conjecture based on the kind of "common sense" for which Rachfahl praises himself so loudly. Namely, those qualities of religious training that breed businessmen and the entire relationship between specific characteristics of business and religious qualifications are altogether the consequence of religious sects developing precisely in an already capitalistic milieu. However, I would ask why no such association appears in the Catholic church and why it did not develop a form of education oriented toward capitalism. Why not in the great centers of the Middle Ages such as Florence, that were, God knows, capitalistically developed to quite another degree than the still thinly populated rural region of western North Carolina, about which I have spoken, or the American colonies with their largely subsistence economy, where 200 years ago the same thing happened? Why has it not developed in Lutheranism?

[10] Education with a prevailing interest in the "hard facts" is an old and, in a very specific way, as I have indicated, religiously anchored principle of Pietist pedagogy. Among the Quakers and Baptists, something very similar may be found from their very beginnings. Today among the reformed sects it is not infrequently exhibited, for example, in the distribution of members among business and other schools and in occupational preference. These specifics are without doubt very important in determining the relation between these forms of religiosity and the development of modern capitalism. Likewise, the well-known accomplishments of the Reformation are certainly of great significance in the area of public education. Yet this latter, very general relationship has its limits. The achievements of the Prussian state in public education are absent from the most capitalistically developed country of the time, England. It is well known that elementary education as such and capitalist development are not parallel. It is a notable exaggeration to say, as Rachfahl does, that no apprehension about increasing popular education exists or existed within Protestantism. This apprehension is particularly true of our staunchly Protestant establishment east of the Elbe. In my essay I have only noted the relationship between certain confessionally determined tendencies toward education and the attitude of *fides implicita*. ["Implicit faith" refers to the commitment of the individual to accept all the teachings of the church, including those that are not understood and those not known.—Trans.]

We must answer that a web has been woven out of psychological tendencies that sprang from very specific ethical religious roots and possibilities of capitalist development. It is true, however, that in surroundings of religious pluralism, this life-style which was cultivated with such powerful energy by the ascetic congregations colored the life-style of other competing denominations from the outset, despite sharp contradictions between them.[11] This became more pronounced as economic life became increasingly permeated with the capitalist spirit, and this coloring may be seen very early in Dutch and American Lutheranism and even in American Catholicism. In German Lutheranism a similar trend may be found, but, as is well known, it emanated from the older Pietism. Naturally this process occurred in such a manner that on the road to "convergence" differences were diminished only by degrees, but never entirely eliminated.[12] Nevertheless, the most significant characteristics of Protestant asceticism, particularly of the Calvinist type, were in fact almost always assimilated according to all we know of it, at least by the Protestant who encountered them. For this reason alone, the simple statistics of the number of Calvinists, for example, among Protestant emigrants provides no argument against the importance of that ascetic form of life. The present-day discussions in Catholicism about how the Protestant superiority in economic qualifications might be appropriated find their counterpart, in subject if not also form, in the remarks of Spener on the prosperous development of the Quakers. Implicitly this similar motive has always and everywhere exerted influence, just as it does now in America.

Setting aside for the moment the question of "inner-worldly asceticism," one might finally question the accuracy of drawing a parallel in essentials between what I meant by this expression and Catholic monastic asceticism.[13] In answering, I shall ignore the fact that medieval devotional

[11] Only 30 years ago, in the confessionally mixed regions of Westphalia, the continual strife between the Lutherans who "dragged salvation through the gutter" (namely, the intestinal canal, because of the *hoc est corpus meum*) and the reformed "hypocritical work saints" was a daily event, especially among the candidates for confirmation.

[12] The distinct nature of the Lutheran Missouri Synod, in contrast to the other denominations, has remained very strong.

[13] It is typical of Rachfahl's manner to make on one side the greatest effort to discredit not only this expression but also the corresponding substantive thesis of its intrinsic relation to the rational monastic asceticism of Catholicism (and this point is in every instance his idea of a so-called critique). On the other hand, he reminds me that in the opinion of respected church historians these specifics of ascetic Protestant religiosity represent an "incomplete" victory over Catholicism. This "incompleteness" suggests a developmental notion accompanying a subjectively unassailable value judgment. It casts Lutheranism, which rejects the idea of holiness through work, as plainly the highest manifestation of Protestantism and constructs a hierarchy downward from this point. Historically, however, the development of inner-

literature of monastic origin, for example, that of Bonaventure,[14] is repeatedly cited by the ethical theorists of such sects, especially the English, with respect to all the prescriptions which I have characterized as "ascetic." Let us simply make a comparison. Monastic asceticism demands chastity. Protestant asceticism, in my sense of the word, demands it also, but within marriage. It requires that all lust be excluded from ethically sanctioned sexual activity, which is to be limited to the rational "natural objective" of the propagation of the species. These regulations were, after all, always more than simply theory, and certain precepts of ascetic Protestantism are well known, such as those of Pietism and the Hutterites, which in part strike us today as outright unnatural. The manner of treating women was deeply influenced by this since it precluded regarding them primarily as sexual objects, in contrast, for example, to Luther's unbroken peasant mentality.

Monastic asceticism demands poverty—and we know with what factual paradox. With the exception of some very strict spiritual orders which were generally treated with the greatest suspicion by the popes, the economic prosperity of the monasteries was almost always considered a consequence of God's blessing and was, along with bequests, in the largest measure a consequence of their rational economic administration. On its side, Protestant asceticism rejected not only the joyful delight of "reposing" among one's possessions but also the striving for possessions for their own sake. I have described the equally paradoxical success that resulted from this. Monastic asceticism required independence from the "world" and especially repudiated naive pleasure. Protestant asceticism did exactly the same, and both converge in the method of exercise (this is finally the meaning of the word "asceticism")—a stringent use of time, work, and silence as a means of suppressing instinctual urges. Furthermore, each compelled detachment from all strong bonds to the creaturely, casting doubt upon all too intensive personal friendships and other similar things.[15] Common to both was the renunciation of pleasure as such, whether it be in the narrowest sense sensual or of an aesthetic-literary type. All use of the goods of this life not justified rationally, such as for purposes of hygiene, was disavowed.

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worldly asceticism is a product of the post-Reformation period and thus more a reawakening of religious motivations which were also promoted by Catholicism, but in an entirely different way and with entirely different results.

[14] Weber is referring to Bonaventure (1221–74), great doctor of the church and Minister General of the Order of Friars Minor. Bonaventure was noted not only for the brilliance of his scholastic philosophy and writings on mysticism but also as a leader of the Franciscans during a period of great dissension within the order. It is his notions about the nature of monastic life to which Weber alludes.—Trans.

[15] An excellent discussion of the attitude toward friendship in early modern times may be found in Nelson 1969.—Trans.

I have extensively reported, down to some detail, how the condition arose that in the Middle Ages the monk was a man who lived "methodically," specifically because of a "vocation." Sebastian Franck's comments reflect a little more understanding of these things than those of my critic.[16] Still, Rachfahl, with his usual accuracy, claims that my scientific thesis is based upon them, although I first mentioned them in my reply to his critique as an example of then contemporary views.

What differentiates rational Protestant asceticism, in my sense of the word, from monastic asceticism is, first, the rejection of all irrational ascetic practices. They have also been equally rejected or restricted by certain particularly important Catholic orders such as the Jesuits. Second, contemplation is repudiated. Finally and principally, the exercise of asceticism is directed toward an inner-worldly way of life, taking effect in the family and in the ascetically interpreted occupation. From this disparity flowed the previously mentioned differences and all the others. Yet if the spirit expressed in the complementary principles of life conduct is not in its intimate core parallel and related, then I do not know when one should speak of "affinity." I mention only in passing something well known, namely, how strongly in Pietistic circles the disappearance of monasteries was sometimes regretted and how quasimonastic forms of organization frequently reappeared precisely in these circles. Furthermore, I refer to what is said about Bunyan in my essays.

The profound tension and deep similarity of both ascetic forms with respect to the place of ascetic ideals in the entire system of religiously oriented life-styles stems from the previously stated causes. What among the monks was considered an actual cause for the expectation of salvation possessed significance in ascetic Protestantism only as a manifestation of salvation. It was not the absolute only one but certainly the most important. Since even modern "methodologists"—particularly those in the field of history, as I have had occasion to note—do not always differentiate these two conditions, it is not surprising that the development of Protestant "work holiness" in practice often looks similar to Catholic characteristics, just as one egg looks similar to another. However, the seeds of these two eggs come from different spiritual fathers, and therefore the fruit developed quite different inner structures.

It would be too lengthy a task to recapitulate the dogmatic foundations of inner-worldly asceticism. For this I must refer the reader to my essay. There it is at least provisionally and schematically indicated that the

[16] Sebastian Franck (1499–1542) was a Lutheran minister who joined the Anabaptists in 1528 and became a well-known minister. Weber is referring here to Franck's idea that after the Reformation "every Christian had to be a monk" (Weber [1920] 1958, p. 121).—Trans.

question of whether this foundation was created by the Calvinist doctrine of predestination or the nontheological dogma of the Baptists is by no means irrelevant for the practical orientiation of life, despite all assimilation. These differences, which in many ways are easily felt, were necessarily overshadowed by points of similarity only in the part of my argument which has been published up until now. Detailed comment would be too much at this point. Yet I must emphatically point out that the basis of the empirical verification of the question of whether those fundamental religious-psychological relations actually had the specific impact for the practice of the conduct of life, as I have maintained, derives in my essays neither from the treatises on dogmatics nor from theoretical treatises on ethics. Rather, it comes from a wholly different set of source material. In particular, I singled out Baxter's and Spener's writing on pastoral care and, most prominently, their answers to inquiries from those seeking pastoral direction about concrete, practical problems in their lives.[17] This material represents a type, insofar as it mirrors practical life, that corresponds approximately to the *responsa* of the Roman jurists to the business and law practices of that time.[18] Certainly these and similar works contain casuistic speculations by their authors, as was also the case with the Roman jurists. This is also true to an enormous degree for the Talmud, which is not comparable to this case or any like it but is likewise closely tied to immediate, practically revised responsa material. Fortunately the form and the structure often reveal quite sufficiently, but not always, where something has been drawn from life. Where this is the case, there is no source, with the exception of personal correspondence and possibly autobiographies, that approaches the same authenticity and vitality. Neither popular pamphlets and tracts nor sermons, which it is naturally completely legitimate to use fully as supplementary material, provide the same reliability. Even less reliable are the creations of a contemporary literature, regardless of how important a secondary source they might be. Most doubtful of all are the external protestations of confessionality by individual capitalistic groups, especially when the influence exerted on them by the "atmosphere of life" created by Protestant asceticism is not considered. Unfortunately, we are infrequently in the favorable position of being able to see as clearly the interaction between religious and capitalist interests as in the example I

[17] Weber is referring, here and previously, specifically to P. J. Spener's *Theologische Bedenken* and Richard Baxter's *Puritan Directory* and *The Saint's Everlasting Rest*. However, Weber was quite familiar with the entire corpus of reformed devotional and pastoral literature, and throughout *The Protestant Ethic* there are numerous references to authors such as Robert Bailey, John Bunyan, and Thomas Adams (see esp. the notes to Weber [1920] 1958, chap. 4).—TRANS.

[18] A discussion of the *responsa* can be found in Schulz 1936.—TRANS.

cited of the weavers of Kidderminster.[19] This fact, however, does not vitiate in any way the importance attached to such studies, as Rachfahl would wish.

The specific impact that a particular form of religiosity might possess could only be determined, in my opinion, through the method I followed, and this issue was clearly my primary concern. This impact did not represent merely an acceleration of an already existing psychological disposition. On the contrary, it implied, at least within the worldly sphere, a new spirit. From their religious life, out of their religiously conditioned family traditions and from the religiously influenced life-style of their environment, there emerged a "habitus" among individuals which prepared them in specific ways to live up to the specific demands of early modern capitalism. Schematically expressed, an entrepreneur with an untarnished conscience stepped into the place of the one whose desire for gain was at most tolerated by God—like the Indian merchant of today who must expiate or make up for his *usuraria pravitas*. This entrepreneur was filled with the conviction that Providence had shown him the road to profit not without particular intention. He walked it for the greater glory of God, whose blessing was unequivocally revealed in the multiplication of his profit and possessions. Above all, he could measure his worth not only before men but also before God by success in his occupation, as long as it was realized through legal means. God had his reasons as to precisely why he was selected for economic success and provided with the means to achieve it, in contrast to others whom God had for good, but of course inexplicable, reasons destined for poverty and hard work. With pharisaical certainty this man went his way in strict legal formality, which was to him the highest and—because there is absolutely no "sufficiency" before God—the singular virtue of importance that may be recognized with certainty. And on the other side, as a skilled craftsman of the cottage industries or as a worker, stood the man of specific industriousness, to whom conscientiousness in his God-intended vocation yielded knowledge of his state of salvation.

The denunciation of particular forms of wantonness, such as the idolatry of the flesh by living easily on one's possessions, delighting in pleasure, or wasting time or money for nonvocational purposes forced entrepreneurs to use possessions gained through vocational activity continually in the vocational manner of capital investment or saving and, through this, possible upward mobility for the ethically qualified poor.

What is most decisive is that occupation and the inner ethical core of the personality formed one unbroken whole. The many isolated beginnings of a practical vocational ethic of this sort in the Middle Ages—I have

[19] Weber is referring to Baxter's comments about the weavers of Kidderminster; see Weber (1920) 1958, chaps. 4, n. 152, and 5, n. 17.—Trans.

purposely refrained from commentary upon this—does not alter the fact that exactly such a spiritual bond was lacking among them.[20] And in the present which operates as readily with the concept of "life," "experience," etc., as with a specific value, the subjective dissolution of this unity—the denigration of the occupationally committed person—is obvious. However, this support has long since ceased to be necessary for modern capitalism. Yet those modern sentiments just indicated have rebelled against its drive—not simply for social-political reasons but, rather, primarily because of its link to the spirit of the "vocational man."

It is true that the significance of religious precepts for life has left a residue that we may still find today, as I have repeatedly demonstrated within and outside of my essays. Where industry still seeks those qualities of its personnel which follow from such a life-style, this frequently manifests itself in the confessional composition of the work force. For example, it may be seen among the foremen and white-collar workers who have risen from more humble beginnings, in contrast to the unskilled workers. This is likewise the case for those who have become entrepreneurs. This all may be discerned statistically only if those contingencies are excluded that result from geographical location, which is often solely determined by the existence of indispensable raw materials; however, the matter is further complicated because handcraft shops are not identified in the statistics. Yet on the whole, modern capitalism is, as I have said, exceptionally emancipated from the importance of such ethical factors. However, with respect to the period of early modern capitalism, it has not occurred to anyone to doubt, for example, that the development of French bourgeois capitalism was closely linked with the Huguenot movement. Further, after the repeal of the Edict of Nantes at the end of the 17th century, the Huguenots emigrated and exported their typical qualifications by no means only to countries with underdeveloped economies but especially also to Holland, where, as I have noted, capital investment was either differently structured or, if only in certain strata, languished in favor of living the life of a rentier, enjoying ostentation and similar things. The assertion that the manner in which bourgeois capitalism developed in the northern states of the United States was not in a very specific way influenced by the equally specific life-style elicited by Puritanism was withheld in Rachfahl's first critique, but not his reply. In his usual hazy manner, he admits himself that the same condition was true of England. In Scotland

[20] Insofar as the opportunity presented itself, I have indicated elsewhere that the emergence of *homo economicus* depended upon some very specific objective conditions. Among them were the geographical, political, social, and other conditions of culture in the Middle Ages, in contrast to those of antiquity. *Handworterbuch der Staatswissenschaften*, 3 Auflage: "Agrarverhaeltnisse in Altertum." [Translated in Weber (1908) 1976.—TRANS.]

the English romantics had already recognized the same relationship.[21] Gothein has identified the same phenomenon in Germany, and I have added some examples to his.

In the case of Holland, I have cited reasons why, in this instance—and I repeat—the very same impact of ascetic Protestantism was to a great degree weakened by a mixture of previously, but only partially, indicated factors. And I do not flatter myself in the least by imagining that I have mentioned even the most important ones.[22] These factors reflect the strikingly early stagnation in Dutch capitalist expansion, and I do not refer especially to the colonies.[23]

All these stimulants, which corresponded to the economic characteristics of particular sects in the Middle Ages, have been known in large part since the 17th century and have not been doubted up to now by anyone familiar with the facts. My statements about ethical incentives are in no way shaken, for reasons mentioned above, by probable historical findings which in themselves may of course be valuable, such as that there were Dutch Lutheran immigrants in Frankfurt along with Calvinist ones. For these reasons such things were, as I have already said, merely noted in my essay. Likewise, I would once again point out that the Russian sects and schismatics that exhibit ascetic-rational characteristics as the core of their nature—and certainly not all of them do—manifest very similar economic characteristics as soon as their youthful estrangement from the world is past. The most explicit extreme of the combination of business qualifications and ethical renunciation of the world may be found in the castration sects.[24]

These original remarks had to be restricted to the use of familiar

[21] Compare, for example, the letter from John Keats to his brother Thomas (July 3, 1818). "These churchmen" have turned Scotland into "colonies of savers and successful entrepreneurs" (the antithesis of Ireland, from whence he writes).

[22] However, naturally not through the predominant adherence of certain political upper classes to Arminianism, which I mentioned in my essay—or their indifference. This was equally true in other places. In Holland too it was these upper strata that most sought, as in England, to withdraw from capitalist activity, at least partially, by "aristocratizing" their fortunes through the purchase of noble estates. That Rachfahl additionally, in the light of my explicit comments about Arminianism in my essay, allowed himself to say that these well-known things were unknown to me and furthermore, when I pointed this out to him, that he found it appropriate to repeat this to his public only reflect everything that I really would like not to reiterate about him continually.

[23] In order to avoid any misunderstanding, this stagnation had certainly very significant internal and external political causes. However, on the other side, this in no way eliminates the effect of the breakdown of ascetic inclinations. I would be entirely unable to answer this question adequately, and no one else can either.

[24] Weber is referring to the Skoptsy, a castration sect established in the 1770s. See editor's note in Weber 1968, 2:610. Also, for a more extended discussion of the relation between ecstatic sexuality and religion, see ibid., 2:602–4.—Trans.

things as illustrations, and despite Rachfahl's pedantry, it must remain so. It would be both useful and necessary for special historical analyses of the development of particular regions if there were further investigations on the amount of influence exerted by the individual confessions. But also needed—in fact much more so—are comparisons of the manner of development in the particular countries influenced by ascetic Protestantism; only such studies can provide some insight into the causes of observed differences in their development. Yet the most pressing questions lie, at least for me, elsewhere. First, the differential impact of Calvinist, Baptist, and Pietistic ethics on life-styles must be pursued in much greater detail. Furthermore, thorough investigation of the beginnings of similar developments in medieval and early Christianity would be of great importance, insofar as the work of Troeltsch leaves some room here. This would of course demand intensive collaboration with specialists in theology.[25] Finally, research is needed to explain, from the economic side, the constantly reappearing distinct affinity of the bourgeoisie for particular life-styles that, while ceaselessly changing, remain continually anchored in the same manner. An important aspect of this, but by no means the only one, consists of the study of individual elements of religious life-styles as they are significantly exemplified by ascetic Protestantism. Many isolated comments from quite different perspectives have already been made about the general problem; yet in my opinion, much of a fundamental nature remains to be said.

At least it is possible to give a short answer to one last question, one which Rachfahl juggled in a most helpless way. Namely, which figures in the entire picture of modern capitalism might be understood without reference to "inner-worldly asceticism"? My answer is the adventurers of capitalist development. The notion of "adventurer" is meant in the same manner as G. Simmel recently defined it in a short, charming essay.[26]

[25]That a number of colleagues who are respected theologians view my endeavors not without interest nor without encouragement is adequate proof for me that there is interest in the subject. I understand entirely that this type of tracing of the relationship between certain religiously motivated tendencies and their consequences for bourgeois life must appear to do injustice to the ultimate value content of the respective religious forms. For persons with sensitive religious natures, the coarseness and externality of these motivations, when assessed religiously, would place them on the periphery of genuine religious concern. This is in fact so. Nonetheless, this purely sociological work must also be undertaken. Among theologians themselves, Troeltsch is prominent in pushing it forward. Certainly it would be best done by experts themselves, since we outsiders in our fashion and from our perspective merely present here and there some possibilities of problematics that may interest them. Perhaps they will now wish to consider these attempts, either critically or with approval. This was my hope; and I now await fruitful and informative criticism from the theological side, but not from dilettantish, bungling polemicists such as Rachfahl.

[26]Published in *Philosophische Kultur: Gesammelte Aufsaetze* (Leipzig: Klinkhardt, 1911); English translation in Simmel 1971.—TRANS.

Their economic importance within the history of early capitalism—but not only there—is recognized as particularly significant. Yet the development of the growing domination of the entire economic system by capitalism may be conceived in a certain sense—and *cum grano salis*—as the development from economic profits of opportunity to an economic system. The genesis of the capitalist spirit in my sense of the word may be thought of as the development from the romanticism of economic adventures to the rational economic conduct of life.[27]

And finally, someone might like to know my opinion of the probable fate of capitalist development, as an economic system, when we *exclude* the unfolding of the specifically modern elements of the capitalist spirit. It might be recalled that Rachfahl has thrown in a few remarks about this that in my opinion are quite frivolous. The question can honestly be answered only summarily in the following way: we do not know. Yet some major characteristics of the development up to now must be recalled, at least for interested readers who are not specialists. They frequently do not escape the common error of thinking that particular technical achievements were the sole cause of the development of capitalism. Yet the capitalism of antiquity evolved *without* technical advances and in fact occurred simultaneously with the cessation of such advances. The technical progress of the Continental Middle Ages was surely of no little importance in creating the *possibility* for the development of modern capitalism, but certainly it constituted no decisive stimulus for development. Objective factors such as certain aspects of climate, which influence life-styles and labor costs, count among the most significant prerequisites, along with the political-social organization of medieval society and the subsequent characteristics of the medieval city, particularly those of the inland city and its bourgeoisie. Medieval society and city were to a considerable degree produced by what was, at least in comparison with antiquity, an inland culture. (See my previously cited article in the *Handworterbuch der Staatswissenschaften* [n. 20 above].) In addition, new forms of productive organization in trade, for example, cottage industries, were specific economic influences; although not entirely alien to ancient culture, they displayed a unique structure, diffusion, and importance.

The great process of development which lies between the highly labile late medieval developments toward capitalism and the mechanization of technology, which is so decisive for capitalism in its contemporary form, culminated in the creation of certain objective political and objective economic prerequisites which are so important for the emergence of the latter. It culminated especially in the creation and diffusion of the ra-

[27] This demands, of course, a closer examination that cannot be cursorily presented here. Viewed purely objectively, a daring entrepreneurial risk is in no way an adventure when substantively it is a given component of a rationally calculated enterprise.

tionalist and antitraditionalist spirit and the entire range of behavior to which in practice it was assimilated. Major insights into this phenomenon may be furnished on one hand by the history of modern science and its practical relation to the economy, which has only developed in modern history, and, on the other hand, by the history of the modern *conduct of life* and its practical meaning for the economy. Aspects of the latter were the subject of my essays and will probably continue to be so. The development of the practical-rationalistic regulation of the conduct of life is obviously something quite different from the development of scientific rationalism and is not necessarily associated with it. The first foundations of modern natural science emanated from Catholic regions and Catholic minds. However, the first attempt to apply science methodically to practical objectives is primarily Protestant. Likewise, it seems that certain conceptual principles which are important for the regulation of conduct have a kind of affinity to the Protestant way of thought. (A more extensive comment is beyond the scope of this essay.) It would be entirely incorrect to assume that faith as such was an obstacle to the development of empirical science either at that time or later. The majority of the heroes in English natural science offer ample evidence to the contrary, from the 17th century up to Faraday and Maxwell, one of whom is known for having preached in his sect's church during the 19th century. The practical and not merely incidental but, rather, methodical inclusion of natural science in the service of the economy is one of the keystones in the development of regulation of life in general. Certain specific influences from the Renaissance as well as the Reformation, especially through the transformation which I have described in a fragmentary way, have contributed decisively to this development. If I were asked seriously how high I estimate the importance of especially the last factor, my answer, after repeated careful examination, is *very* high. That no "numerical" scale of importance exists in historical accounting is something I cannot control.

Enough and more than enough. The mass of the public cannot now be expected to give the "criticized" work a thorough reading just because they have read an unworthy "critique" and one plainly devoid of understanding. To most readers, this sort of belligerent character, like the "critic" Rachfahl, will always appear to be correct—that he belongs to such ranks I believe I have demonstrated. It would seem inconceivable to persons who do not know the situation well that a professor of history stepping forward with such a sense of assurance would, as a result of a strikingly superficial and biased reading, fundamentally misunderstand the entire question at issue and then not muster the necessary qualities to admit this error after it had been demonstrated to him. That, unfortunately, does nothing to change the fact that this sadly *is the case* or, regrettably, that I had to prove this at the cost of space in a journal

which is unable to accept such a profusion of necessarily sterile polemics—through the explicit fault of the critic—as apparently the *Internationale Wochenschrift* is able to do.[28]

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[28] When his earlier statements are compared with his present statements, one quickly comes to the speculation that the latter represent more a type of penalty for my certainly irreverent conduct than anything else.

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Black Ghetto Diversity and Anomie: A Sociopolitical View[1]

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On the basis of responses to two "anomie" scales across different black neighborhoods, this study reports findings compatible with the viewpoint that the American black urban ghettos harbor a rich variety of subculturally distinct residential areas. More specifically, it suggests that sociopolitical variables which highlight the relationship between neighborhoods and the structures of power in the city are a more important source of subcultural differences between black residential areas than are variables derived from census data such as rate of racial turnover.

The scholarly literature on black Americans has largely ignored the issue of black neighborhood diversity. That is, investigations of the black experience have generally failed to seriously explore the possibility that within the black ghettoes of our cities exists a rich variety of subculturally distinct residential areas.[2] An example of this tendency is the large body of sociological and anthropological essays and ethnographies which debate whether blacks in the United States have created a viable culture distinct from that of the dominant society (see, for example, Keil 1966; Liebow 1967; Hannerz 1969; Berger 1970; and Blauner 1970). Characteristically, participants in this controversy have countered one narrow conception of the black ghetto with another, ignoring altogether the possible linkages between distinctive black subcultures and neighborhood-level characteristics such as rate of racial change.[3] One might ask whether the actual black residential areas described in this literature (e.g., the black streetcorner

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[2] For stylistic reasons, the term "neighborhood" is used in this paper as a synonym for the value-neutral concept of "residential area."

[3] The best of the debaters is Hannerz (1969), who offers a rich and detailed description of the different life-styles coexisting within a single Washington, D.C., black ghetto neighborhood. A follow-up to Hannerz's study might explore whether the distribution of these competing life-styles in different black ghetto neighborhoods is affected by neighborhood-level characteristics such as rate of racial turnover.

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district located in a highly transient area of a Washington, D.C., black ghetto which Liebow [1967] observed) are really as representative of life in all black ghetto neighborhoods and, therefore, as interchangeable as their authors indeed imply? One recent study suggests they are not.

After examining responses to Srole's (1956) anómia scale in three black inner-city residential areas of a medium-sized Northeastern city (population 83,000), Wilson (1971) found that scores on the scale among blacks were significantly lower in a racially stabilized black neighborhood than in two emerging black areas where rapid racial succession had occurred during the previous decade. These findings support Wilson's overall theoretical orientation, namely, that "anomie is primarily a function of community stability, independent of poverty" or segregated living conditions (Wilson 1971, p. 86).[4] Moreover, they are compatible with the assumption of black neighborhood diversity to be explored in the present paper and suggest that the rate of racial turnover of neighborhoods might be an important source of this diversity.

Specifically, Wilson posits the disruption of neighborhood institutions resulting from rapid population turnover as a fundamental cause of anomie[5] among residents of racially transitional areas. Conversely, he characterizes the racially stabilized black neighborhood as a "relatively stable social system" which helps integrate social ties, producing among its residents low anomie. A more schematic representation of the Wilson community stabilization interpretation of anomie is presented in figure 1. In this model, social integration functions as a consequence of neighborhood stability and as a cause of low anomie. If social integration operates

Independent Variable		Intervening Variable		Dependent Variable
Neighborhood Stability	⟶	Social Integration	⟶	Anomie
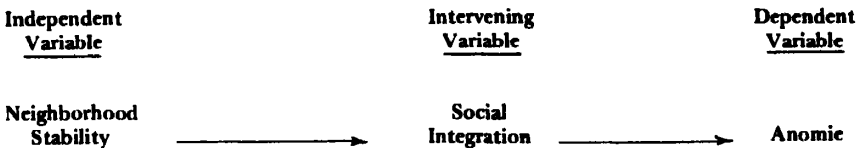
FIG. 1.—Community stabilization model representing social integration as an intervening test factor between neighborhood and anomie.

[4] Wilson (1971) never explicitly defines "anomie" apart from his operationalization of it (i.e., Srole's anomia scale), thus, in effect, ignoring the analytical distinction between *anomie* as a condition of a social system and *anomia* as a subjective condition of individuals (for the classic discussion of this distinction, see Merton [1964, pp. 225–30]).

[5] Throughout the introductory and "Theoretical Perspective" sections of this paper, the term "anomie" is employed as a proxy for the phrase "high scores on Srole's anomia scale." Responses to Srole's scale are interpreted as a measure of subjective feelings of despair and hopelessness rather than as a measure of the degree to which a person feels normless or confused. A clarification of how the term "anomie" will be used in later sections of this paper can be found in the discussion of dependent variables below.

as an intervening test factor, then holding it constant would cause the relationship between neighborhood and anomie to sharply decline or disappear.

Less compatible with Wilson's initial argument, however, is his discovery that after simultaneously adjusting for various controls, including several social integration measures, the neighborhood effect remains strong, explaining about 7% of the variance in anomie scores. This leads Wilson to speculate further about the "kind of social integration process" that may protect residents of racially stable ghettoes from "various strains toward anomie . . ." (Wilson 1971, p. 86). Without referring to any additional data, he postulates that anomie results when individual and collective success goals are regularly frustrated by limited opportunities. In a stable black residential area, blacks may learn to redefine success "in ways in keeping with the shared symbols of a black, lower-class reference group" (Wilson 1971, p. 86), thereby reducing strain. Racial stability, Wilson suggests, may not be a sufficient condition for activating this culture-building process among ghetto residents. Only if the ghetto is sufficiently isolated from the normative influences of the wider society will a subculture emerge where middle-class success values are subordinated to black lower-class ones. Wilson's argument also implies that racially unstable ghetto neighborhoods lack both the stability and the isolation necessary for generating a black subculture.

The present study does not question the validity of Wilson's substantive findings. However, it does take issue with his reference-group interpretation of the community stabilization process and his invocation of it as a possible explanation for the neighborhood-anomie relationship. Before testing Wilson's reference-group explanation (a kind of subculture-of-poverty argument), this paper presents an alternative theory to account for the relationship between type of residential area and scores on Srole's anomia scale.

THEORETICAL PERSPECTIVE

Whether black Americans have developed viable subcultures within the ghettoes of our cities has been subject to much debate (see, for example, Moynihan 1965; Keil 1966; Liebow 1967; Hannerz 1969; Berger 1970; Blauner 1970; Rainwater 1970). Contrary to Wilson (1971) and other subculture theorists (e.g., Keil 1966; Blauner 1970), I suspect that, while distinctive values may evolve in racially stable black areas, an awareness among residents of wider societal values remains strong. The evidence from recent ethnographic studies of low-income black ghetto areas (e.g., Liebow 1967, pp. 213–14; Hannerz 1969, p. 188; Rainwater 1970, pp. 305–6) suggests that blacks, without rejecting the dominant success

values of society, develop a tolerance for lesser degrees of success and for different types of success which are realistic in view of the condition of blocked opportunities in which many blacks find themselves (see also Rodman 1963; Della Fave 1974*a*, 1974*b*). From this viewpoint, which I share, if the opportunity structure was to suddenly improve, blacks would unhesitatingly take full advantage of the new situation, since their preferred goals are those of mainstream society. Thus, I find unconvincing Wilson's argument that the subordination of middle-class success goals to black lower-class ones could account for the lower scores on Srole's anomia scale in Wilson's "Ghetto."

Another possible explanation for the relatively lower level of anomie in certain black neighborhoods, such as Wilson's Ghetto, involves exploring the extent to which a black neighborhood is protected by influential political, administrative, and business interests outside of the community (compare Suttles 1972, p. 239). Wilson's observation that Ghetto seemed "to function with a minimum of conflict—both internally and with outside white institutions—and to have a high degree of internal leadership" (Wilson 1971, p. 72) could indicate the effectiveness of its connections to the wider society, while the relatively high incidence of officially recorded delinquency in Ghetto, compared with the two more transitional areas, might be better interpreted as a reflection of more effective law enforcement in Ghetto than as a reflection of a higher true delinquency rate. These facts suggest that Ghetto's effective linkages to the city, rather than its physical and alleged social isolation, might account for the relatively low level of anomie among its residents. Contrary to Wilson, I postulate, all other things being equal, that a ghetto which lacks effective linkages to the wider society will exhibit relatively high levels of anomie. Suttles's (1972) concept of the defeated neighborhood should help clarify the reasoning which led to my prediction. As described by Suttles, the defeated neighborhood, in contrast to the protected or defended neighborhood, "is subject to insufficient or quixotic enforcement of building standards, zoning rules, police protection, and wide disparities in the delivery of all available community services. Above all it is a community which citywide, regional and federal agencies treat as an object without much fear of retaliation from a local constituency . . . it is a community so heavily stigmatized and outcast that its residents retreat from most forms of public participation out of shame, mutual fear, and an absence of faith in each other's collective concern" (Suttles 1972, p. 239). Hence, one might expect, following the Durkheimian argument which attributes individual malaise to "a relaxation of social bonds" (Durkheim 1951, p. 214), that residents of "defeated" ghetto areas are more likely to experience higher levels of anomie than those of "defended" ones. Alternatively, a line of argument

more consistent with Wilson's "strain" assumptions would postulate that, although residents of "defeated" neighborhoods are more disengaged from societal institutions and, by implication, less committed to wider societal values, they should nevertheless exhibit lower anomie because of their greater likelihood of developing an alternative subculture which would protect them from the normative sources of anomie-producing strain.

The principal objective of the research reported in the present study was to test the relative validity of reference-group and sociopolitical explanations of the neighborhood-anomie relationship in particular and of black neighborhood subcultural diversity in general. The results challenge the accuracy of Wilson's attribution of a relatively low level of anomie to the residents of stable black areas as well as the validity of his reference-group argument. They also underscore the importance of sociopolitical variables as determinants of distinctive subcultures (social worlds) within the black ghetto.[6]

RESEARCH DESIGN

Research Site

To explore these issues, I have examined survey data from two low-income residential areas, "North Ghetto" and "South Side," which between 1950 and 1965 experienced sharply different rates of black population change. The two neighborhoods are located in Western Contra Costa County, part of the San Francisco–Oakland metropolitan region. One of the neighborhoods, South Side, is located in Richmond, a medium-sized industrial city and seaport, while the second, North Ghetto, is a small neighboring, virtually all-black district, only part of which is incorporated in the city of Richmond. (Selected population and structural characteristics of the two areas are reported in table 1.)

South Side.—Between 1950 and the year of this study (1965), South Side's racial composition shifted dramatically from solidly white working class to predominantly black working class. In 1950 only 16% of South Side's population was black; by 1960, 52% was black; and in 1965, the figure was 80%, indicating by Wilson's criteria that throughout this 15-year period South Side was a racially unstable area. (This trend toward complete black succession has continued. By 1970 approximately 90% of

[6] My use of the term "subculture" is identical with Fischer's (1976). He writes: "Subcultures are composed of two parts: a 'subsystem'—a set of interconnected social networks (in a sense, overlapping and superimposed social circles)—and the associated subculture proper—a set of norms and behaviors common to the subsystem. The term 'subculture' will be used for both the network set and its culture; it is loosely synonymous with 'social world'" (1976, p. 259).

TABLE 1

SELECTED POPULATION AND STRUCTURAL CHARACTERISTICS OF NORTH GHETTO AND SOUTH SIDE, 1965–66*

CHARACTERISTIC	RESIDENTIAL AREA	
	North Ghetto	South Side
Total population	4,520	18,000
Percentage black	93	80
Percentage of household heads employed in unskilled jobs	68	42
Percentage point increase in size of black population between 1960 and 1965	2	28
Percentage of adults living in Richmond area less than five years	12	36
Concentration of black-owned businesses	High	Low
Concentration of black-directed churches	High	Low
A settlement house offering a variety of social services	Present	Absent
Police and fire protection	Poor	Satisfactory
Percentage of local residents who complain about local services†	High	Low

* Information derived from the following sources: U.S. Bureau of the Census (1962); Richmond City Planning Department (1967); Richmond Model Cities Program (1970, 1971); a 1965 adult survey described in Research Design, below; and personal field observations.

† A qualitative assessment based on responses to several open-ended items from the 1965 adult survey.

South Side's population was black.) In contrast, the racial composition of Wilson's "moderately" transitional area (Northeast) shifted somewhat more abruptly, from 15% black in 1960 to 85% black in 1969. Like "Northeast" in 1969, South Side in 1965 visibly lacked black-directed churches, local citizen groups, and settlement houses which might contribute to neighborhood cohesiveness. Compared with residents from other black neighborhoods in Western Contra Costa County who were interviewed in 1965, South Side's residents complained the least about the quality of local facilities and city services such as fire and police protection, indicating relatively good structural relations between South Side and local government.

North Ghetto.—Like Wilson's Ghetto, North Ghetto is the city's oldest black neighborhood, its black population first having settled there during World War II. Since the late 1940s, the population has remained at least 90% black, indicating, by Wilson's standard, a racially stable black ghetto. In addition, like Wilson's Ghetto, North Ghetto is geographically well defined, bordered on the north and west by farmlands extending to the Bay and separated from the rest of Richmond on the south and east by railroad tracks, a fertilizer plant, oil refineries, and a dump. North Ghetto's organizational life also resembles Ghetto; unlike South Side, it is served by a dozen or so churches and a settlement house, which might be expected to strengthen local social ties.

As mentioned above, only part of North Ghetto is incorporated in the

city of Richmond. The other demographically similar but more populated subarea is under the jurisdiction of the county. Compared with South Side and the city area of North Ghetto, the county portion is very much the prototype of the defeated neighborhood—a community plagued historically with inadequate services and facilities despite local efforts to correct this condition, a reflection of the community's stepchild status in relation to city and local government. Police and fire protection, provided by the county, are unsatisfactory. The county area, numbering around 3,300 residents, is regularly patrolled by a single patrol car from the sheriff's office, and for other services citizens must rely on county facilities 17 miles away. Lacking their own fire department, residents of the unincorporated area of North Ghetto depend upon fire trucks from a neighboring town, and these have to cross six sets of railroad tracks. Facilities for children are also inadequate. Since the county provides no recreational services for the area, children must depend upon the facilities at Shields Playground, a park located within the city sector of North Ghetto.

These and other services and facilities, so badly needed by residents of the county area, cannot be provided from its low tax base. Consequently, annexation to the city of Richmond has been viewed by many residents as the only means of overcoming the community's stepchild status. However, many who own property in the county sector, especially the greenhouse operators and land speculators who own most of the surrounding farmland, have consistently and successfully blocked plans to incorporate the county sector of North Ghetto, contending that annexation would raise their property taxes without benefiting them. A move to annex the county area in 1965, for example, failed when owners of over half the area's assessed property vetoed the measure. Also opposing annexation are those who engage in illicit activities such as gambling, prostitution, and drug dealing. This marginal element benefits the most from the lax law enforcement which has given the county area its reputation as a "no man's land." (For a more detailed treatment of the annexation issue, see Luke [1954] and Wenkert [1971, pp. 63–103]).

We recall that these neighborhoods and subneighborhoods were specifically selected in order to empirically assess the relative validity of the sociopolitical and reference-group arguments. Compatible with the sociopolitical thesis is the expectation of higher anomie in North Ghetto than in South Side. Wilson's reference-group viewpoint, by contrast, predicts the exact opposite. Also contrary to Wilson's thesis is the expectation derived from the sociopolitical argument that residents of North Ghetto's county sector, the area most resembling a "defeated" neighborhood, should exhibit significantly higher anomie scores than their counterparts from the less isolated incorporated portion of the North Ghetto community.

The Adult Survey

Pertinent data are derived from a secondary analysis of survey data
lected in 1965 by the Survey Research Center in Berkeley, Califoı
A weighted sample of 684 adults was drawn—the product of a multis
stratified probability sample of occupied households in Western Co
Costa County, California. Of the 326 black respondents, 191 were f
South Side and 107 from North Ghetto. Among North Ghetto's black
spondents, 37 lived in the incorporated area while 70 resided in the laı
more populated county sector of the community (for a detailed des
tion of the sampling procedures, see Wilson [1966]).

A standardized interview format was administered to all respond
Whereas some items required standardized answers in predetermined c
gories, others were open-ended. Interviewers were matched with res
dents according to race and sex. The present study focuses exclusi
on the black respondents from South Side and North Ghetto.

Dependent Variables

As previously indicated, Wilson (1971) failed to theoretically define
mie apart from his operational definition (Srole's anomia scale). Wi
the context of his article, the term "anomie" appears to function
synonym for a subjective sense of despair and hopelessness (see Wi
1971, p. 66). In contrast, the old Chicago School perspective on com
nity stability and change, from which much of Wilson's theoretical
sition is derived, evokes a more Durkheimian definition of anomie, r
ring to a state of normlessness in which the rules of the dominant so
(particularly those of its legal system) are "no longer binding or
in an area or for a population subgroup" (Lander 1954, p. 89; see
Wirth 1938; Shaw and McKay 1942; cf. Durkheim 1947, 1951). To in
greater precision in defining dependent variables, I will introduce sepa
measures of despair and normlessness (to be interpreted as beliefs a
the extent of normlessness in the wider society).

Despair.—Since Wilson employed Srole's (1956) five-item anomia
to measure anomie, I wanted to construct an index comparable to Sr
(1956). While not exactly equivalent, similar items were available
the 1965 Richmond adult survey. For a comparative check on the in
changeability of Srole's scale and the rough approximation of it use
this study, the Likert-type items from each scale are reproduced b
Following Dean (1961) and Fischer (1972), the items employed he
a proxy for Srole's were used to measure personal feelings of despair
hopelessness. The items were scored with the hopeful response low,
despair response high, and the others intermediate. The percentag
despair responses ranged from 29% to 70%, an average of 42%.

five items were summated according to Likert procedures to create an index. Interitem correlations produced a reliability coefficient (Cronbach's α) of .4421.

Srole Anomia Scale	Approximation of Srole Anomia Scale
There's little use writing to public officials because often they aren't really interested in the problems of the average man.	Most people in government are not really interested in the problems of families like mine.
Nowadays a person has to live pretty much for today and let tomorrow take care of itself.	A person should live for today and let tomorrow take care of itself.
In spite of what some people say, the lot of the average man is getting worse, not better.	In our country opportunities for success are available to everyone.
It's hardly fair to bring children into the world the way things look for the future.	You should not expect too much out of life.
These days a person doesn't really know whom he can count on.	If you don't watch yourself people will try to take advantage of you.

Perceived normlessness.—Paralleling Lander (1954), an anomie-type index was constructed which could indicate the absence of legal norms in society, the presence of contradictory norms, or the belief that the moral order is too weak to warrant trusting other people. After an item analysis, the following six items were selected:

1. I often have trouble deciding which are the right rules to follow.
2. It is alright to get around the law if you can get away with it.
3. Suckers deserve to be taken advantage of.
4. Everyone in this world is out for himself.
5. If you don't watch yourself people will try to take advantage of you.
6. Most people don't care what happens to you.

The items were scored with the social trust response low, the societal normlessness response high, and the others intermediate. Normless responses ranged from 11% to 75%, an average of 35%. The items were combined as in the previous scale, Cronbach's $\alpha = .5191$. Responses to the scale are interpreted as beliefs about the extent of normlessness in the wider society[7] rather than as symptoms of personal normlessness or con-

[7] The following passage from Merton (1964) captures the meaning of "anomie" or "normlessness" intended in the remainder of this paper: "In a word, the degree of anomie in a social system is indicated by the extent to which there is a lack of consensus on norms judged to be legitimate, with its attendant uncertainty and insecurity in social relations. For if norms are not shared, then one cannot know what to expect of the other, and this is a social condition admirably suited for producing insecure re-

fusion (see Form [1975] for a similar interpretation of anomie scales).[8]

Independent Variables

The primary independent or treatment variable is neighborhood, South Side and North Ghetto. The following three classes of covariates, similar to those utilized in Wilson's study, are introduced as controls: (1) socioeconomic status (as measured by family occupational status and by the respondent's level of education); (2) social integration (as measured by membership in voluntary associations, contacts with neighbors, and home ownership); and (3) miscellaneous social factors (age, sex, and size of household).[9] Sex and home ownership are obvious dichotomies. Family occupational status is divided into lower class, skilled labor, and managerial-professional. Age, size of household, education, number of voluntary associations, and amount of neighborliness are treated as interval

lations with others" (1964, p. 227). The purpose of the perceived normlessness index was to estimate the degree to which the anomie or normless condition of certain black residential areas might affect a resident's perception of the amount of anomie or normlessness in the larger social system. In contrast to the concept of "normlessness," which refers to a property of a social system, the concept of "perceived normlessness," as employed in this paper, refers specifically to the individual's observation or perception of anomie or normlessness in the wider society.

[8] A reader of an earlier draft of this paper has warned me that the last item of the despair scale ("If you don't watch yourself people will try to take advantage of you") is identical to item 5 in the perceived normlessness scale, "undoubtedly causing a built-in correlation between the two." This type of measurement decision could cause a problem in a study which conceptualized "despair" and "perceived normlessness" as logically distinct variables, because the very way in which the two variables were operationalized might produce an association between them which could then be erroneously interpreted as a causal relationship. By way of a mild rejoinder, I should point out that in the present study I had no intention of treating "despair" and "perceived normlessness" as mutually exclusive concepts. Rather, I view the perception of the wider society as normless as one important aspect of "subjective despair." In addition, we must recall the reason I constructed the perceived normlessness scale was to provide a measure of societal normlessness that was more valid than Srole's measure. The "social distrust" item was the only appropriate item in the Srole-type scale to measure anomie in the Durkheimian sense of societal normlessness. Since, in my judgment, the "social distrust" item was a good single measure of "perceived normlessness," I decided to include it in both scales. (My "social distrust" item was also the best available substitute for Srole's measure of social isolation). Considering my intentions, I cannot really see how the so-called built-in correlation between the two scales can be construed as a methodological shortcoming of this study.

[9] Wilson (1971) employed length of residence in neighborhood as a measure of social integration. Unfortunately, the adult survey upon which my study is based did not ask subjects about length of time spent in area of residence. Another difference between the two studies is reflected in Wilson's decision, following Gordon's (1967, pp. 938–39) suggestion, to treat home ownership as an indicator of socioeconomic status rather than as a social-integration measure (but see Wilson [1971, p. 83]). That home ownership may also signify entrenchment in an area seemed, however, more compatible with the available historical information on North Ghetto and South Side.

scales. Associational membership was computed as the total number of organizations belonged to by an individual. The neighborliness index was constructed from responses to the following three moderately intercorrelated items: (1) About how many of the people in this neighborhood do you know by name? (2) My friends all live in the neighborhood; and (3) Do you know anyone in this neighborhood who keeps up with things and whom you can trust to let you know what is *really* going on—someone whose opinions you pay special attention to? A respondent received one point for knowing the names of fewer than 16 neighbors, two points for knowing between 16 and 50 neighbors, and three points for knowing over 50 neighbors. A respondent received one point for disagreeing with the statement, "My friends all live in the neighborhood," two points if undecided, and three points for agreeing. A respondent received one point for a negative response to the local opinion leader item and three points for a positive response. Scores on the neighborliness scale could thus vary from 3 to 9.

Method of Analysis

Following the logic of contextual and covariance analysis, a multiple regression analysis was implemented to estimate the indirect and direct effects of area of residence on despair and perceived normlessness, to assess the relative effects of the independent variables on the dependent ones, and to detect different slopes within neighborhoods and subneighborhoods. Statistical analyses were performed with the SPSS (Nie et al. 1975).

RESULTS

North Ghetto versus South Side

Despair.—Table 2 presents the mean despair scores for blacks by neighborhood from both Wilson's research on the East Coast and mine on the West Coast. In the West Coast study, black scores are significantly higher in North Ghetto than in the more transitional South Side area. Therefore, Wilson's discovery of lower despair in the racially stable ghetto is not repeated in my study. More compatible with Wilson's analysis is the finding that neighborhood explains a significant 2% of the variance, implying the importance of neighborhood as a source of despair.

An adequate test of the Wilsonian perspective, however, must also take into account antecedent variables, such as family occupational status, education, age, household size, and sex, which might be causing the relationship between neighborhood type and despair. That is, one might reasonably attribute our preliminary results to differences in the types of indi-

TABLE 2

MEAN DESPAIR SCORES AMONG BLACK ADULTS OF FIVE INNER-CITY NEIGHBORHOODS*

Neighborhood	Mean	SD	N
	Wilson's Study (F = 13.60; P < .001)†		
Ghetto (96% black)............	8.7	2.8	203
Northeast (85% black).	9.8	3.2	148
West Side (63% black).	10.4	3.2	132
	West Coast Study (F = 5.03; P < .05)‡		
North Ghetto (96% black).......	10.58	2.52	95
South Side (80% black)........	9.82	2.51	171

* In both studies the range of the despair measure varies from 5 to 15.
† These results from Wilson are derived from his table 2 (1971, p. 74).
‡ The zero-order variance is 2%.

viduals who settle in racially stable and racially unstable areas rather than to differences in neighborhood-level characteristics such as rate of racial change. According to this "compositional" argument, what appears as a neighborhood effect in the present study could actually be an artifact of not having adequately controlled for neighborhood differences in the background characteristics of local residents.

An examination of the intercorrelations between despair, neighborhood, and the antecedent variables uncovered two patterns in the data which together strongly suggest that the zero-order effect of neighborhood on despair is largely artifactual: (1) the correlations between despair and the antecedent variables were weak to moderate, with the two SES measures (level of education and family occupational status) producing the strongest zero-order correlation coefficients (—.271 and —.238, respectively); and (2) a greater concentration of variable attributes conducive to despair was found in North Ghetto than South Side (data not presented).

Table 3 reports the results of a regression analysis used to interpret the relationship between neighborhood and despair. In row 1, β is equivalent to the difference in mean despair scores between North Ghetto and South Side, while in the subsequent rows the β's are equivalent to the respective differences in mean despair scores after adjustment for the variables indicated as controls. After adjusting the data for the antecedent test factors (specifically, the miscellaneous social factors and SES variables described above), we observe, as expected, that the β coefficient estimate of the causal effect of neighborhood is substantially weaker than the zero-order estimate and is statistically not significant.

Other data presented in table 3 support the community stabilization model (see fig. 1) representing social integration as an intervening vari-

TABLE 3

REGRESSION ESTIMATES OF EFFECT OF NEIGHBORHOOD ON DESPAIR BEFORE AND AFTER CONTROLLING FOR MISCELLANEOUS SOCIAL FACTORS, SOCIOECONOMIC STATUS, AND SOCIAL INTEGRATION (Black Adults Only)

Classes of Control Variables	β[a]	Variance[b]
No controls	.749*	.020*
Simultaneous controls for miscellaneous social factors and socioeconomic status	.270	.002
Simultaneous controls for miscellaneous social factors, socioeconomic status, and social integration	.010	.000

[a] The β coefficient is the unstandardized regression coefficient. The regression coefficients reported here are equal to the difference in despair means between North Ghetto and South Side before and after adjustment for the variables indicated as controls.

[b] Proportion of total variance accounted for by neighborhood before and after adjustment for the variables indicated as controls.

* $P < .05$.

able explaining the nonspurious or causal aspect of the relationship between neighborhood and despair. After adding the social integration factors to the regression equation containing the antecedent test variables, β drops nearly to zero. The increment to multiple R^2 reported in column 2, row 3 also supports the model: When all test factors are entered in the regression equation, neighborhood contributes nothing to the total variance.[10]

Perceived normlessness.—Following a line of reasoning first suggested by Shaw and McKay (1942), Lander (1954) argues that residents of racially homogeneous areas are less exposed to conflicting norms and societal disorganization, implying that residents of racially stable black

[10] In Wilson's (1971) East Coast study, community stability factors accounted for about as much of the total variance in anomia scores as the traditional SES variables. With respect to the relative importance of SES and community-stability variables, my findings (not previously reported) are similar to Wilson's. The combination of neighborhood and social-integration factors (including home ownership, voluntary association membership, and neighborliness) explains 4.4% of the variance in despair net of the miscellaneous social factors and SES variables (or nearly 30% of the explained variance). Nearly the same increment to multiple R^2 is accounted for by the standard SES factors. (All the independent variables together accounted for approximately 15% of the variance in despair scores). Inspecting each neighborhood separately reveals a similar pattern. Contrary to the standard deprivation arguments (e.g., Merton 1968), which conceptualize anomia largely as a consequence of blocked access to success goals, these results suggest that when predicting black scores on an anomia scale similar to Srole's (1956), community-stability factors have a statistical importance approaching that of the traditional SES variables.

ghetto neighborhoods would observe the least anomie.[11] Table 4 presents the mean perceived normlessness scores for blacks by neighborhood. Contrary to Lander's hypothesis, scores are substantially higher in North Ghetto than in South Side. Unlike the pattern of zero-order correlations produced with the despair measure, nearly all the correlations between perceived normlessness and the SES and miscellaneous social factor variables are either weak or negligible. (Only the correlation between education and perceived normlessness approaches .20.) Not surprisingly, then, the regression estimates in lines 2 and 3 of table 5 show that the neighborhood effect produced when the SES and miscellaneous social factor variables are statistically controlled is only slightly lower than the zero-order effect, suggesting that the neighborhood contextual variable is indeed a cause of perceived normlessness. By contrast, the zero-order effect of neighborhood on despair was found to be largely spurious.

More in line with our despair findings is the pattern of intercorrelations

TABLE 4

MEAN PERCEIVED NORMLESSNESS SCORES[a] AMONG ADULTS IN TWO INNER-CITY NEIGHBORHOODS[*]

Neighborhood	Mean	SD	N
North Ghetto (96% black)	11.46	2.85	94
South Side (80% black)	10.39	2.41	180

[a] The range of the perceived normlessness index varies from 6 to 18.
[*] $F = 9.69$; $P < .01$; zero-order variance is 3.8%.

[11] Whereas both Wilson (1971) and I use rate of black population change as a measure of community stability, Lander (1954) employs percentage of blacks as an indicator of community stability. Lander found an increasing official delinquency rate for both blacks and whites as the proportion of blacks in an area approached 50%, but as the percentage exceeded 50% the delinquency rate markedly declined, reaching its lowest rate in the most solidly black areas. This pattern suggested to Lander that cultural conflict and social tension were greatest when a population approached maximal racial heterogeneity. "Competing and conflicting moral values," he reasoned, were more likely to develop in racially mixed areas, producing extreme social instability and anomie in the local population. Reinterpreting Lander's findings, Wilson (1971) argues that areas reaching maximal racial heterogeneity "are usually passing through the most critical phase of the invasion-succession cycle; the constant uprooting and resettling has a disruptive effect on neighborhood organization bringing about increased anomie" (p. 67). Thus, contrary to Lander, who uses "racial heterogeneity" as an indicator of intense racial or cultural conflict, Wilson views it as an indicator of intense racial succession. Another difference between Wilson and Lander is that Wilson focuses upon the disruption of neighborhood institutions resulting from rapid population turnover rather than interracial conflict as a major source of anomie in areas shifting from white to black. Conversely, he characterizes the older established black ghetto as a "relatively stable social system" which helps integrate social ties, suggesting that anomie should be relatively low in such an area.

TABLE 5

REGRESSION ESTIMATES OF EFFECT OF NEIGHBORHOOD ON PERCEIVED NORMLESSNESS BEFORE AND AFTER CONTROLLING FOR MISCELLANEOUS SOCIAL FACTORS, SOCIOECONOMIC STATUS, AND SOCIAL INTEGRATION (Black Adults Only)

Classes of Control Variables	β[a]	Variance[b]
No controls	1.058**	.038**
Simultaneous controls for miscellaneous social factors and socioeconomic status	.805*	.020*
Simultaneous controls for miscellaneous social factors, socioeconomic status, and social integration	.414	.005

[a] The β coefficient is the unstandardized regression coefficient. The regression coefficients reported here are equal to the difference in perceived normlessness means between North Ghetto and South Side before and after adjustment for the variables indicated as controls.

[b] Proportion of total variance accounted for by neighborhood before and after adjustment for the variables indicated as controls.

* $P < .05$.

** $P < .01$.

between perceived normlessness, neighborhood, and the social-integration factors, which indicates that the causal impact of neighborhood on perceived normlessness is mediated by the social-integration factors. Of the three measures of social integration, the direct path (as measured by the standardized partial regression coefficient) from home ownership to perceived normlessness is the strongest, with scores significantly lower among homeowners ($\beta = -.19$, $P < .01$). Unexpectedly, blacks are more likely to view society as less integrated, that is, to score high on perceived normlessness, when their involvement in informal neighboring is high ($\beta = .14$, $P < .05$). Although in the expected direction, the relationship between membership in voluntary associations and perceived normlessness is weak and statistically not significant ($\beta = -.09$, N.S.). Inspecting the relationship between neighborhood and the social-integration measures shows informal neighboring and tenant status to be considerably more characteristic of residents from North Ghetto than South Side, with membership in voluntary associations only slightly more prevalent among the former than latter.[12] In view of the direction and strength of the social-integration items in relation to neighborhood and perceived normlessness, I expected that controlling for them would cause the neighborhood–perceived normlessness relationship to sharply decline. As table 5 indicates, after introducing these controls the neighborhood effect becomes noticeably weaker,

[12] The zero-order correlations (r's) between neighborhood and informal neighboring, home ownership, and organizational membership are .21, —.20, and .06, respectively.

indicating that the social-integration factors partly mediate the relationship between neighborhood and perceived normlessness.[13]

At this point we review the preliminary results:

1. Contrary to Wilson (1971) and Lander (1954), the stable isolated black ghetto district under examination (North Ghetto) manifested higher rates of despair and perceived normlessness than the unstable area (South Side). These findings are compatible with the sociopolitical viewpoint.

2. The result of adjusting the data for possible spurious or noncausal relationships was that the regression of despair on neighborhood did not approach substantive or statistical significance, while with perceived normlessness, the neighborhood effect (before controlling for the mediating social-integration factors) remained significant, explaining an additional 2% of the total variance ($P < .05$). The results derived from these more rigorous neighborhood comparisons, then, only partially support the sociopolitical argument since, according to that viewpoint, both the rate of despair and of perceived normlessness should be higher in stable isolated black ghetto neighborhoods.

3. The social-integration measures functioned as intervening test variables between neighborhood and the two anomie-type scales; however, unexpectedly, the pattern of intercorrelations between the social-integration factors, neighborhood, and the two anomie scales differed from that implied in standard community stabilization interpretations of anomie. For example, in the present study, those who scored high on neighborliness scored significantly higher on perceived normlessness than those who scored low on the neighborliness measure (see fig. 1; compare Wilson 1971, p. 81).

Overall, then, the preliminary results are more compatible with the sociopolitical viewpoint than with either Wilson's (1971) reference-group argument or the more standard community stabilization interpretation which Wilson had endorsed at the outset of his research.

[13] With respect to the relative importance of SES and community-stability variables in predicting perceived normlessness scores, I found that whereas the traditional SES factors account for only 22% of the explained variance, the proportion explained by the community-stability factors (including home ownership, voluntary associations, neighborliness, and neighborhood) was nearly 60%. (At the level of aggregation above the neighborhood, the nine independent variables accounted for about 13% of the variance in perceived normlessness scores). Within each neighborhood, the social-integration factors were more important as predictors of perceived normlessness than the standard SES measures of occupational status and educational attainment, with the differences most pronounced in North Ghetto. Again the results compare favorably with Wilson's contention that as predictors of anomie, social-integration factors are, at the very least, as important as SES factors.

Subarea Comparisons within North Ghetto

To test more directly the relative validity of reference-group and sociopolitical arguments, I compared despair scores in the two subareas of North Ghetto. One recalls that in contrast to the city area, the county sector of North Ghetto is a more isolated community which exhibits many of the characteristics of a defeated neighborhood—for example, inadequate fire and police protection—symptoms of the community's third-class status in relation to local government.

Compatible with Wilson's reference-group perspective is survey evidence suggesting that county residents are less committed to a middle-class lifestyle and its emphasis on the value of hard work. Blacks from the county area are somewhat more likely to agree with the statement, "An easy life is a happy life," than those from the city area ($r = .18$, $P < .10$). Recalling Wilson's argument, only if a ghetto is sufficiently isolated from the dominant value orientations of the larger society will a black subculture evolve where success is redefined in a manner conducive to reducing despair-producing strain. Yet, contrary to this line of reasoning, despair is actually slightly higher in the institutionally more isolated county area (partial slope $\beta = .62$, N.S., the net variance is 1.3% of the total variance or 6.4% of the explained variance). Furthermore, county residents favoring the "easy life" score dramatically higher on despair ($r = .46$, $P < .001$), as well as on other indicators of psychological malaise (e.g., self-hatred), than those committed to hard work. Hence, these results are more compatible with the control assumptions of the sociopolitical interpretation, which attributes feelings of despair to a weakening of societal ties, than with the strain and subcultural postulates underlying Wilson's.

Also compatible with the sociopolitical viewpoint is the expectation that residents of defeated black neighborhoods should be more likely than residents of less stigmatized areas to view the wider society as normless. In neighborhoods lacking adequate law enforcement, where residents are regularly exposed to criminal activities and exploitative interpersonal relationships, one might expect residents to be more inclined than residents of better-policed areas to view the wider society, if not all human societies, as a jungle, that is, as approaching the state of normlessness.

Evidence from my own fieldwork suggests that illicit transactions and exploitative relationships are more widespread and visible in North Ghetto's county than its city sector. Along the major streets of the county neighborhood, we find a profusion of small shops and service establishments catering to a masculine clientele—liquor stores and taverns, carry-out food stores, pool halls, and service stations. Here, streetcorner men congregate at all hours to tell stories, shoot craps, have a drink, look for excitement, and hustle (compare Liebow 1967). As indicated earlier, the area rightly

has the reputation of a "no man's land" where lawlessness and crime go unchecked, thus making it especially attractive to criminals and action seekers from outside the area. In contrast to the county sector, the better-policed city section of North Ghetto lacks the institutional underpinnings conducive to an intensive street life and hence is less likely to attract criminals from outside the community. In accordance with the sociopolitical argument, which stresses the strength of linkages between neighborhoods and local governments, I expected perceived societal normlessness scores to be substantially higher in North Ghetto's county sector than city sector.

Table 6 reports the difference in perceived societal normlessness means between North Ghetto's county and city subareas. Again the survey data support the sociopolitical interpretation: County residents of North Ghetto are significantly more likely than their city counterparts to perceive the wider society as normless ([partial slope $\beta = 1.63$, $P < .05$], the net variance is 7.2% of the total variance or 33% of the explained variance).

In sum, the pattern of subarea findings thus far reported is compatible with the study's sociopolitical interpretation of black neighborhood subcultural diversity.

Also supportive of the sociopolitical viewpoint are previously unreported differences in subarea slopes within North Ghetto. For example, the correlation between despair and neighboring is .28 in the county sector and —.32 in the city area. (The difference in partial slopes .37 versus —.62 is significant at $P < .01$.) The interaction of the subarea variable

TABLE 6

REGRESSION ESTIMATES OF EFFECT OF NORTH GHETTO SUBNEIGHBORHOOD VARIABLE ON PERCEIVED NORMLESSNESS BEFORE AND AFTER CONTROLLING FOR MISCELLANEOUS SOCIAL FACTORS, SOCIOECONOMIC STATUS, AND SOCIAL INTEGRATION (North Ghetto Black Adults Only)

Classes of Control Variables	β[a]	Variance[b]
No controls...	1.532*	.066*
Simultaneous controls for miscellaneous social factors and socioeconomic status	1.605*	070*
Simultaneous controls for miscellaneous social factors, socioeconomic status, and social integration	1.627*	.072*

[a] The β coefficient is the unstandardized regression coefficient. The regression coefficients reported here are equal to the difference in perceived normlessness means between the county and city sectors of North Ghetto before and after adjustment for the variables indicated as controls.

[b] Proportion of total variance accounted for by North Ghetto subneighborhood variable before and after adjustment for the variables indicated as controls.

* $P < .05$.

with neighborliness explains 19.3% of the variance net of the combined additive effects of the subarea variable and all the previously described covariates (or 48% of the explained variance). Regressing perceived normlessness on neighborliness produces a similar, although less dramatic, outcome. The positive association of neighborliness with despair and perceived normlessness in the underpoliced and relatively isolated county section of North Ghetto could reflect more generally the fear of exploitative relationships felt especially among those coresidents of defeated black ghetto neighborhoods who through frequent contacts with criminals and streetcorner blacks are made aware of the fragile quality of interpersonal relationships in their ghetto world (see also Liebow 1967, chap. 7). This interpretation assumes that those with relatively weak local ties are less exposed to or entangled in the type of exploitative relationships conducive to viewing the wider society as normless and to personal feelings of despair.[14] By contrast, the negative association of neighborliness with despair and perceived normlessness in North Ghetto's better-policed and less isolated city district may simply mirror the usual correspondence that one would expect between associational patterns and psychological disposition (see Fischer 1973, p. 323).

These apparent subneighborhood differences in the very meaning of interpersonal relationships, then, also lend support to the thesis that the strength of linkages between neighborhoods and local government (e.g., as reflected in differences in the quality of local police protection) is a structural condition which when allowed to vary contributes to the creation *of subculturally distinct neighborhoods within* the black ghetto.

CONCLUSIONS

The findings reported in this study suggest that measures derived from census data, such as rate of racial turnover, are poor indicators of subcultural diversity within the black ghetto compared with sociopolitical variables which highlight the relationship between black residential areas and the structures of power and exploitation in the city. When stable North Ghetto and racially shifting South Side were treated as distinct residential areas, the causal or nonspurious effect of neighborhood on despair

[14] South Side residents actively engaged in neighboring also score significantly higher on perceived normlessness than those less involved ($B = .20$, $P < .05$), suggesting that residents of transitional areas who are initially fearful and distrustful of their neighbors attempt to reduce their anxiety by cultivating neighbors (see Suttles 1972, pp. 235–36, but compare Wilson 1971, pp. 81–82); that is, for newly arrived residents of transitional areas, intensive neighboring may partially reflect their perception of the neighborhood as normless. As the data from the racially stable county sector of North Ghetto suggest, time may not always mitigate these fears of exploitation, especially among residents of low-income districts where exploitative behavior is widespread.

was negligible, while on perceived normlessness the effect was only moderately strong. By contrast, once North Ghetto was viewed as consisting of two subculturally distinct neighborhoods, differentially linked to local governmental structures, neighborhood of residence had considerably more explanatory power, especially when accounting for scores on the perceived normlessness measure. Furthermore, with both dependent variables, the total net effect of the subarea and subarea-neighborliness variables was striking, nearly doubling the explained variance over the other variables.

To test the sociopolitical viewpoint adequately requires of course a far more comprehensive sampling of black ghetto neighborhoods than was attempted in this modest, but I hope suggestive, study. For example, with a sufficiently large sample of black neighborhoods, one could both measure the impact of neighborhood-city linkages on perceived normlessness, while partialling the effect of other neighborhood-level characteristics, and estimate precisely the relative statistical power of competing neighborhood contextual arguments. This formidable task remains to be done.

In conclusion, the results reported in this paper should alert social scientists to the possibility that the standard population variables, such as percentage of blacks or rate of racial turnover, may obscure much of what is subculturally distinctive about different neighborhoods within the black ghettos of our cities. In addition, they strongly suggest that a more structural approach to urban neighborhoods is needed before we parade any new generalizations about *the* black ghetto. In particular, we need to understand better the sociopolitical forces influencing the amount and quality of police protection in an area which may contribute to the emergence of "defeated" neighborhoods and other "no man's lands" within the urban community (see Suttles 1972). That such heavily stigmatized districts are more widespread in black than white residential areas may be partially a reflection of municipal policy regarding vice districts, which has always favored white over black residential areas. Spear's (1967, p. 25) description of how the Chicago police dealt with the problem of vice at the turn of the century is instructive:

> City authorities, holding that the suppression of prostitution was impossible, tried to confine it to certain well-defined areas where it could be closely watched. The police frequently moved the vice district so as to keep it away from commercial and white residential areas. Invariably they located it in or near the black belt, often in Negro residential neighborhoods. The chief of police declared that so long as prostitutes confined their activities to the district between Wentworth and Wabash, they would not be apprehended. Neighborhood stability, then, was threatened not only by the influx of Negro "shadies," but by the presence of an officially sanctioned vice district catering primarily to whites.

Of course, as my research suggests, not all ghetto neighborhoods become vice districts or "no man's lands." Future investigations should systematically explore to what degree discriminatory municipal policies actually contribute to producing a different distribution of such neighborhoods in black than in white areas. This type of structural approach to black residential areas, which focuses on the relationship between neighborhood subcultures and the structures of power in the city, could help rid the social sciences altogether of the monolithic conceptions of the black ghetto which have so persistently dominated both the sociological and popular imagination.[15]

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[15] For an important exception see Warren's (1975) recent research of black ghetto neighborhoods, which uncovered rich variation of neighborhoods within Detroit's black ghetto. He writes: "*The critical fact about the black ghetto as a whole is the diversity* of neighborhood milieux. In particular we have stressed the nature of internal variability. The black ghetto may appear as a monolithic pattern from the outside, but is in fact characterized by an essential duality: local neighborhoods which are often more uniform than similar white areas or which are frequently more mixed in terms of social levels and life styles and therefore less uniform than white neighborhoods" (1975, p. 69).

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Substantive Theory and Statistical Interaction: Five Models

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For a series of theoretical models involving two independent interval-scale variables acting together to produce a joint causal effect, different modes of analysis are described. In each case statistical interaction is involved, but the form of this interaction differs with each model. Methods for deriving appropriate interaction terms are given. These models provide a flexibility for statistical analyses appropriate to a broader range of theories than can be treated well by conventional regression procedures. If, as seems likely, theoretical propositions are often stated in empirical research in a form appropriate to additive multiple regression, these methods suggest that such propositions might be reexamined and reformulated. It is possible that the methods have particular relevance for some aspects of functional, equilibrium, balance, and conflict theories.

There are many theoretical models which require for the statement of testable propositions the use of more than one independent variable at a time. A complete statement of Merton's (1957, p. 140) deviance paradigm, for instance, requires the use of two independent variables simultaneously, that of "socialization into culture goals" and "access to institutionalized means." Smelser's (1962, p. 222) value-added paradigm for hostile outbursts similarly requires the simultaneous use of "prior strain" and "structural conduciveness" variables for stating a hypothesis predictive of outbursts. In both cases additive multiple regression, where each variable entered into the equation becomes the subject of a unique and independent hypothesis, is an inappropriate mode of analysis. As these theories postulate that the effects of certain variables are conditioned by the states of other variables and therefore involve consideration of the fuller setting, they are holistic in nature and require tests for interaction effects.

The propositions which predict the effects of variables in the simple additive multiple regression are singular in the sense that they are not contingent on the states of other variables. Those which predict statistical interaction are multiplar in the sense that, explicitly or implicitly, they necessarily predict simultaneously for more than one variable at a time.[1]

[1] Lazarsfeld (1958) uses the term "contingent" to describe interactive relationships. The term "multiplar" is deliberately introduced for two reasons. First, it is intended to qualify

Where two independent variables, X_1 and X_2, are postulated, the conventional interaction term for the two is their product, X_1X_2. The entry of this term into the analysis, however, may not be enough. Not only does it fail to deal adequately with all forms of multiplar propositions, but it also seems possible that there is essentially only one form for which it is completely appropriate.[2]

In order to develop the forms of interaction appropriate for the various types of multiplar propositions,[3] it is necessary to differentiate between the latter and to state them clearly enough to form the basis for mathematical derivations. Once this is done there should be two benefits. First, a greater variety of modes of analysis should become available, providing opportunities for the testing of more sophisticated propositions. Second, greater consciousness of this variety and of the models for which each is appropriate should lead to greater sensitivity in the initial statement of testable theoretical propositions. If different modes of analysis are known to be available, the tendency to state all hypotheses in a singular form appropriate to one mode of analysis, additive multiple regression, should be curtailed. It is often the case that reexamination of theory in the light of other modes of analysis leads to the conclusion that an alternative statement of the propositions is preferable.

To call the list of models presented here a typology would be pretentious. It is quite possible that the names adopted for the types will prove to be inadequate and that both new names and new models will emerge. Nevertheless, it will be seen as the analyses proceed that there is a progression of models and modes of analysis from the simpler to the more complex. In particular, each succeeding mode of analysis tends to contain within it many of the features of preceding modes, indicating a progress toward greater generality.

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propositions rather than relationships. Second, it is intended to refer to propositions which become clumsy if phrased in terms of contingent relationships and are more elegantly phrased as predictions of the effects of two or more variables acting systemically. Neither the relative excess model nor the two equilibrium models subsequently discussed are best expressed in terms of a relationship between two variables being contingent on the value of a third, though this is not impossible.

[2] It is assumed here that all variables are measured on nondichotomized interval or ratio scales. When independent variables are dichotomized, this product is the only possible term.

[3] The term "interaction" is used here in the conventional sense appropriate to multiple regression and not in the more general sense used by Goodman (1970, 1972) for contingency table analysis. In regression analysis a "dependent" variable is specified, and interaction is defined as occurring between "independent" variables in their effect on the dependent variable. In Goodman's analysis no dependent variable is specified, the effects of variables are defined as the frequencies to be found in the cells of the table, and a relationship between two variables is necessarily defined as an interaction between them.

The models identified and analyzed in this paper are as follows.

1. The relative excess model. This is well known, being typified by theories of relative deprivation, one version of status inconsistency theory (which has given rise to much controversy), absolute change in a variable, economic surplus, etc. While it is the simplest of the models I discuss, it is also the hardest to deal with. A different approach to the analysis is suggested, but the principal stress will be on the attention which should be paid to the model in attempts at ex post facto explanations of unexpected effects in tests of singular propositions.

2. The conduciveness model. Also well known, this model is dealt with by means of conventional multiplicative interaction terms and is used when variables reinforce each other or are conducive of each other's effects by acting as a medium of transmission, a resource, a gateway, etc. Our discussion will focus primarily on the limitations of the method in comparison with the other models and on an interpretation of the results.

3. The symmetric equilibrium model. While the model is in principle well known, predicting consequences when two variables are not at functionally equivalent levels, not in balance, or not in an equilibrious relationship with each other, methods for applying it are relatively crude. One large problem lies in determining just what constitutes "balance" in terms of two variables on two different scales. A method is derived which avoids this problem and which is distinct from the other methods, but identification problems exist.

4. The asymmetric equilibrium model. This model differs from the former in permitting the effects of the two different forms of disequilibrium ($X_1 > X_2$ and $X_1 < X_2$) to have different consequences. It is therefore more general and requires a very different form of statistical interaction which is difficult to estimate. An iterative method is devised for its solution.

5. The proportional excess model. This model is similar to the relative excess model but is stated proportionally. Not only does it pose problems in solution, it also is difficult to specify accurately how the model should be stated (i.e., which variable should form the base for the proportion). The model is therefore not developed.

In addition to these, curvilinearity is discussed, partly because it is analogous to interaction and may be confounded with it and partly because it is necessary for models 3 and 4 above. For each of the four principal models, examples using real data are given. Despite the complexity of the interactions, it is in principle possible in each case to express the whole effect by means of a single variable, the interaction term, though it differs from model to model. Whether this is meaningful will depend on the circumstances.

Finally, two examples will be cited from recently published analyses. In one case the existence of the models provides a framework for suggesting that both the substantive discussion and the analysis are inadequate, an

argument supported by reanalysis. In the other the substantive theory is adequate, but the method adopted is seen to be crude.

The theoretical models are described below along with methods which may be used for statistical analysis.[4] The models are ordered in general according to the terms which are involved in the analysis. Each model will be seen as tending to involve a set of terms which includes sets of terms used in earlier models but with some additions. The theoretical nature of the models cannot be said to increase similarly in complexity, though to some degree it does. The ordering is therefore mainly algebraic. It will also be found that the kinds of problems encountered with each model differ slightly, and the focus of discussion will therefore vary from model to model.

THE RELATIVE EXCESS MODEL AND SUBTRACTIVE INTERACTION

The clearest example of the relative excess model is perhaps relative deprivation. This suggests that some dependent variable, such as frustration and/or aggression, increases to the extent that the variable X_1 (wants) exceeds an equivalent value of variable X_2 (gets) and decreases to the extent that the reverse occurs. The statement of the proposition requires the use of two concepts, and the proposition is therefore multiplar. In mathematical notation it can be stated

$$\hat{Y} = a + b(c + X_1 - dX_2) , \qquad (1)$$

where b is the positive effect, X_1 is wants, X_2 is gets, and c and d are unknown constants applied to correct for the possibility that the scales of X_1 and X_2 are not expressed in comparable units or derived from comparable origins.

Another instance of this model is that of status discrepancy (Lenski 1954; Blalock 1966, 1967; Hope 1975) in one of its forms, which Hope terms "signed

[4] The models will be illustrated by empirical examples using real data and by diagrams using contour lines. Some examples of this kind of analysis are to be found in Schmid and McCannell (1955), Hoiberg and Cloyd (1971), Hawkes (1973), and Haaland (1975). The principal difference in the analyses proposed here will be that the space within which the contours are given is bivariate rather than geographic and that the surfaces delineated by the contours are generated by multiple regression using polynomials and interaction terms (cf. Hawkes 1973, 78:144). The multiple regression equation is used to predict values of a dependent variable over the bivariate space (X_1, X_2), and the surface obtained is three dimensional. The contours show this surface. A generalized analysis capable of producing a complex curved surface can be obtained by using as predictor variables powers of X_1 and X_2 up to the cube and all possible interaction terms between the six resultant variables. Such an analysis can be used for exploratory analysis where no specific model is postulated. Care must be taken to avoid multicollinearity in this and other analyses by some method such as that of orthogonal polynomials as described by Anderson and Bancroft (1952, pp. 207 ff.). The method, developed by Tchebysheff, is not designed for interaction terms but should be suitable as it is not essential that zero intercorrelations be obtained. It ensures, given certain assumptions, that the variable X^j is uncorrelated with all powers of X other than j.

inconsistency." But I intend to generalize beyond such specific examples here, because the phenomenon may be less rare than supposed and could even be distorting the interpretation of multiple regression equations intended to test singular propositions.

Equation (1) is here termed subtractive interaction. It defines a plane surface in the three-dimensional space ($\hat{Y}$, X_1, X_2) which can be restated thus:

$$\begin{aligned}\hat{Y} &= (a + bc) + bX_1 - bdX_2 \\ &= a_{0.12} + b_{01.2}X_1 + b_{02.1}X_2\end{aligned} \tag{2}$$

where $b_{01.2} = b$ and $b_{02.1} = -bd$.

Any two singular propositions which predicted a negative coefficient for X_2 and a positive one for X_1 would therefore fit the equation. Moreover, it is impossible to determine whether subtractive interaction or singular additive effects are operating by the usual method of control variables. Where the interaction term is

$$X_3 = (c + X_1 - dX_2) , \tag{3}$$

X_3 is merely an additive linear function of X_1 and X_2 (Blalock 1966, 1967; Namboodiri, Carter, and Blalock 1975, p. 178). Therefore, $R_{3(12)}$ is unity, and complete multicollinearity would occur in any regression of Y on X_1, X_2, and X_3. Moreover, the constants c and d are unknown, making X_3 undefinable. Blalock's solution involved the determination of the causal processes by which X_1, X_2, and X_3 effect changes in Y. If any intervening variables can be identified and measured, they can be placed in the analysis, preferably by means of a path analysis. If, for instance, X_4 is a variable intervening between X_3 and Y, and provided that $R_{4(12)}$ is not unity, a path diagram of the following form could be used (fig. 1). Intervening variables between X_1 and Y or X_2 and Y would be equally useful.

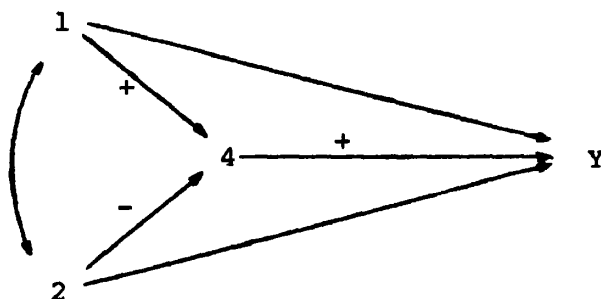



FIG. 1.—Path diagram illustrating use of variable X_4 intervening between difference term ($c + X_1 - dX_2$) and Y.

Use of Canonical Correlation

If a second dependent variable subject to parallel theoretical propositions is available, another method is possible. Suppose that (ignoring constants) the two predictions are of the following form, which assumes positive singular effects and relative excess effects:

$$\hat{Y}_1 = aX_1 + bX_2 + c(dX_1 - eX_2) \quad (4)$$

and

$$\hat{Y}_2 = fX_1 + gX_2 + h(dX_1 - eX_2)\,. \quad (5)$$

An approximation to this model is provided by figure 2, which is of a form amenable to canonical correlation. The approximation is contained in the lower canonical variate. The equations derivable from this figure are

$$\hat{Y}_1 = a'kX_1 + b'kX_2 + c(dX_1 - eX_2) \quad (6)$$

and

$$\hat{Y}_2 = a'gX_1 + b'gX_2 + h(dX_1 - eX_2)\,, \quad (7)$$

in which the ratios of the singular coefficients for X_1 and X_2 in the two equations, $a'k/b'k$ and $a'g/b'g$, are the same. In the original versions, (4) and (5) above, this is not necessarily the case. The procedure fits Hope's (1975) model, which substitutes for the two status variables, X_1 and X_2, two others. The first is a weighted sum of X_1 and X_2, indicating a general status. The second is a weighted difference of the two, indicating discrepancy of status. In Hope's model the weights used for the two new status variables should be the same whatever the variable treated as dependent on them, as the weights should depend entirely on characteristics of the two original statuses and not on any subsequent causal process. Canonical analysis conforms to this model and thus provides an alternative to the procedures summarized by Hope.

Van de Geer (1971, pp. 164 ff.), in a reanalysis of Duncan, Haller, and Portes's (1968) data, provides an example. They used data relating "parental encouragement to achievement," "family socioeconomic status," and "child's intelligence" to the child's occupational and educational aspirations. They also used the same three independent variables relating to the child's friend.

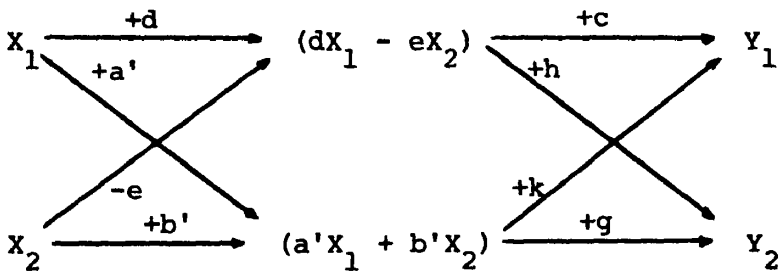



Fig. 2.—Causal model of difference and positive singular effects for two dependent variables.

Van de Geer's canonical equations therefore involve six independent and two dependent variables, yielding two canonical variates for each set. The resultant path diagram obtained by Van de Geer is shown in figure 3.

In this diagram Van de Geer interprets variate η_1 as a latent "aspiration" variable. All paths except one (possibly insignificant) indicate positive relationships to both types of aspiration. This is as would be predicted for a "generalized aspiration" variable. Van de Geer does not interpret η_2, but it is possible to interpret it in terms of subtractive interaction. Let us consider only the two variables "parental encouragement" and "family SES" for the respondent. Through canonical variate η_2 it will be seen that children from low-SES families who have high parental encouragement will tend to have high educational but low occupational aspirations. They appear to be focusing on the achievement-oriented means of mobility. Children from families of high SES but with low parental encouragement will tend to have high occupational aspirations but low educational aspirations and are focusing apparently on ascriptive status. The canonical variate η_2 can be interpreted as a "differentiated aspiration" variable. The variable χ_1 indicates consistency between structural position and socialization into goal orientations, while the variable χ_2 indicates inconsistency between these two.

Theoretically, we can interpret the relationship as one in which the higher the parental encouragement relative to the family's SES, the higher the child's educational aspirations relative to his occupational aspirations. On the other hand, the higher the family SES relative to parental encouragement, the higher the child's occupational aspirations relative to his educational aspirations.

It is then interesting to note, in figure 3, that the child's intelligence and his/her friend's intelligence appear to operate in ways equivalent to parental

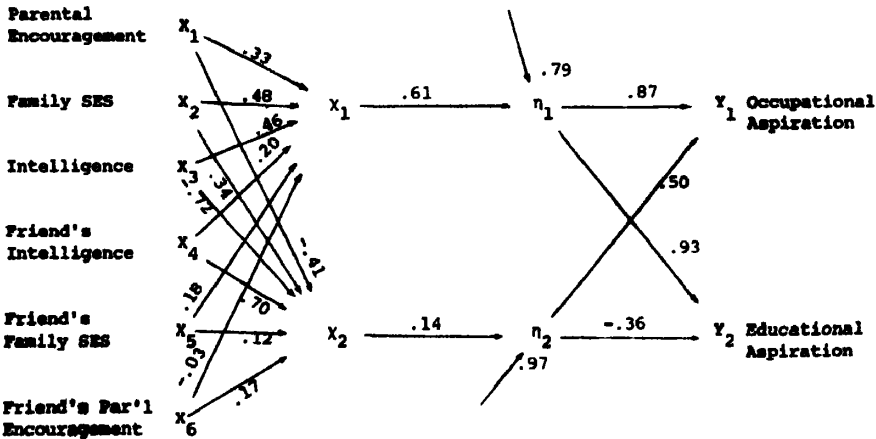



FIG. 3.—Canonical analysis diagram adapted from Van de Geer (1971, p. 164) illustrating difference effect through η_2.

encouragement and SES, respectively. It is difficult to interpret the second canonical variate in terms other than those used here, suggesting that the technique may have untangled a subtractive interaction effect.

A weakness of this form of analysis is that differences in the signs of the composite paths from the exogenous variables through the canonical variables are determined entirely by differences in the signs of the paths to the χ canonical variates. If two variables have paths of opposed signs to a given χ variable, then the signs of the composite paths to both the dependent Y variables are also opposed. If a subtractive interaction effect is predicted, it must therefore apply to both dependent variables, either in the manner illustrated by Van de Geer's example or in parallel, in which case the signs of the paths from the η variable to the Y variables will be the same. We can also postulate the case in which signs from the X's will be the same but those to the Y's different, suggesting that increasing values on the X's will tend to produce a change from high values on one of the Y's to high values on the other.

The example is very similar to that described by Hope (1975) in discriminating between Lenski's (1954) "vertical dimension" of status, a generalized status, and "signed status inconsistency" orthogonal to it. The canonical analysis appears to disentangle the two quite well, but, like the methods Hope describes, it does not control for the possibility that effects other than these, expressed by singular propositions, may exist and be producing spurious effects. Without the measurement of intervening variables as suggested by Blalock (1966), no test can be applied for this contingency. Hope argues that substantive parsimony may dictate a preference for a multiplar over a set of singular propositions. It must be added that spurious effects may occur in either direction. If not predicted, subtractive interaction effects may distort the interpretation of regression coefficients when attempts are made to explain unexpected results by means of singular propositions.

Some Implications

Let us assume that each of the two variables X_1 and X_2 has a singular positive effect on Y and that the difference $(c + X_1 - dX_2)$ also has an effect but that the latter has not been predicted. Adding these effects,

$$Y = a + b_1X_1 + b_2X_2 + b_3(c + X_1 - dX_2) \tag{8}$$

$$= (a + cb_3) + (b_1 + b_3)X_1 + (b_2 - db_3)X_2 \tag{9}$$

in which b_1, b_2, b_3, c, and d are all positive. A multiple regression on X_1 and X_2 as required by the two singular hypotheses will tend to produce strong confirmation for X_1 but may produce insignificant or even reversed results for X_2. If the reversed result for X_2 is statistically significant, an ex post facto singular explanation may be attempted if the possibility of subtractive

interaction is ignored. As almost no attention is paid to subtractive interaction, it is quite likely that the correct explanation will be entirely missed.

An instance is provided by an analysis by Kostroski and Fleisher (1976). In this case the dependent variable Y was the proportion of bills supported by the American Farm Bureau (AFB) for which a senator cast positive votes. The two independent variables were X_1, the percentage of the senator's electoral votes which came from agricultural areas, and X_2, the percentage of his state's personal income derived from farming. Singular propositions suggested that both of these should be positively related to the senator's vote. In fact, $b_{y1.2}$ was positive and highly significant while $b_{y2.1}$ was negative and highly significant, with t-coefficients of 5.31 and 4.11, respectively. No singular proposition could be advanced to explain $b_{y2.1}$, and, as r_{12} was .81, multicollinearity was suggested as the cause.

When the possibility of a relative excess model was considered, it was quickly seen to provide a plausible explanation. The multiplar proposition stated that the greater a senator's electoral support from his state's agricultural sector relative to the size of that sector, the greater the likelihood of his voting for AFB-supported bills. This would indicate that a senator in a state with a limited agricultural sector but whose support was highly concentrated in that sector would possibly have been elected as part of a conflict between the agricultural and nonagricultural sectors and would be strongly proagriculture. This led to the substitution for X_2 of the variable X_3, the proportion of the work force employed in agriculture, r_{13} being close to .60, reducing the danger of multicollinearity. The same result was achieved, though with slightly decreased levels of probability. The analysis was then repeated with all three variables substituted (Fleisher, oral communication). The dependent variable was now the senator's proportional vote for NAACP-supported bills, and the two independent variables were the percentage of his electoral vote from black areas and the percentage of state population that was black. The same result was obtained. Multicollinearity would predict random fluctuations in all coefficients. The mutual support of these analyses tended to support a relative excess model.

One of the difficulties of this type of theory, in fact, is that it will often be the case that consistency, that is, small differences, between the two independent variables will be expected. This suggests that relative excess theory will often refer to two independent variables which are highly correlated with each other. When unexpected relationships with the dependent variable are found, it should not be assumed too readily that multicollinearity is the explanation.

The implication is that theories predictive of subtractive interaction should be considered in all multiple regressions before they are performed and preferably before the measurement of the variables. If any such theory seems to have any validity, an attempt should be made to perform appropri-

ate analyses. Any surprising relationship discovered in multiple regression could be due to the failure to take this precaution and might be explained in this way.

THE CONDUCIVENESS MODEL AND SIMPLE MULTIPLICATIVE INTERACTION

The type of substantive theory for which simple multiplicative interaction is most appropriate appears to be illustrated most clearly by the effect of "structural conduciveness" in Smelser's (1962, pp. 241, 256) value-added paradigm for collective behavior. The concept of structural conduciveness is of much wider utility than for this paradigm only, but Smelser's model for hostile outbursts provides a useful vehicle. It states, briefly, that the degree to which hostile outbursts occur is dependent on the degree to which "strain" and "structural conduciveness" exist. One aspect of structural conduciveness is the accessibility of a target. If both variables are measured on ratio scales, the prediction will be that, when accessibility is zero, strain will not produce attacks on the target; and if strain is zero accessibility will not produce attacks. Nonzero values on both variables are required to produce a nonzero probability for hostile outbursts.

In this example "accessibility" is passive, furnishing merely the gateway through which the effect of strain passes. In other examples it may not be possible to specify so easily whether a variable is passive. Smelser suggests that intergroup hostility will facilitate attacks when strain occurs. In this case both conditions are active, and it is therefore conceivable that the theory could be stated the other way around, each variable being conducive of the effect of the other.[5] In general, the greater the degree to which the quality represented by one variable is present, the more it facilitates a specific monotonic effect of the other by providing resources, acting as a medium or gateway, reducing resistance, diverting or directing its influence, changing its form, etc.

Most simply this relationship can be expressed by hypothesizing that the linear relationship of a dependent variable, Y, with an independent variable, X_1, is itself a linear function of X_2.[6] This means that the slope expressing

[5] Lazarsfeld (1958) drew a different distinction according to whether the variable is antecedent or intervening between the other and the dependent variable. If the variable X_2 is antecedent, he termed it a "condition" for the resultant rate of increase of Y with X_1. If X_2 intervenes between X_1 and Y, he termed X_2 the "contingency." Following the same criteria, he termed the relationships between Y and X_1 "conditional" and "contingent" relationships, respectively.

[6] This is merely the simplest model. More complex models might specify that the relationship changes with X_2^2, $\log X_2$, or with $1/X_2$; or the relationship itself might be expressed in terms of $1/(X_1 + k)$, similarly changing with functions of X_2.

this relationship, or the partial derivative, is

$$\frac{\partial Y}{\partial X_1} = c + dX_2 , \tag{10}$$

which is derivable from the equation

$$Y = cX_1 + d(X_1X_2) + e . \tag{11}$$

Predictive equations of this form may be obtained by multiple regression in which the term X_1X_2 is a multiplicative interaction term with e, the ordinary least-squares residual.

From this it will be seen that the relationship of Y to X_2 is dependent on the state of X_1, and also that c is the rate of increase of Y with X_1 for the specific condition, $X_2 = 0$. Unless there is a strong reason for supposing that the relationship of Y to X_2 is 0 when X_1 is 0, it is necessary to use the three terms X_1, X_2, and $X_3 = X_1X_2$ in a multiple regression. This will be particularly the case when X_1 and/or X_2 are measured on interval rather than ratio scales, as the origin of the scale is in this case arbitrary.

Multiplicative interaction of this form always yields a contour pattern of the general form shown in figure 4. The origins of X_1 and X_2 are not shown because their positions will vary with different analyses. Any given analysis will also consist of only a section of the total pattern. If, for instance, X_1 and

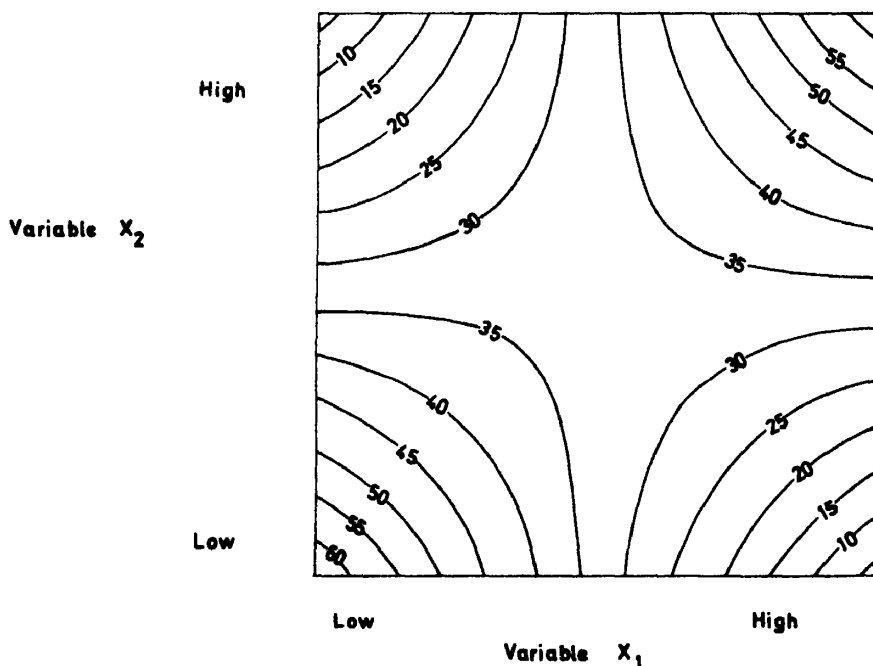



FIG. 4.—The general contour pattern produced by multiplicative interaction using the terms X_1, X_2, and X_1X_2 to predict Y.

X_2 are positive ratio-scale variables and each has a positive effect but no effect at all when the other has a value of zero, the analysis would indicate only the top right-hand quarter of the pattern. The center of the diagram consists of a "saddle." The latter is important as it constitutes a feature of the model which distinguishes it from the symmetric equilibrium model, which will be described later. In specific cases the saddle, of course, may run from high X_1/high X_2 to low X_1/low X_2 rather than along the other diagonal as shown here.

There are several implications which follow.[7] The first is that if an independent variable, X_1, is measured on an interval rather than a ratio scale no theoretical meaning can be attached to the partial regression coefficient for the other variable, X_2, in the regression equation.[8] Change of the origin of the scale of X_1, as would occur, for instance, if it were scored as deviations from the mean before obtaining the interaction term X_1X_2, could change the value of $b_{y2.13}$, $\beta_{y2.13}$, and $r_{y2.13}$.[9] No point would be served in such a case by testing the statistical significance of $r_{y2.13}$.[10]

The second is that such a test is meaningful if X_1 is measured on a ratio scale, but that it is a test only of the specific relationship of Y to X_2 when X_1 is meaningfully 0.[11] This is a theoretical matter. Smelser's paradigm makes such a specific prediction, provided that it is stated appropriately (in the example given, attacks on a specific target) and provided that ratio scales are available.

It can be shown that changes in the origins of X_1 and X_2 will change the values of at least some of the partial coefficients. The proof of this is a little

[7] From this point a number of arguments follow which are somewhat contrapuntal to those of Althauser (1971; all references to Althauser are to this work). In general, the two are parallel, but the contrasts are as marked as the similarities, and, in particular, the implications are very different. All of the relevant points will be specified by a series of footnotes.

[8] In the discussion which follows, it is assumed that all references to the values of coefficients are to those calculated from the data of a given sample. Variation between samples is not considered.

[9] Althauser's arguments center on such changes in origin. The notation here follows the convention that b signifies the unstandardized regression coefficient, β the standardized regression coefficient, and r the correlation coefficient.

[10] In analysis of variance a distinction is argued between interaction and "main" effects. It is answered above that this makes empirical sense only when variables are measured on ratio scales. In analysis of variance the categories of the qualitative variable may be coded as dummy variables for the equivalent multiple regression (Fennessey 1968). These are dichotomous and may be coded (0, 1). In this form they are a simple form of ratio scale in which scale value 0 = no such quality present. The identification of main effects may therefore be meaningful for analysis of variance but not necessarily for multiple regression when the independent variables are measured on interval scales. Althauser does not discuss the question of the values of the unstandardized regression and partial correlation coefficients, but it seems implicit that he is assuming they are not constant.

[11] Althauser does not consider this point.

tedious and is given in the Appendix. From this it can be seen that if the origins are changed by the simple addition of the constants c and k, giving $X'_1 = X_1 + c$ and $X'_2 = X_2 + k$, then the partial coefficients obtained by the multiple regression of Y on (X'_1, X'_2, X'_3), where $X'_3 = X'_1X'_2$, can be derived as follows (primes distinguish the coefficients obtained in the new regression):

$$b'_{y1.23} = b_{y1.23} - kb_{y3.12} \qquad b'_{y2.13} = b_{y2.13} - cb_{y3.12} \tag{12}$$

$$b'_{y3.12} = b_{y3.12} \tag{13}$$

$$\beta'_{y1.23} = \beta_{y1.23} - k\frac{s_1}{s_3}\beta_{y3.12} \qquad \beta'_{y2.13} = \beta_{y2.13} - c\frac{s_2}{s_3}\beta_{y3.12} \tag{14}$$

$$\beta'_{y3.12} = \frac{s'_3}{s_3}\beta_{y3.12} \tag{15}$$

$$r'_{y3.12} = r_{y3.12} \qquad R'_{y(123)} = R_{y(123)}\ . \tag{16}$$

The formulae for the remaining two partial correlation coefficients are not simple, but it is clear that they vary with changes in the origins of the other variable. The three-dimensional surface in space (Y, X_1, X_2) denoted by the multiple regression of Y on (X_1, X_2, X_3) is identical with that denoted by the multiple regression of Y on (X'_1, X'_2, X'_3) with the exception that the two are located differently relative to the origin. The two multiple correlation coefficients are the same.[12]

It can be seen that the partial correlation and regression coefficients for X_1 and X_2 are unstable, depending on the position of the origin of the other variable. The same is true of $\beta_{y3.12}$ but not of $r_{y3.12}$ or of $b_{y3.12}$, which are constant.[13] The significance test for the interaction term is stable and useful, but those for X_1 and X_2 must be interpreted with great caution. If no meaningful origin can be attached to the scales of X_1 and X_2, the value of $\beta_{y3.12}$ is without any specific meaning except in the special case to be noted below.[14]

[12] Althauser does not make this assumption; indeed, he makes the reverse, assuming that "R square for the non-additive model will be smaller . . ." (p. 466), that is, different when the origins of X_1 and X_2 differ.

[13] Althauser unfortunately did not pursue the matter of the unstandardized regression coefficient, which can be seen here to be the key to the problem.

[14] Althauser makes the same point but draws the inference that the method is therefore meaningless. Thus, "it would appear, in short, that including multiplicative terms in regression models is not an appropriate way of assessing the presence of interaction among our independent variables" (p. 466). This assumes that, if $\beta_{y3.12}$ is not constant with changes in origins of X_1 and X_2, then the appropriate F- or t-test also is not constant. This has been shown here to be false.

An implication is that it should be possible, by the use of appropriate constants in the change of origins of X_1 and X_2, to ensure that the partial coefficients for X_1 and X_2 are 0. This is proved in the Appendix, where it is shown that when

$$X'_1 = X_1 + \frac{b_{y2.13}}{b_{y3.12}} \qquad (17)$$

the partial regression and correlation coefficients for X_2 will be 0. And when

$$X'_2 = X_2 + \frac{b_{y1.23}}{b_{y3.12}} \qquad (18)$$

the partial coefficients for X_1 will be 0. In each case the partial correlation and unstandardized regression coefficients for the interaction term will remain the same. There is justification here for considering this model as indicating a single systemic effect rather than two effects each contingent on the other variable, as the whole effect can in fact be expressed and predicted by the interaction term alone under these conditions. Under these conditions also $\beta_{y3.12} = R_{y(123)}$.

It should be noted that the use of the interaction term X_3 will sometimes induce multicollinearity among the three variables.[15] Change of the origins of X_1 and X_2 will produce variations in this. Nevertheless, $r_{y3.12}$ remains constant. It is quite possible to manipulate the origins of X_1 and X_2 so as to reduce $R_{3(12)}$ to quite low values, but this will normally have no effect on the test for interaction, and the other two tests will be meaningless under these circumstances.[16] There will be occasions when singularity of the matrix is closely approached and no partials can be obtained. Changing the origins of X_1 and X_2 before obtaining X_3 will often avoid this, but no purpose is served in reducing $R_{3(12)}$ to a low value once absolute singularity has been avoided. Oddly enough, once this has been achieved, it is then possible to calculate the coefficients for the regression equation assuming the original origins. But even if the latter are meaningful, we cannot conclude that $b_{y1.23}$ and $b_{y2.13}$ are significant as the two partial correlations are zero under these conditions. We can therefore draw the following conclusions in any analysis involving the multiple regression of Y on X_1, X_2, and $X_3 = X_1X_2$.

[15] Althauser's chapter is couched in these terms. Strictly speaking, this is not correct as the instability in the three beta weights is a function of the origins of X_1 and X_2 and not of fluctuations between samples produced by large standard errors.

[16] Althauser is more pessimistic: "Both the depression of b_3 [i.e., $\beta_{y3.12}$] and these possible fluctuations of estimates [i.e., of $\beta_{y1.23}$ and $\beta_{y2.13}$] pose severe problems for the substantive interpretation of the regression estimates in such models as these. Substantive interpretation of these betas seems as fickle when estimates so depend on the means of the individual variables as when, as for Gordon, small differences between correlations create striking differences in estimates" (p. 466). The arguments above show that the beta referred to is considerably more fickle than the partial correlation coefficient. Care should be taken, though, to ensure that $R_{3(12)}$ is not too high, as this may introduce undue rounding errors into computer calculations.

1. The values of all three betas, and of $r_{y1.23}$, $b_{y1.23}$, $r_{y2.13}$, and $b_{y2.13}$, will vary according to the origins of the scales of X_1 and X_2. If these two variables are measured on interval rather than ratio scales, these coefficients are therefore arbitrary. The t-tests for X_1 and X_2 are therefore also arbitrary.

2. The values of $b_{y3.12}$, $r_{y3.12}$, and $R_{y(123)}$ do not vary with changes in the origins of X_1 and X_2, nor does t for the significance test for X_3.

3. It is possible, by changing the origins of X_1 and X_2 by the specific constants previously given in equations (17) and (18), to obtain a regression equation in which the causal effect is entirely expressed by the single interaction term, all coefficients for X_1 and X_2 being 0.

4. The following significance tests are meaningful, provided that singularity has been avoided: (*a*) that of $r_{y3.12}$ always determines whether significant interaction is occurring; (*b*) those of $r_{y1.23}$ and $r_{y2.13}$, *if* X_2 and X_1, respectively, are scored on ratio scales and there are specific predictions for each coefficient when the other variable has value zero; and (*c*) that of $R_{y(123)}$.

5. It is always possible, after performing the initial multiple regression, to determine the value on the scale for either variable at which the other variable has no effect. This may be of substantive import in some cases.

6. It is also possible to determine the range of the effect (b-coefficient) of X_1 over the range within which cases are distributed on the scale of X_2, and vice versa. This is not provided by the regression equation but may be deduced from it by substituting values on the control variable at the extremes of its range.

7. It follows that, even when X_1 and X_2 are measured on ratio scales, $b_{y1.23}$ and $b_{y2.13}$ may refer to relationships which are well outside the ranges within which cases are distributed on the two variables. This will occur when no cases have values anywhere near zero on either variable.

8. In order to obtain full benefit from the analysis, the theory to be tested must be very precisely stated. Item 4*b* above refers to whether $r_{y1.23}$ and $r_{y2.13}$ are meaningful and how directionality is predicted. If it is predicted that high values of X_2 are conducive of more positive effects from X_1, $r_{y3.12}$ is expected to be positive. If low values of X_2 are so conducive, $r_{y3.12}$ is expected to be negative.

9. In the test of significance of $r_{y3.12}$, a high $R_{3(12)}$ is not a matter for great concern, nor is change of the origins of X_1 or X_2 a solution to this. If singularity of the matrix occurs, it is possible to avoid it by manipulating origins before calculating X_3.

10. If a multiplicative interaction term is included in a path analysis, the standardized path coefficients from X_1 and X_2 to (X_1X_2) and from all three of these to a dependent variable can be interpreted only if X_1 and X_2 are measured on ratio scales. If they are not, it is necessary to express the com-

bined effect as a single one. This appears to be best achieved by changing the origins of X_1 and X_2 so that their paths to the dependent variable are zero as in item 3 above. The three variables may now be entered in the path analysis showing (X_1X_2) as dependent on X_1 and X_2.

Empirical tests of the statements in items 1, 2, 3, and 9 above are easily performed and were made on some real data which were known by a more generalized earlier test to conform to this model. The results were as predicted. In particular, $r_{y3.12}$ remained constant in three different multiple regressions involving different origins of X_1 and X_2, in which $R^2_{3(12)}$ was .87, .98, and .74, respectively.[17]

It is perhaps useful to consider forms of conduciveness theory which do not predict a zero effect of one variable when the other variable has a value of absolute zero. If X_1 is a ratio-scale variable conducive of the effects of X_2, a nonzero value for $b_{y2.13}$ may be predicted if other unmeasured variables are also conducive of X_2's effect, if this effect is enhanced but not completely determined by nonzero values on X_1, or if zero on the ratio scale of X_1 cannot be taken to indicate a complete absence of the quality X_1 is intended to indicate. In the latter case there is a specific form of measurement error; in the former two it can be said that the presence of the quality indicated by nonzero values of X_1 is a sufficient but not necessary cause of differing values in Y. Some universities observed that presence of loose rocks was conducive to broken windows during the turbulent 1960s and 1970s. Cementing these down reduces this conduciveness but does not prevent breakage, as other conducive materials are likely to be present.

THEORY OF DECREASING OR INCREASING EFFECT AND THE GENERAL U-CURVE

Such theories involve only a single independent variable whose effect changes with its own value, predicting a curvilinear relationship. There are a variety of possible forms which curves may take (Ezekiel and Fox 1959, p. 69). In each case the relationship of an independent variable, X_1, to Y is hypothesized to be a function of the state of X_1 itself. In the simplest form, $\partial \hat{Y}/\partial X_1 = c + dX_1$, which may be derived from the general equation for a parabola:

$$\begin{aligned} \hat{Y} &= a + cX_1 + dX_1^2 \\ &= a + b_{y1.2}X_1 + b_{y2.1}X_2 . \end{aligned} \tag{19}$$

As before, this can be converted to a single effect by change of the origin

[17] It is hoped that this will be a convincing demonstration that the use of multiplicative interaction terms is not meaningless and that the meanings of the results must be interpreted with care.

of X_1:

$$\hat{Y} = a' + b'X'^2_1 , \qquad (20)$$

where

$$X'_1 = X_1 - k , \qquad (21)$$

$$a' = a - b'k^2 = a - dk^2 = a - \frac{c^2}{4d} , \qquad (22)$$

and

$$b' = -\frac{c}{2k} = d , \qquad (23)$$

and, again, $b_{y2.1}$ does not change with the origin of X_1, nor does $r_{y2.1}$, though r_{12} will vary drastically.

Some relationships are not so simply expressed by postulating that the rate of increase of Y with X_1 is itself a linear function of X_1. Where, for instance, it is predicted that X_0 will initially rise steeply with X_1 but that this increase will taper off, another expression is needed. The following,

$$\hat{Y} = a + bX_1 + \frac{c}{X_1} , \qquad (24)$$

giving

$$\frac{\partial \hat{Y}}{\partial X_1} = b + \frac{c}{X_1^2} , \qquad (25)$$

is an appropriate form of analysis subject to the discussion below.

The determination of curvilinear relationships by the hyperbola,

$$\hat{Y} = a + b\left(\frac{1}{X_1}\right), \qquad (26)$$

is limited in the forms it can take by the fixed origin of X_1. In order to provide a completely flexible form, $(k + X_1)$ must be substituted for X_1 (Ezekiel and Fox 1959, p. 70). Further flexibility is added by the addition of the term dX_1 to the equation

$$\hat{Y} = a + \frac{b}{k + X_1} + dX_1 . \qquad (27)$$

In this form it is capable of yielding the variety of curves indicated in the cells of figure 5, including asymmetric U-curves, but it is not in a form immediately amenable to multiple regression. General approaches to the estimation of nonlinear equations are discussed by Goldfeld and Quandt (1972) and by Malinvaud (1970). For the specific equation (27) above, a method which uses conventional multiple regression will be described.

Multiplying (27) through by $(k + X_1)$, transferring a term, and dividing throughout by k, we obtain

$$\hat{Y} = \left(a + \frac{b}{k}\right) + \left(d + \frac{a}{k}\right) X_1 + \frac{d}{k} X_1^2 - \frac{1}{k} X_1 \hat{Y} \qquad (28)$$

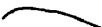
$$= a_{y.123} + b_{y1.23}X_1 + b_{y2.13}X_1^2 + b_{y3.12}X_1\hat{Y} , \qquad (29)$$

		k: Positive		k: Negative	
		d: Positive	d: Negative	d: Positive	d: Negative
b: Positive	$b_{y2.13}$	+	–	–	+
	$b_{y3.12}$	–	–	+	+
	Curve				
b: Negative	$b_{y2.13}$	+	–	–	+
	$b_{y3.12}$	–	–	+	+
	Curve				

FIG. 5.—Predictions of signs of coefficients in equation (29) for various types of curve. Note: $X_2 = X_1^2$; $X_3 = X_1\hat{Y}$. As constant a is not generally predictable, the intercept and $b_{y1.23}$ are unpredictable. Signs of the other two coefficients do not fully determine the form of curve, which should be checked by other means.

where

$$
\begin{aligned}
k &= -\frac{1}{b_{y3.12}}, \\
d &= kb_{y2.13}, \\
a &= k(b_{y1.23} - d), \\
b &= k(a_{y.123} - a).
\end{aligned}
\tag{30}
$$

and

In order to obtain the correct equation, it is now necessary to use the three predictor variables X_1, X_1^2, and $X_1\hat{Y}$, but the latter cannot be obtained as $\hat{Y}$ is unknown. To overcome this an iterative procedure may be used. Using only X_1 and X_1^2 the predicted value of Y is obtained, and this is used for a second regression on X_1, X_1^2, and $X_1\hat{Y}$, the process being continued until the predicted value of Y obtained is essentially the same as that used to obtain it. The method might be termed one of reflective interaction as the predicted value of Y is reflected back to predict Y in interaction with X_1.

Figure 5 provides predicted signs for the coefficients $b_{y2.13}$ and $b_{y3.12}$. The coefficient $b_{y1.23}$ is indeterminate as it depends on the constant a, which is normally not predictable. To simplify matters, a small diagram in each cell shows the type of curve expected. Some of these are monotonic, and for each of these the predicted signs of the two coefficients are the same as for one of the nonmonotonic curves. These coefficients therefore provide only a first check. It is advisable to calculate the value of $1/(k + X)$ and to enter it in

a new multiple regression with X in order to provide direct tests for b and d, predictions for which are to be found in the marginals of figure 5. The significance test for $b_{y3.12}$ indicates whether the curve differs significantly from a parabola as the latter is predicted by X_1 and X_1^2.

An additional feature of these predictions should be noted. In order to make them, it is necessary that X_1 should have a range extending to either side of zero. This ensures against the possibility that $k = 0$, in which case the method is invalid as $k\hat{Y} = 0$. It also ensures that the sign of k depends entirely on whether the "hook" in the curve occurs at high or low values of X_1. Hence, it is advisable to express X_1 as a deviation from the mean before commencing the analysis.

Just as it is necessary that k should not be zero, it is also necessary that b should not, as in these circumstances k is unidentifiable. Experience suggests that as b approaches zero the iterative process becomes unstable. Using artificial data in which Y was the simple function $(X_1 + e)$, where e was random error, the method produced no convergence of predicted values. At no point did the multiple correlation diverge notably from that obtained in the first step, using only X_1 and X_1^2, and wild swings in the partial regression coefficients continued throughout. In an example using real data the same thing happened, and in this case it was concluded from the first step that the relationship most closely approximated a parabola. In cases in which this happens the first step, if it consists only of the variables X_1 and X_1^2, helps to indicate that another model, linear or parabolic, provides a better fit.

An Empirical Example

As an example, artificial data were created for curve a in figure 6 for 120 cases distributed rectangularly on X_1. Random error was added to the calculated value of $\hat{Y}$. The equation of the line a is

$$\hat{Y} = -4.06 - X_1 + \frac{196.078}{15.006 - X_1}, \tag{31}$$

which can be expressed as $\hat{Y} = 8.93 - 0.729X_1 + .0666X_1^2 + .0666X_1\hat{Y}$.

To obtain the predictive equation from the data by means of multiple regression it is necessary to know $\hat{Y}$. Two iterative routes were used. In the first Y (with error) was regressed on X_1 and X_1^2 to obtain $\hat{Y}_1$. The latter was then used in regressing Y on X_1, X_1^2, and $X\hat{Y}_1$ to obtain $\hat{Y}_2$. The procedure was repeated, 10 multiple regressions being performed. The values of $\hat{Y}_i$ converged, $\hat{Y}_8$ and $\hat{Y}_9$ having a correlation of 1.00000 and a correlation of .99964 with the error-free values of Y originally calculated. The equation obtained was $\hat{Y} = 8.721 - .719X_1 + .0784X_1^2 + .0648X_1\hat{Y}$, giving

$$\hat{Y} = -7.58 - 1.21X_1 + \frac{251.26}{15.417 - X_1}, \tag{32}$$

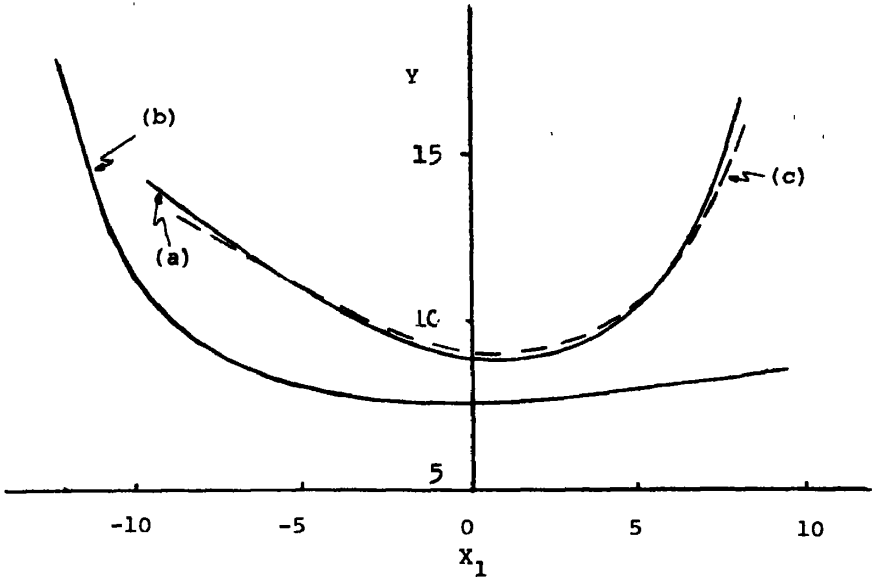



FIG. 6.—Three asymmetric nonmonotonic curves. Line *c* is line *a* with random error added, as predicted by iterative method described. Equations: (*a*) $Y = [196.078/(15.006 - X_1)] - X_1 - 4.06$, (*b*) $Y = [1/(.375 + .025\,X_1)] + .2X_1 + 5.00$, and (*c*) $\hat{Y} = [251.26/(15.417 - X_1)] - 1.21X_1 - 7.58$.

which looks rather different but yields a curve almost identical with *a* in (31) above when plotted by the broken line. The small difference is probably due to correlations with the random error. The multiple R was .55, and the curve obtained is shown in figure 6.

The second route was identical except for the regression of Y on X_1, X_1^2, and X_1Y (with error) in the first step. It took slightly longer to converge on the same result, $\hat{Y}_9$ and $\hat{Y}_{10}$ having a correlation of .99999. It also tended to show greater initial oscillation in the regression coefficients obtained at each step.

There are two reasons for going into these curvilinear relationships in such detail. The first is that the analyses given are relevant for two later analyses involving two independent variables. The second is to stress the relationship between interaction terms for two variables and terms which express a curvilinear relationship. This relationship makes it possible for either to create a spurious impression of the other.

Spuriousness, Curves, and Interaction

One of the ways in which statistical interaction has often been tested is by the subdivision of a sample into two subsamples. This creates subsamples high and low in value, respectively, on the variable X_2. The relationship be-

tween Y and X_1 is then obtained for each subsample, possibly with a test for the differences between the two regression coefficients. When this test is performed on correlation coefficients it is definitely wrong, as the hypothesis must refer to regression coefficients (Blalock 1971). But it is also wrong because it is impossible by this approach to control for the occurrence of spurious interaction.

Suppose that two subsamples have been obtained, subdividing at the median of X_2, and that X_1 and X_2 are positively correlated. The subsample high on X_2, therefore, also tends to be high on X_1. If the relationship of Y to X_1 or to X_2 is in the form of a U-curve, one subsample will tend to consist of one end of this curve while the other will tend to consist of the other end. A test for the difference between the two regression coefficients does not necessarily indicate that interaction between X_1 and X_2 exists.

Performing the analysis using a simple multiplicative interaction term does not avoid this problem. If r_{12} is positive, then X_1^2 is likely to be positively related to (X_1X_2). Consider the case in which the relationship of Y to X_1 and X_2 is correctly described by

$$Y = a + b(X_2 + k)^2 + e_y \tag{33}$$

and where X_1 and X_2 are related by the equation $X_2 = d + fX_1 + e_2$. Equation (33) can then be transformed into

$$\begin{aligned} Y = {} & [a + bk(k + d)] + bfkX_1 + b(k + d)X_2 \\ & + bfX_1X_2 + bke_2 + bX_2e_2 + e_y \,. \end{aligned} \tag{34}$$

If, testing for an interactive relationship, Y is regressed on X_1, X_2, and X_1X_2, the coefficients for these three variables would be equivalent to the appropriate constants in equation (34) plus components derived from the constants for the unincluded variables e_2, e_y, and X_2e_2. As it is likely that the relationships between these variables and the included variables will be small, these components are also likely to be small. The relationship therefore will appear to be interactive. The argument is easily reversed, assuming that the true relationship with Y is interactive. To be sure that the relationship is one of interaction rather than parabolic curvilinearity, it is necessary to control by using the terms X_1, X_2, $X_3 = X_1^2$, $X_4 = X_2^2$, and $X_5 = X_1X_2$ simultaneously. A test of the partial correlation $r_{y5.1234}$ will then confirm that multiplicative interaction is occurring while tests of $r_{y3.1245}$ and $r_{y4.1235}$ will provide evidence for the occurrence of curvilinearity. If both seem to be occurring, there is still the possibility that a symmetric equilibrium model might provide a better fit. Whether the suggested controls are needed or the alternative model should be considered depends on what makes theoretical sense, but it is best that this should be dealt with before statistical analysis if possible.

When the symmetric equilibrium model is considered below, it will be

found that similar arguments can be applied to that model but that controls are impossible for the same reasons as apply to the relative excess model. Substantive, as opposed to geometric, parsimony must be used carefully in all analyses using these models. One of their virtues lies in the close attention they require be given to initial theory construction.

THE PROPORTIONAL EXCESS MODEL

In the discussion of the relative excess model, the effect of the difference between the levels of two variables was assumed to be a linear function of $(c + X_1 - dX_2)$. It is possible that the theory could be better stated in another way, namely, that the effect is a function of the proportional difference in levels rather than the absolute difference. If both variables are measured on interval scales, an additive constant will be required for both variables and a multiplier for one variable; and we could state the theory in this way, providing for direct singular additive effects in addition:

$$\begin{aligned}\hat{Y} &= a + bX_1 + cX_2 + \frac{d[e + X_1 - f(g + X_2)]}{g + X_2} \\ &= (a - df) + bX_1 + cX_2 + \frac{dX_1}{g + X_2} + \frac{de}{g + X_2}\,.\end{aligned} \tag{35}$$

If we can be confident that X_1 is measured on a ratio scale, the last term disappears as e approaches 0. There are identification problems as use of the iterative method proposed earlier produces an underidentified model. But probably as severe is the specification difficulty. Should the denominator in the ratio variable include X_2 or X_1? As shown, the equation is linear in X_1 and curvilinear in X_2; reversing the ratio reverses this. Yet it is extremely difficult to specify which ratio is correct.

The problem is exemplified in an analysis by Spilerman (1970) in which he used four proportional excess variables to indicate relative deprivation of urban blacks: "The ratio of percent of Nonwhite males working in traditionally Negro occupations to percent of the White male labor force so employed; the ratio of Nonwhite median family income to White median family income; the ratio of the Nonwhite male unemployment rate to the corresponding White rate; and the ratio of Nonwhite median education to White median education" (p. 640).

In each case the variables may be assumed to be ratio scaled, and there are no complications. But as they stand, the four variables differ in that two indicate relative deprivation positively and two negatively, although white rates are in the denominator throughout. Two are ratios of rates indicating well-being and two of rates indicating the reverse. Taking the interesting example of unemployment rates, we could transform the ratio to indicate relative deprivation negatively either by taking the reciprocal or by using

the employment rate. These yield different answers. Empathically, we can imagine blacks expressing their relative deprivation in any of the following four ways (assuming that 10% of whites and 20% of blacks are unemployed): "We have twice the percentage unemployed that they do"; "They have only half the percentage unemployed that we do"; "We have only eight-ninths the percentage employed that they do"; and "They have $1\frac{1}{8}$ the percentage employed that we do."

These four statements appear to be equivalent, yet across a sample of cities they yield four variables none of which correlates unity with any of the others. For purposes of multiple regression they are therefore not equivalent; yet the problem of specifying which one is correct is formidable. Spilerman does not attempt to do so, and the four ratios he uses illustrate a problem which is quite additional to those relating to such variables discussed by Schuessler (1973).

The specification and estimation problems are sufficiently complex to prohibit an attempt to derive a solution here. Instead, it is suggested that a generalized analysis might be attempted, delineating the three-dimensional form of the regression surface of Y on X_1 and X_2.

SYMMETRIC EQUILIBRIUM AND THE PARABOLIC CYLINDRICAL SURFACE

In the discussion of what was termed relative excess theory, it was assumed that Y increased when X_1 was high relative to X_2 and decreased when X_1 was low relative to X_2. In a different version of such a theory, it may be hypothesized that Y increases under both conditions. This is illustrated in figure 7. The variable $\hat{Y}$ is minimal along the line ab and increases at distances D off this line at right angles to it. The whole model could be reversed, providing for decreasing values of $\hat{Y}$ with increasing D. For this reason line ab will be referred to as the inflection line.

The model fits one form of equilibrium theory. The inflection line represents the line which defines values of variables X_1 and X_2 which indicate states functionally appropriate for each other. Cases which do not lie on this line are then in a state of disequilibrium, and we may predict higher values of some variables, Y (e.g., reequilibrating activities, social change, conflict, social problems, search for solutions), and lower values of others (e.g., mental health, employment, normative integration).[18]

[18] One of the classic dilemmas of sociology is illustrated by comparing this with a proposition suggested by Orum (n.d.). He suggested that political conflict in cities between blacks and whites, as indicated by formation of civil rights groups, should increase as the two groups became more similar. Generalized, it could be predicted that competition and conflict would increase as a subordinate group improved its position toward parity with a superordinate group and would decrease as the former surpassed the other and itself became superordinate. Such a proposition is consistent with Coser (1964) and is almost identical with Organski's (1968) proposition regarding war between nations. It predicts

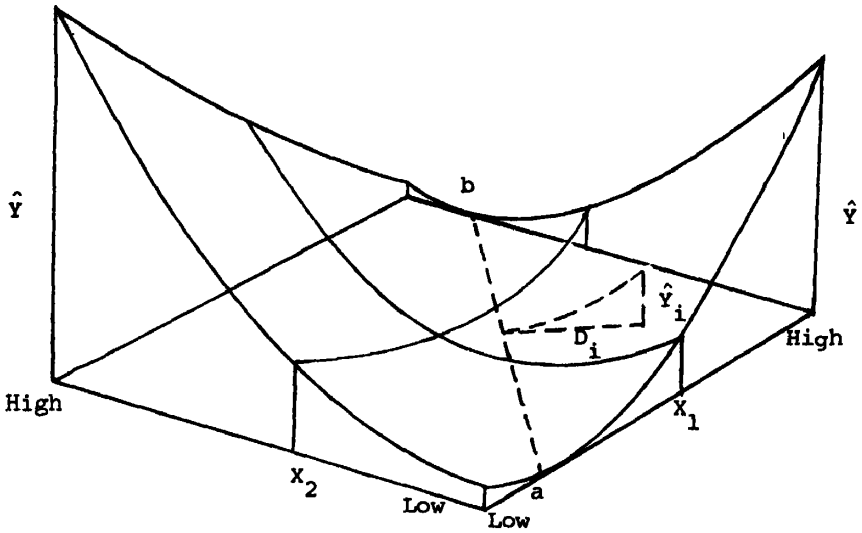



FIG. 7.—Three-dimensional perspective of symmetric equilibrium model. Line *ab* is that of minimum $\hat{Y}$, which increases with D_i^2 to either side of it.

The model assumes symmetry and continuity. The surface representing $\hat{Y}$ in figure 7 is a smooth curve with no discontinuities and is symmetric to either side of the straight line *ab*. Other models could be developed; this appears to be the simplest.

One problem inherent in such theories is the difficulty of establishing the position of the inflection line. Social theories are rarely operationalized precisely enough to provide a priori equations of this nature. Nor is it expected that they should be, as the position of this line is itself a matter for research hypotheses and empirical investigation. In part, then, the analysis is required to demonstrate, first, that the effect is found and, second, that the inflection line is in the general position predicted.

Accepting that the position of the line is unknown, it is still possible to postulate that it is p units distant from a parallel line which passes through the origin of X_1 and X_2. If the distance of any point from the inflection line is D and that from the parallel line through the origin is H, then

$$D = H - p\,. \tag{36}$$

The distance H can be represented (Van de Geer 1971, p. 24) by

$$H = X_1 \sin\theta - X_2 \cos\theta\,, \tag{37}$$

the highest level of conflict on the diagonal, unlike the model proposed above. The paradox may be resolved if it is considered that conflict itself is systemic, attaining stability when opposing forces are equal and decreasing to the point of extinction as one of the opposing forces predominates, when the system is transformed into one of Dahrendorf's (1959) "imperative coordination."

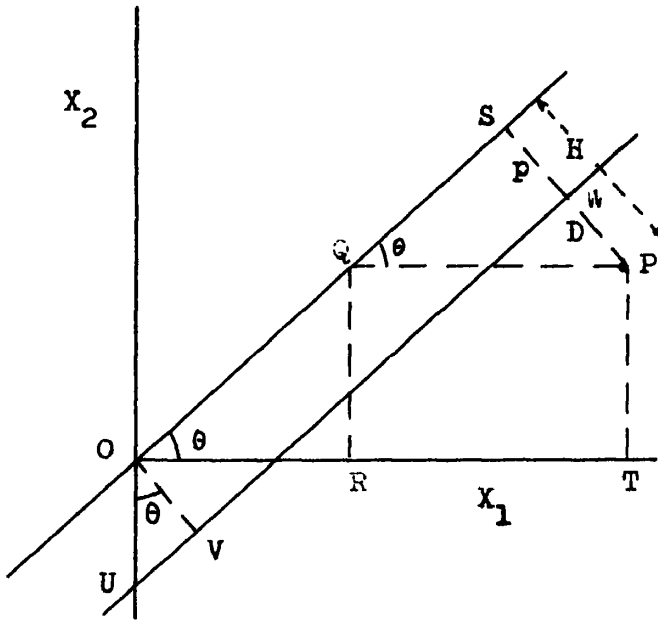



FIG. 8.—The derivation of distance, D, of the point, P, from the inflection line, UW

where θ is the angle between the inflection line and the axis of variable X_1. In figure 8 the line UW represents the inflection line; OS, the parallel line through the origin 0; and P, a point distant D from the inflection line. The distance OT is equal to the value of X_1 for point P, and QR is equal to the value of X_2. Lines SP and OV are at right angles to OS and UW. The convention is adopted that distance p and H are measured positively downward from S and that distance D is similarly measured from point W.

It can be seen that $OR = QR \cot\theta = X_2 \cot\theta$, $QP = OT - OR = X_1 - X_2 \cot\theta$, and $SP = D + p = H = QP \sin\theta = X_1 \sin\theta - X_2 \cos\theta$. As the slope of the line OS is equal to the tangent of angle θ and OS passes through the origin, the equation of this line is

$$X_2 = X_1 \tan\theta . \tag{38}$$

For the equation of the inflection line UW, we must obtain the value of the distance OU: $OU = -OV/\cos\theta = -p/\cos\theta$. As the slope of the inflection line UW is also equal to tan θ, we obtain for the equation of this line

$$X_2 = -\frac{p}{\cos\theta} + \tan\theta \, X_1 . \tag{39}$$

The theoretical model may now be expressed by the hypothesis that $\hat{Y}$ is a parabolic function of D, that is,[19] that

$$\hat{Y} = c + kD^2 . \tag{40}$$

Substituting in this equation for equations (36) and (37), we obtain

$$\begin{aligned} \hat{Y} = c + kp^2 - 2pk \sin\theta\, X_1 + 2pk \cos\theta\, X_2 + k \sin^2\theta\, X_1^2 \\ + k \cos^2\theta\, X_2^2 - 2k \sin\theta\cos\theta\, X_1X_2 , \end{aligned} \tag{41}$$

and, if no other effect is present, this could be represented by the regression equation, $\hat{Y} = a_{y.12345} + b_{y1.2345}X_1 + b_{y2.1345}X_2 + b_{y3.1245}X_3 + b_{y4.1235}X_4 + b_{y5.1234}X_5$, where $X_3 = X_1^2$, $X_4 = X_2^2$, $X_5 = X_1X_2$.

The five variables which must be entered into the multiple regression are therefore defined. If the model is correct and without any additional effects, the equation of the inflection line may be obtained by equating the partial differentials of $\hat{Y}$ by X_1 and X_2 to 0, giving, respectively,

$$X_2 = -\frac{b_{y1.2345}}{b_{y5.1234}} - \frac{2b_{y3.1245}}{b_{y5.1234}} X_1 \tag{42}$$

and

$$X_2 = -\frac{b_{y2.1345}}{2b_{y4.1235}} - \frac{b_{y5.1234}}{2b_{y4.1235}} X_1 , \tag{43}$$

each of which reduces to

$$X_2 = \frac{-p}{\cos\theta} + \tan\theta\, X_1 . \tag{44}$$

If additional effects exist, this identity does not occur, as for instance, if $b_{y3.1245}$ and $b_{y4.1235}$ are 0, or if $b_{y5.1234}$ is 0, the two partial derivatives produce two equations which are not the same but yield lines at right angles to each other. If the coefficients are not zero but too small, the equations will probably yield two lines which are at a large angle to each other. Where the two lines are each generally oriented as predicted and intersect at a small angle, this constitutes evidence that the model has validity, though it is still necessary to state the forms of the regression coefficients under which the line is in the predicted direction and the relationship generally as predicted. Preparation of a contour diagram is helpful in diagnosing effects.

Table 1 provides the predicted signs of the regression coefficients for the two conditions, direction of the inflection line and whether k in equation (40)

[19] The two variables are on different scales which are, in relation to each other, arbitrary. Linear transformation of either or both scales will produce a transformation of the whole in which line ab is oriented differently to the axes and the vertical distance, h, from any point to this line differs from H. It is easily possible to show that h is a linear function of H. The predicted value of Y is therefore parabolic under both conditions, and linear transformation of either or both variables will not affect the prediction.

TABLE 1

PREDICTION OF SIGNS OF PARTIAL REGRESSION COEFFICIENTS FOR GIVEN PREDICTIONS OF SIGNS OF TAN θ AND k

TAN θ	k: Positive	k: Negative
Positive.	$b_{y3.1245}$ Positive	$b_{y3.1245}$ Negative
Positive.	$b_{y4.1235}$ Positive	$b_{y4.1235}$ Negative
Positive.	$b_{y5.1234}$ Negative	$b_{y5.1234}$ Positive
Negative.	$b_{y3.1245}$ Positive	$b_{y3.1245}$ Negative
Negative.	$b_{y4.1235}$ Positive.	$b_{y4.1235}$ Negative
Negative.	$b_{y5.1234}$ Positive	$b_{y5.1234}$ Negative

is predicted to be positive or negative. In each cell all three coefficients must be of the predicted sign.[20]

Controlling for Other Effects

In the model just described the polynomial required to account for the predicted effect is the same as the one earlier recommended to ensure that parabolic effects for X_1 and X_2 are not confused with their interaction. The three different analyses are, therefore, closely linked with each other and may be confounded. Even if the significance tests indicated in table 1 prove correct, the regression coefficients obtained need not conform precisely to those predicted in equation (41). It is not possible to test for the independence of a symmetric equilibrium effect from the additive effects of the five

[20] Taylor (1973) provides analogous modes of analysis for Heider's balance theory and Osgood's principle of attitude congruity (Osgood and Tannenbaum 1955). These are more complex by reason of requiring three interacting variables, but, for any given value on one of these, the other two should interact as suggested here. In the case of Heider's theory, Taylor simplifies the analysis by assuming dichotomous variables, thus ensuring that $X_1^2 = X_1$ and $X_2^2 = X_2$. The model therefore reduces to simple multiplicative interaction. In the case of Osgood's theory, Taylor assumes one dichotomous and two ratio-scale variables. His analysis is simplified by his ability to assume ratio-scale variables on identical ratio scales which makes it possible to assume that $\theta = 45$ degrees with the inflection line passing through the origin. He differs also in assuming a discontinuous model with linear increases of $\hat{Y}$ with D on each side of this line. This requires the performance of two analyses, one on each side. The mode of analysis offered here is more general. It will be noted also that his derivation of D differs from that given here and is seemingly incorrect. With $\theta = 45$ degrees this does not invalidate his analysis, as $|\sin\theta| = |\cos\theta|$. By assuming that the third variable in Osgood's congruity theory is dichotomized $(1, -1)$, Taylor avoids a very sticky problem. This variable is held to interact with the other two in two ways. First, when it is zero the entire interaction disappears (i.e., k in [40] becomes 0). Second, as it changes from negative to positive the angle of θ changes. In the general formulation this angle might be expressed as $\theta = (\gamma + gX_3)$ while, in (40) above, tX_3 would have to be substituted for k. This produces complex problems in estimating coefficients. In Taylor's analysis these are avoided but at the expense of precision and flexibility.

variables in the multiple regression by which it is detected. The expected effects of random sample variation and of spuriousness will tend to make the two equations (42) and (43) differ, in turn making it difficult to estimate the position of the inflection line. Nor is it necessarily appropriate to estimate its position as intermediate between the two. It would be possible, for instance, when the two coincide fairly closely, to calculate the intersection of the two lines and place through it a line with a slope equal to the mean of the slopes obtained in the two equations (42) and (43).

But if, in addition to the disequilibrium effect, there is a parabolic effect of X_1, partial differentiation with respect to X_1 will produce a false equation while that with respect to X_2 will produce the correct equation. The mean slope as an estimate is therefore false. Additive linear effects by X_1 and X_2 will cause equations (42) and (43) to differ only in the two constant terms. Simple multiplicative interaction between X_1 and X_2 will change the coefficient $b_{y5.1234}$ and therefore change the two equations derivable for the line.

It seems that theoretical considerations must determine in part the estimation of exactly where the inflection line should be placed. If additional effects are predicted, consideration must be given as to how these would affect equations (42) and (43). This would be determined by examining their effect on the terms in the two equations.

In using a contour diagram in diagnosing the kinds of additional effects which may be occurring, care must be taken to interpret the contour lines only within the spatial domain occupied by cases, as the extrapolation of the equation beyond this domain may be quite misleading.

Even at this point numerous explanations of the curved surface produced by the equation clearly are possible. It is not only possible but desirable to express the disequilibrium effect by a single coefficient for a single interaction term. This would produce, essentially, a return to a singular model. In the present case the term is clearly D^2. Given the position of the inflection line, D can be calculated. Where the line is

$$X_2 = u + vX_1 , \tag{45}$$

v is $\tan\theta$ and θ can be identified, giving

$$D = X_1 \sin\theta - (X_2 - u)\cos\theta . \tag{46}$$

But numerous positions of the line can still be selected. There are several alternatives:

1. Based on the diagnosis already performed, select a position which produces any expected extralinear or curvilinear effect from X_1 or X_2. This solution does not produce clear criteria for any single position and is always open to doubt.

2. Select a position of the line which produces the maximum correlation between Y and D^2. An iterative solution is needed for this in which the line is rotated about the intersection of the two lines produced by the two partial

differentials. The contour diagram will normally provide a good indication as to where the optimum position is. This will produce a single solution.

3. Place the line through the intersection of the two lines produced by the partial differentials and any other point which appears to be theoretically fixed. The most obvious appears to be the origin of X_1 and X_2, assuming that they are measured on ratio scales. This solution is questionable as the existence of effects additional to those of disequilibrium will shift the inflection line away from the origin, so it should only be used when it is fairly clear that they do not exist.

Solution 2 appears to be the most preferable. It is in fact possible to obtain it without any of the previous analysis by selecting an iterative series of values of θ, calculating $H = X_1 \sin\theta - X_2 \cos\theta$, and thereafter regressing X_0 on H and H^2 for each value of θ. The earlier analysis makes it easier to select an initial value of θ close to the final solution. Given that the earlier analysis was not performed and that the optimum θ provides the equation $\hat{Y} = a' + b_{y1.2}H + b_{y2.1}H^2$, the value of D is given by

$$D = H + \frac{b_{y1.2}}{2b_{y2.1}}, \tag{47}$$

and the position of the inflection line of given θ is given by

$$X_2 = \frac{b_{y1.2}}{2b_{y2.1}\cos\theta} + \tan\theta\, X_1 . \tag{48}$$

As the effect of D^2 has been shown to be perfectly predictable by X_1, X_2, X_1^2, X_2^2, and X_1X_2, it is impossible to test for its independence of these terms. There are two alternatives. The first is to select such terms as seem theoretically justified. If theory suggests a linear relationship of Y with X_2, we can regress Y on D^2 and X_2; if a curvilinear relationship is suggested, we can regress Y on D^2, X_2, and X_2^2. If there are strong theoretical reasons (as opposed to, say, past empirical experience) for predicting a linear relationship with X_2 in addition to equilibrium effects, it may be preferable to perform each iterative series of regressions on H, H^2, and X_2 so as to permit the optimum angle θ to emerge under control for X_2. The second alternative is atheoretical, that of performing a stepwise multiple regression of Y on D^2 and all five terms. Failure of D^2 to emerge with a significant relationship indicates that an equilibrium model is not supported under conditions in which its potential effect has been maximized.

It has been shown that direct effects from any of the five terms additional to that of disequilibrium will produce errors in the position of the inflection line identified by the preceding methods. An approach paralleling that suggested by Blalock (1966) for the relative excess model is possible. If such additional effects appear to be suggested by theory and if intervening variables in these processes can be identified, these variables may be included

in the multiple regression from the beginning. That is, if X_3 and X_4 are intervening variables, an initial multiple regression may be performed on X_1, X_2, X_1^2, X_2^2, X_1X_2, X_3, and X_4, or on H, H^2, X_3, and X_4 in an iterative series. Assuming that difficulties of multicollinearity do not occur, the coefficients for the terms involving X_1 and X_2 (or H and H^2) will predict a surface from which the spurious effects have been partialled out. The procedures described can then be used with the extra benefits that fewer complications from spurious effects will be expected. It may, for instance, be possible to adjust the position of the inflection line so that it passes through the origin of (X_1, X_2) with greater confidence.

An Example

In an earlier generalized analysis of data for 40 nations, an example of data which appeared to fit this model was discovered. In this case the dependent variable Turmoil (Tanter 1965; Southwood 1978) was found to be related to McClelland's (1961) cultural variable *n* Achievement and to the adjusted secondary school enrollment ratio. The theory in this case is derived from Merton's deviance paradigm, *n* Achievement indicating degree of socialization into a culture goal, while secondary school enrollment indicates degree of access to institutionalized means of achievement. Merton's paradigm deals with only one sector, as the existence of the culture goals and interference with access to institutions are assumed. Along an apparent diagonal from low on each to high on each variable, Turmoil was minimal, increasing to each side of this line. However, among nations at the lowest educational levels this pattern was disrupted, and the analysis produced a surface which was more complex than that predicted by a symmetric equilibrium.

The data were retested by the method described above with the omission of the five cases lowest in education in order to provide a clearer example. The multiple regression was run on the variables X_1, X_2, X_1^2, X_2^2, and X_1X_2, where X_1 is a linear transformation of McClelland's *n* Achievement and X_2 is the adjusted secondary education ratio. The equation obtained was $Y = -4.239X_1 - 1.435X_2 + .2175X_1^2 + .03705X_2^2 - .09288X_1X_2 + 80.21$.

The surface delineated by this equation is shown in figure 9. This type of surface could be produced by the predicted surface intersecting with another similar to it and at a slight angle to it, and there seems no evidence of any spurious interaction effect. Differentiating Y with respect to X_1 and X_2 and equating each to zero provides the following two equations, respectively: $X_2 = -45.64 + 4.684X_1$ and $X_2 = 19.36 + 1.2535X_1$.

These are shown in figure 9 as lines *a* and *b*, respectively. Thus, there seems to be no simple solution, the total effect apparently including some components additional to that predicted. However, within the domain occupied

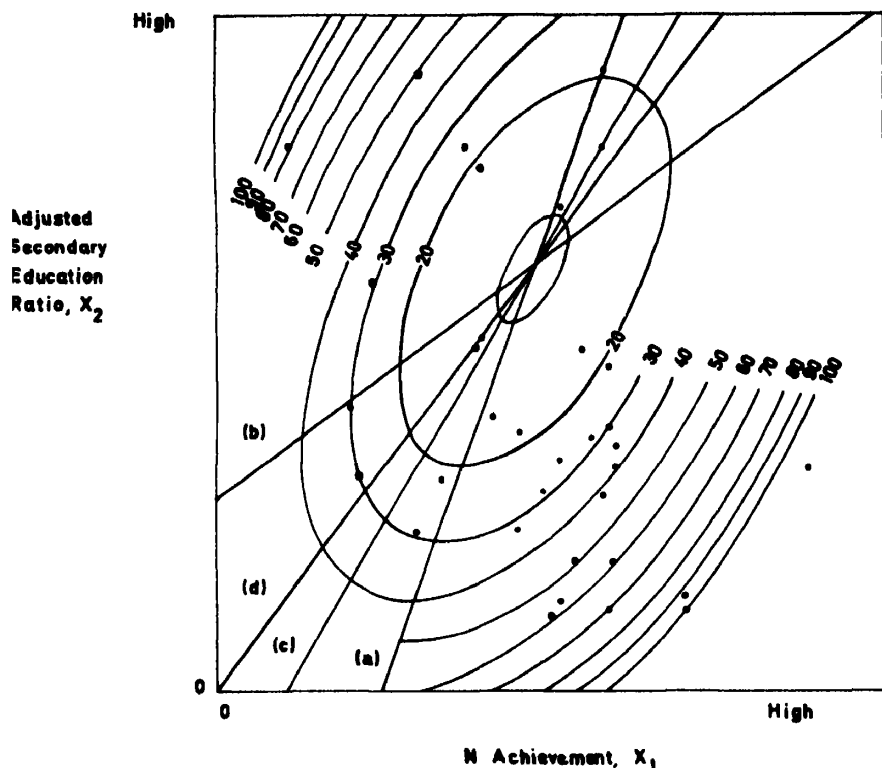



FIG. 9.—Contour diagram showing levels of Turmoil predicted by X_1, X_2, X_1^2, X_2^2, and X_1X_2.

by cases, it appears that most of the effect would be accounted for by a parabolic surface centered on a line lying intermediate between *a* and *b*. Line *c* is a line inserted visually as an estimate of the line which might, if *D* is measured from it, produce a maximal correlation with D^2. This may seem a crude method, but in fact the variation in predictive power for lines close to this line which pass through the intersection of *a* and *b* will be seen to be very slight. Line *d*, finally, is placed through this intersection (for maximal effect) and through the origin of X_1 and X_2 as this appears to be theoretically best, and reasonable since it lies close to line *c*. There appears to be no theoretical reason for suggesting that any additional nonmonotonic curvilinear effect exists for either variable. As the adjusted secondary education ratio is strongly related to the level of economic development, however, it is at least possible that a negative monotonic relationship exists with X_2.

The values of *D* were calculated for each of the four lines with the hypothesis that D^2 would in each case be an important predictor of *Y*, but with some potential effects from X_1 and X_2 as previously discussed. In each case

a stepwise multiple regression was run with a permissive criterion of F = 1.1 for entry into the equation. The variables X_1, X_2, X_1^2, X_2^2, X_1X_2, D_i, and D_i^2, were entered, i referring to the line from which D was measured.

Only in one case were any other variables beyond D_i^2 entered, that of line a. The results were as follows: (1) Line a . . . D_a^2, X_2, X_2^2 entered, R = . 686 (the multiple-partial correlation coefficient for [X_2, X_2^2] was not significant at the .05 level; R for D_a^2 alone was .630); (2) line b . . . D_b^2 entered, R = .644; (3) line c . . . D_c^2 entered, R = .668; and (4) line d . . . D_d^2 entered, R = .674.

As none of these results suggests any significant additional effect and as the line d which seemed theoretically most justifiable produced the highest R from D_i^2 alone, this line appears to be the best choice. The decision is that this line is an estimate of the inflection line and that no other effect of X_1 and X_2 seems to exist. Alternative approaches would have been to vary the slope of the line through the origin and select that which yielded the D_i^2 with the highest correlation with Y, or to do this without requiring the line to pass through the origin. Neither of these would in the present case increase the explanatory power by much, if anything. The contour diagram

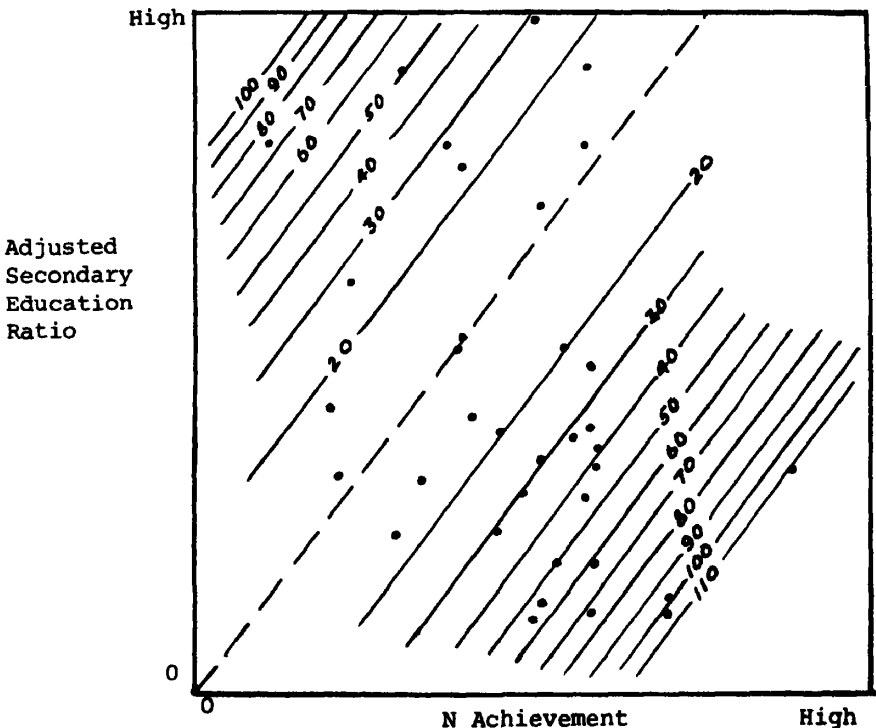



Fig. 10.—Final contour diagram showing values of Turmoil predicted by single interaction term appropriate to symmetric equilibrium theory. Broken line indicates line of minimum predicted Turmoil (the inflection line).

for the final predictive model is shown in figure 10 and is a typical example of the symmetric equilibrium model.[21]

Significance Testing

The tests used in the example take the value of $\hat{Y}$ predicted by a polynomial in X_1 and X_2 and treat it as if it used only one degree of freedom. Within the limits of a multiple regression in which this predicted value is used as a predictor variable, this is quite correct, but the assumption is incorrect in that more than one degree of freedom was already used in the determination of this $\hat{Y}$. There are difficulties which prevent a correct test, but there are some alternative approximate methods.

Suppose that we use R^2_S as the term for the coefficient of determination obtained by the optimum D^2, and R^2_5 for that obtained using (X_1, X_2, X_1^2, X_2^2, X_1X_2). The term R^2_S uses four degrees of freedom: one for θ and three in the multiple regression on H and H^2. While the equation can be transformed into one in the five variables used to obtain R^2_5, the six coefficients are not independent of each other. The term R^2_5 will always be equal to or greater than R^2_S as it does not depend on the same restrictions and uses six degrees of freedom. We can test for whether R^2_5 is significantly greater than R^2_S by using

$$F = \frac{R^2_{5.S}}{1 - R^2_{5.S}} \cdot \frac{N - 6}{2}, \qquad (49$$

where the squared multiple partial correlation coefficient,

$$R^2_{5.S} = \frac{R^2_5 - R^2_S}{1 - R^2_S}, \qquad (50)$$

as the set of variables producing R^2_5 is inclusive of that producing R^2_S.

But we shall most probably want to test whether the model's predictive power is independent of the subsets (X_1, X_2, X_1^2, X_2^2) and (X_1, X_2, X_1X_2). The first will test against simple curvilinearity (i.e., that θ is 0), and the second against a conduciveness model.

Suppose that the subset consists of k variables, including X_1 and X_2. As H is a weighted sum of X_1 and X_2, a test of H^2 against the k variables is appropriate. Obtaining a partial correlation coefficient for this, $r_{yH^2.1 \ldots k}$,

[21] It is not suggested in the present analysis that nations are seeking equilibrium by moving toward line *ab*. It is rather clear that they are not. The tendency of nations to lie in the bottom right-hand corner, with low educational enrollment and high stress on the culture goal of achievement, is in accordance with Hagen's (1962, pp. 232–33) suggestion that groups with low status will tend to develop such a goal. This is for reasons quite other than those of avoiding turmoil, involving perhaps manifest functions of *n* Achievement having nothing to do with the latent consequence of turmoil.

H^2 uses one degree of freedom, but one extra was used in optimizing θ pr
to entry. Testing the partial correlation in the normal way underestima
the number of degrees of freedom used by H^2, but as these are not exhaust
simultaneously there is no correct estimate. This approach is therefore ratl
liberal.

Treating H^2 as if it used two degrees of freedom means that the followi
test would be used:

$$F = \frac{r^2_{yH^2 1 \ldots k}}{1 - r^2_{yH^2 .1 \ldots k}} \cdot \frac{N - (k + 2) - 1}{2}. \qquad (5$$

This test treats the partial as if it were a multiple partial and overestima
the degrees of freedom used by H^2 in the multiple regression. In addition, t
F-test is nondirectional, while we need a directional test predicting a positi
sign. This test is therefore quite conservative and weak. The correct ansv
lies somewhere between the two. Use of all three tests, preferably using
.10 level of significance for the last F-test, provides the most information.

Inability to reject the alternative model does not necessarily mean tl
we reject the symmetric equilibrium model. It may well be that neitl
model can be shown to be independent of the other. In this case we mı
make a decision based on theoretical parsimony. A contour diagram is ve
useful for diagnostic purposes, but it must be used in conjunction witl
judgment as to which model is easier to explain in substantive terms.

THE ASYMMETRIC EQUILIBRIUM MODEL AND THE COMPOUND HYPE BOLIC/LINEAR FORM

The model and mode of analysis just described is the special case in wh
it is postulated that the relationship of the dependent variable with the t
independent variables is symmetric about the inflection line. This is ı
necessarily the case. The form taken by disequilibrium on each side of t
line is not the same. A classic example is that of Dickens's (1890) Micawb
"Annual income twenty pounds, annual expenditure nineteen ought and s
result happiness. Annual income twenty pounds, annual expenditu
twenty pounds ought and six, result misery." A happiness/misery varia
should relate to this disparity according to the relative excess model. I
it is very likely that there are behaviors which will increase with happin
but not decrease with misery, for example, expenditure on punch and gene
jollity (despite Micawber's apparent propensities), which may approach z
at the 20/20 point. Dickens's two types of disequilibrium, in fact, may
quire quite different theories to explain frequencies of the same type of
havior. Some types of behavior may increase on one side of the line of eq
librium but remain constant on the other. Others may increase to either s

but at different rates.[22] In the two extremes, which are special cases, the models of subtractive interaction and of symmetric equilibrium will be reached. In between lies the model of asymmetric equilibrium, which requires consideration. The mode of analysis used must provide for the relationship of Y with D to differ on each side of line ab in figure 8. In fact, this asymmetry may be of any of the kinds portrayed in figure 5, meaning that no inflection line need exist.

As the model is one of asymmetric curvilinearity on D, the general equation is of the same form as (27): $\hat{Y} = a + [b/(c + H)] + dH$ in which

$$H = X_1 \sin\theta - X_2 \cos\theta \,. \tag{52}$$

The distance H is again measured orthogonally from a diagonal line passing through the origin of (X_1, X_2). The analysis is flexible enough to place any detected diagonal of minimum value for Y wherever it falls, by adjustment of c and d, but this is specific to a given value of θ.

It first appears that an appropriate analysis can be performed by using X_1 and X_2 directly in place of H. Using an extension of the procedure used in the earlier section, this would yield an iterative series of multiple regressions of Y on X_1, X_2, X_1^2, X_2^2, X_1X_2, $X_1\hat{Y}$, and $X_2\hat{Y}$. This has three flaws. First, it yields an equation from which the required constants are overidentified. Second, it is too flexible, permitting too many spurious effects to enter into the resultant regression equation. Third, its flexibility permits a line along which $\hat{Y} = \infty$ to occur in absurd positions, rendering the results meaningless. It is therefore preferable to calculate H directly for a series of values of θ and obtain the equation which yields a maximum multiple correlation with Y.

This means that, in comparison with the symmetric equilibrium model, only one solution emerges for the value of θ, that which gives maximum explanatory power for the model. It is possible to perform a generalized analysis and prepare a contour diagram to obtain an initial estimate of the optimum θ before commencing the iterative multiple regressions. As maximizing the explanatory power of the model provides a single solution and is comparable with conventional procedures, this method appears to be quite appropriate.

An Example

As an example of this form of analysis, data similar to those for symmetric equilibrium will be used. While Turmoil appeared to fit a symmetric model,

[22] Adoption of the symmetric model also implies confidence that the scales for the two variables X_1 and X_2 are linear indicators of the relevant concepts. Even if the symmetric model is correct for isomorphically scaled variables, departures from this isomorphism may distort the data so as to make an asymmetric model necessary.

the relationship of Internal War (Tanter 1965; Southwood 1978) to th same two variables appeared to be asymmetric. At low values of n Achieve ment and high adjusted secondary education ratios, Internal War appeare to be uniformly low over the period tested. The same disruption of the pat tern at extremely low levels of secondary education occurred, and the gen eral position of the diagonal seemed by inspection to be roughly the same.

The same sample of 35 cases was used, omitting the five with lowest level of secondary education. Three diagonals were placed through the origin, th first identical with that found in the analysis for Turmoil and the other tw about four degrees to either side. For each diagonal line, $H = \sin\theta X_1 - \cos\theta X_2$ was calculated, and iterative solutions were obtained for each. I each case $r_{\hat{y}_i\hat{y}(i-1)}$ achieved a value of 1.00000 at step $i = 8$, more or less The multiple R's obtained were .785, .791, and .790, with the central diagona producing the marginally highest multiple correlation. The two analyses, o Turmoil and Internal War, were therefore supportive of each other, as ther appears to be no theoretical reason to suppose that the angles of the tw diagonals would be different. The fact that they appear to be identical is surprising feature as normal measurement error would be expected to pro duce greater variation. The angle θ selected here is therefore the same a that finally selected in the previous analysis.

The equation used in this case is the one obtained at the sixteenth iterativ step. Although convergence appeared to have occurred at the seventh step there was further marginal convergence in the regression coefficients afte this. While this was very small, it was decided to take advantage of the extr information provided by the nine additional steps used. The equation ob tained was

$$\hat{Y}_{16} = 2.3916 + .38154H + .028920H^2 + .031912H\hat{Y}_{15}\,, \qquad (53$$

where $\hat{Y}_{15}$ and $\hat{Y}_{16}$ were for all practical purposes identical and the multipl correlation was .791. When transformed this yielded the equation

$$\hat{Y} = -40.35 - .90624H + \frac{1339.49}{31.336 - H}\,. \qquad (54$$

As a further check the variable $1/(31.336 - H)$ was calculated, and a multi ple regression of Y on this variable and H was performed. This yielded a equation which differed from (54) only in the fourth significant figure of th regression coefficient for H. The contour lines produced by this equation ar shown in figure 11.

A series of tests was run to determine if any additional effects from X and X_2 could be estimated to exist in addition to this. These included step wise multiple regressions on all variables, including $1/(31.336 - H), H, H^2, X$ X_2, X_1^2, X_2^2, X_1X_2, and others. None indicated that any significant effect existed other than that predicted by equation (54) above. The largest R ob tained was .820, and this was at the sixth step of the stepwise multiple regres

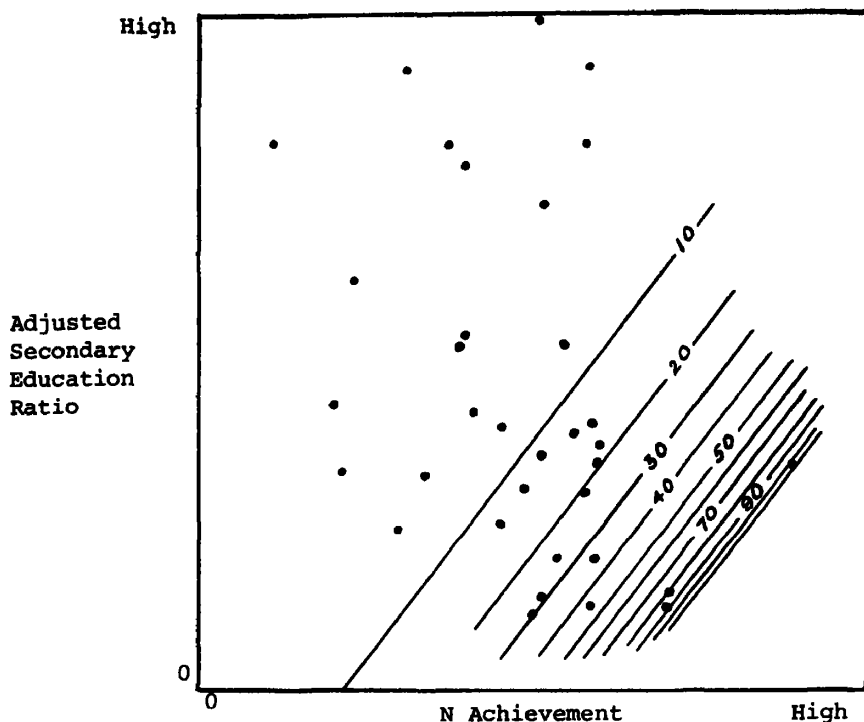



FIG. 11.—Final contour diagram showing values of Internal War predicted by the analysis for asymmetric equilibrium model.

sion, during which $1/(31.336 - H)$ was the only variable to show consistent significance. It was therefore concluded that figure 11 and equation (54) are the clearest and simplest expressions of the total effect of X_1 and X_2 on Y. The value of Y predicted by equation (54) may be used as a single variable expressing this effect.

In the earlier section on curvilinear relationships, it was said that if the coefficient b in the term $b/(k + X_1)$ was 0 the iterative method suggested was unstable. When some real data which appeared to fit the model were analyzed by this method, they failed to show a clear convergence for all values of θ used. The best solution obtained showed a surface very close to a symmetric model, and the first step, using H and H^2 as predictors, gave a multiple correlation close to those in all the subsequent steps. A subsequent stepwise regression using the best estimate of θ obtained again supported the conclusion that the data was best fitted by a symmetric model.

Significance Testing

The same difficulties met earlier with the symmetric equilibrium model apply to this model but with some extra ones. The coefficient of determination

R^2_A for the asymmetric surface uses five degrees of freedom, one for each constant in equation (53) and one for θ. This can be transformed into an equation using seven variables including two reflective interaction terms, but this is not a useful equation, and there is no point in testing for whether it is statistically independent of the asymmetric equilibrium model.[11]

Placing θ equal to 0 and 90 degrees reduces the equation to two versions in $[X_1, 1/(X_1 + c)]$ and $[X_2, 1/(X_2 + c)]$, respectively. While it seems doubtful that the value of c obtained in the analysis is necessarily the best for all, it is not possible to estimate better ones. Using the value of θ and c obtained in the iterative solution, we can therefore calculate $1/(H + c)$, $1/(X_1 + c)$, and $1/(X_2 + c)$ and place these, together with X_1 and X_2, in a multiple regression for a rough test of the null hypotheses that $\theta = 90$ or 0 degrees. As before, we can either treat the partial correlation coefficient conventionally (a liberal test) or assume that it uses three degrees of freedom and treat it as if it were a multiple partial. The three degrees of freedom are used by the estimation of θ, c, and the regression coefficient. We can perform similar tests against the same subsets of variables as for the symmetric model, remembering that $1/(H + c)$ must, in the conservative test, be treated as using three degrees of freedom.

A SUBSTANTIVE EXAMPLE OF THEORY BUILDING

One important feature of the models presented is that they require close attention to the substantive theory in question in order to determine which mode of analysis is required. This attention will sometimes show that the theory is deficient and often in the same way, namely, that the prediction refers only to a limited portion of the total bivariate space. It is very often the case that theoretical formulations have been derived in contexts which are relatively narrow and do not specify what is likely to happen beyond these. Merton's (1957) deviance paradigm, for instance, does not consider the case of the individual not socialized into cultural goals, yet, if we are to perform empirical research on his propositions, we must measure this socialization and consider such individuals (e.g., Gould 1969). If we assume that both variables are measured on interval or ratio scales, it is necessary to provide predictions over the entire bivariate space for the two independent variables. Not until this is done with reasonable precision is it possible to use the full range of both variables for analysis or to choose the model which is most appropriate.

Research reported by Chirot and Ragin (1975) predicting the occurrence of peasant rebellion provides a useful example of the way in which the set of models may be used for better specification of the theory and consequently better analysis. In that part of their analysis which is relevant here, they use some generalizations from Hobsbawm (1959), Tilly (1967), Moore (1967),

and Wolf (1969) to predict the degree to which peasant rebellion occurred in Rumanian counties in 1907. Their prediction depends on the two variables, "commercialization of agriculture" and "traditionalism," the first measured operationally by the percentage of arable land in the county devoted to the then new cash crop, wheat, and the second by the percentage of the rural population who were illiterate. Two dependent variables are used, the volume and the spread of violence.

The various observations agree that "sudden creation of a large-scale market in land has profoundly unsettling effects on peasants long used to considering land as community or family property whose utilization ought to be subject to many social ties and requirements" (p. 429). Chirot and Ragin quote Tilly (1967) to the effect that revolt against the Jacobin reforms of the French Revolution took place in the Vendee, where such unsettling circumstances existed but not in areas which "had long been engaged in commercial agriculture and had well-established commercial towns" (p. 428). Chirot and Ragin themselves state the proposition that "a highly traditional peasantry in a non-market economy will not be more prone to rebellion than a somewhat traditional peasantry in a somewhat commercialized economy. It is the combined effects of traditionalism and of market forces that produces the potential for rebellion. In other words, the multiplicative interaction effects must be taken into account in order to test the theory" (p. 430). They then proceed to use the two variables with their product as a single interaction term, thus adopting what has here been termed a conduciveness model.

In order to consider whether this is appropriate, four models should be considered, including the relative excess model. This may seem inappropriate as Chirot and Ragin are clearly predicting high levels of revolt when both commercialization and traditionalism are high. Reversing the scale of traditionalism and calling it modernism, however, could lead to formulation of the proposition that higher levels of revolt occur as the level of commercialization rises relative to that of modernism. This seems eminently reasonable, given that increasingly low levels are predicted in the opposite circumstance. The observations cited appear to be silent on this point. If a symmetric equilibrium model is to be judged appropriate, two features should be sought. The first is that a diagonal in the bivariate space should be predicted along which levels of revolt should be uniformly low. While this is not entirely clear, it is probable that this is what Chirot and Ragin meant in their comparison of a highly traditional peasantry in a nonmarket economy with the somewhat traditional peasantry in a somewhat commercialized economy. It can at least be inferred that theory predicts no revolt among the first of these, matching Tilly's observation of no revolt among the nontraditional peasantry in a commercialized economy.

The second feature of the symmetric equilibrium model is that higher lev-

els of revolt should be predicted when both commercialization and traditionalism are low (i.e., modernism is high). Chirot and Ragin do not discuss this. Wolf (1969) makes reference to "the new literati" operating "in a communication field vastly larger than that of the past, and full of new learning which suggests powerful visions not dreamed of in the inherited ideology" (p. 288), but only in the context of nontraditional commercialization. In this setting "we witness . . . a fusion between the 'rootless' intellectuals and their rural supporters" (p. 289). It is more probable that the combination of recent trends toward high literacy and low commercialization would be likely to produce widespread rank disequilibrium in the manner described by Galtung (1964) and, thus, violent behavior. But it is also likely to result in migration to more commercialized areas, reducing the volume of violence but possibly spreading it to other areas. There are, therefore, grounds for expecting both the amount and spread of violence to be higher in this situation than along the diagonal; but this does not mean that the effect would be symmetric, and, thus, the asymmetric model is suggested.

The conduciveness model implicitly adopted by Chirot and Ragin does not provide in any way for the prediction of equal levels along a diagonal and is not appropriate, but another part of their theory does fit this model. Citing Wolf (1969, p. 277 ff.) and Moore (1967, p. 474), they adopt the proposition that, given the existence of high traditionalism and commercialization, middle peasants are more likely to revolt than either the poor and landless or the wealthy. They are both more likely to be threatened by these conditions and, having greater resources and autonomy, are more likely to react to them. Increased numbers of middle peasants, therefore, may be regarded as conducive to the channeling of unsettling forces into violent action.

In the absence of unsettling conditions this should have no effect. As Chirot and Ragin used this variable in a strictly additive sense, their test (with negative results) was somewhat inadequate. Given the above analysis, we are led to suppose that a second-order interaction is needed in which Chirot and Ragin's middle-peasants variable interacts in a conduciveness model with the interaction of commercialization and traditionalism, the latter pair in an asymmetric equilibrium model. But this means that the multiplicative interaction should also be predicted for the opposite conditions; that is, that higher proportions of middle peasants should also be conducive to revolt under conditions of high literacy and low commercialization. This is not clear. According to Galtung, overeducation relative to work opportunities at several levels should nurture both leaders and followers in revolt. High proportions of middle peasants may contribute to this, and the argument that they have resources and relative autonomy holds. It does not appear unreasonable to adopt the second-order interaction, though the arguments for it on both sides of the inflection line are by no means of equivalent weight.

It is necessary in the analysis to assume that diffusion is not taking place (Naroll 1970; De Silva 1966). This is a separate methodological issue but particularly acute when "spread of violence" is one of the dependent variables involved. Chirot and Ragin's data were reanalyzed, omitting one variable. They concluded that the variable had no significant effect and it is not important for this example except in a negative sense. Had it been included, it would have prevented identification of the constants by introducing two more regression coefficients and only one more constant. The model used was

$$(\hat{V} \text{ or } \hat{S}) = a + bH + \frac{c}{H + k} + dM + eMH + \frac{fM}{H + k}, \qquad (55)$$

where V = violence, S = spread of violence, H = distance from a specific diagonal through the origin of (C, T), C = commercialization, T = traditionalism, M = middle-peasant strength, and small letters designate unknown constants. All variables are as operationalized by Chirot and Ragin (1975). This equation yields, for purposes of analysis, the following independent variables in the iterative series of multiple regressions: H, H^2, M, MH, MH^2, and ($H\hat{V}$ or $H\hat{S}$).

The omission of Chirot and Ragin's other independent variable is important at this stage. The entry of the variables listed above into a multiple regression yields seven coefficients including the intercept. In equation (55) above there are seven constants which require estimation, and the model is soluble. Had an extra independent variable been included in equation (55), one extra constant would be required; but two extra coefficients would be derived from the multiple regression, and the constants would be overidentified.

In the analysis of V the following two unstandardized equations were obtained. The first is an asymmetric model, the second a symmetric one:

$$\hat{V} = -.450 - .00891H^{**} + \frac{113.8^{**}}{73.7 - H} - 5.99M^{**} - .057MH^{**} + \frac{454.4M^{**}}{73.7 - H} \quad (R^2 = .86) \qquad (56)$$

and

$$\hat{V} = 1.16 + .0244H + .000611H^2 + .182M^{**} + .0250MH^{**} + .000802MH^{2**} \quad (R^2 = .86), \qquad (57)$$

where $^{**}t > 2.5$.

Both analyses increase the coefficient of determination by about .07 above Chirot and Ragin's analysis, which included one extra variable. But clearly the asymmetric model has no advantage over the symmetric one. No difficulties were encountered in obtaining convergence in the iteration, which occurred very rapidly, but the variable $H\hat{V}$, unlike all other variables, did

not achieve significance.[23] The angle of the diagonal with the C-axis which provided the largest R was 136 degrees. This is remarkably close to symmetry with the two variables C and T, both of which are percentages, but it is likely that no significance should be attached to it.

Chirot and Ragin concluded that M had no effect. The present analysis reverses this conclusion, with the reservation that its distribution is distinctly skewed and the significance test questionable. But in each model the sign of the interaction terms involving M is as predicted, indicating that higher values of M are in this sample conducive to greater effects of H.

It appears that there is nothing to choose between the two models, symmetric and asymmetric equilibrium. A partial explanation for this lies in the fact that the data are distributed asymmetrically. Partial differentiation of equations (56) and (57) with respect to H and equating to zero provides the value of H for which minimum V is predicted:

$$H = 73.7 \pm \sqrt{\frac{113.8 + 454.4M}{.00891 + .057M}} \qquad \begin{matrix} M = 4\%, & H = -16.5 \\ M = 70\%, & H = -15.5 \end{matrix} \qquad (58)$$

and

$$H = -\frac{.0244 + .0250M}{2(.000611 + .000802M)} \qquad \begin{matrix} M = 4\%, & H = -16.3 \\ M = 70\%, & H = -15.6\,. \end{matrix} \qquad (59)$$

Substitution of values of $M = (4, 70)$ at extreme ends of its range in this data shows, first, that M does not change the estimated value of H for minimum $\hat{V}$ very much. This is as it should be, as M should not have such an effect. But it also shows that this value of H lies considerably outside the range of the data. Among the 32 counties the minimum value of H is 59.6; they therefore lie entirely on one side of the inflection line. Needless to say, these values of H for minimum $\hat{V}$ are so far outside the range of H that they represent unreliable extrapolations. It is surprising that the two models yield such similar estimates.

Analysis of S produced results no more successful than those of Chirot and Ragin (1975). In the series of iterative multiple regressions for the assymmetric equilibrium model, an angle of 120 degrees with the C axis produced a maximum R, but in no case did convergence occur. The symmetric equilibrium model for this angle produced a maximum R^2 of .35 compared with Chirot and Ragin's .388, the latter being helped considerably by the variable omitted in the present analysis, which was the only one to achieve significance in theirs. But, as in their analysis, none of the variables achieved significance, at least in the multiple regression. Uncontrolled, H had a correla-

[23] When the final result was checked by inserting the value of k in equation (52) and performing the multiple regression direct, a singular matrix was obtained. This was avoided by expressing H, M, and $1/(H + k)$ as deviations from their means before obtaining the interaction terms MH and $M/(H + k)$. The t-coefficients reported above were obtained in this way.

tion of .53 with S, but nothing beyond this appeared to have any effect. This could indicate that the relative excess model, applied to commercialization and modernism, finds some support. Without further indication as to why the volume and spread of violence should require different models, it is best merely to record the result for more thought. It is, of course, possible that diffusion from adjacent counties confounds the result more for this variable and also that it is unreliable.

In another recent example, the theory is adequately stated, and precisely the same mode of analysis is indicated. House and Harkins (1975) extended a test of status inconsistency theory by suggesting that some individual characteristics such as age and motivational factors may be conducive to status inconsistency effects on, for instance, psychological strain. In accordance with the conduciveness model, they used a simple multiplicative interaction between this characteristic and the variables used to operationalize status inconsistency. It was in the latter analysis that their procedure was less adequate. Using measures of occupational (O) and educational (E) status, they explicitly state the hypothesis that both kinds of inconsistency will be stressful. This suggests that something close to a symmetric equilibrium model might be appropriate. In order to achieve this, they first define the position of the diagonal along which the two statuses are thereafter assumed to be consistent. To do this they collapse both status variables into four ordered categories which are treated as interval scales each with only four possible values. The cross-tabulation of the two variables, therefore, has 16 cells, of which the 10 down the diagonal are treated as consistent, leaving three cells in each corner as inconsistencies of opposite degree. Membership in each of these sets of three cells is coded by means of a dummy variable, named X and Y, respectively. The equation predictive of the inconsistency effects is then given as

$$D = a + b_1O + b_2E + b_3X + b_4Y \,. \tag{60}$$

Such an equation imposes distinct constraints on the possible forms taken by the relationship. Figure 12 illustrates the general form, ignoring the fact that the two variables are discontinuous. The analysis bars the three plane surfaces from being anything but parallel, and this may introduce a distinct but unnecessary asymmetry. In the diagram it has been assumed that the true diagonal of lowest $\hat{D}$ is offset toward the high E/low O corner and that the coefficients b_1 and b_2 have adjusted to this by inclining the planes upward toward the low E/high O corner. This means that, as the high predicted values in the latter corner are already partly taken care of, the step in this corner produced by the dummy variable X is not as great as that produced by Y in the opposite corner, which has the reverse situation. The implication is that errors in estimating the position of the correct diagonal may affect the distribution between X and Y of explained variance in D.

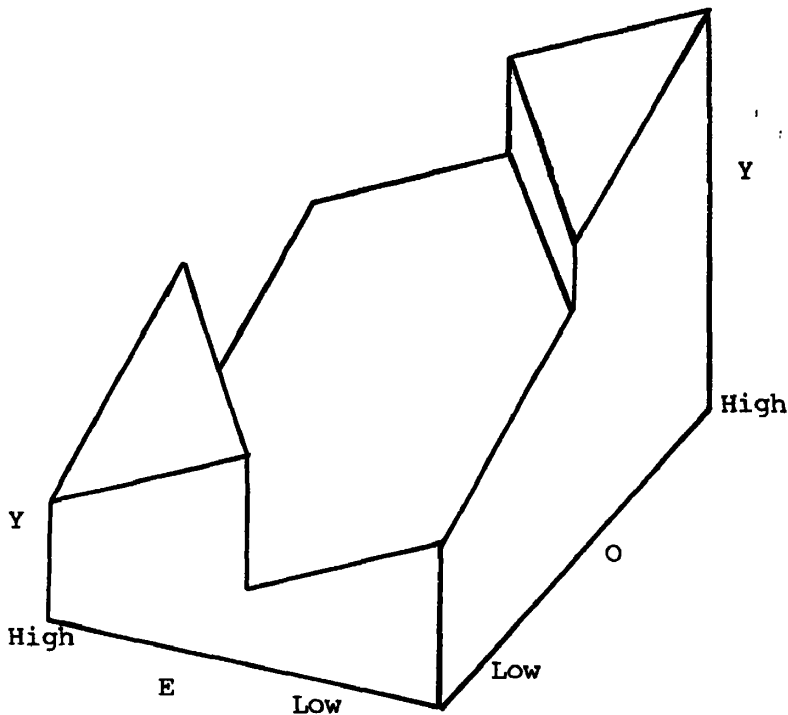



FIG. 12. —Illustration of the House/Harkins model in which disequilibrium in E and O is represented by two dummy variables under control for additive effects of E and O. E and O are assumed to be measured on interval scales.

It is not clear if House and Harkins's result is of this kind, though it seems unlikely. The scales of variables O and E are not clear; thus, their unstandardized regression coefficients are not interpretable.

While their method is relatively crude, it does provide for control for additive effects, and it also distinguishes the two kinds of inconsistency by the use of two dummy variables. In their theoretical introduction, House and Harkins make it clear that the processes leading to psychological stress are not identical for both kinds of inconsistency, and their analysis makes it in principle possible to separate the two statistically, though with the qualifications mentioned. The models proposed above do not do this.

CONCLUSION

Given sufficiently precisely stated models, it is possible to test a variety of different forms of theory which cannot and should not be tested by the conventional additive model of multiple regression with singular propositions. In each case statistical interaction is required, but each type of theory

initially requires different combinations of interaction terms which can be derived from the model.

While these forms of analysis do not supersede the singular additive model, which will provide the most parsimonious model for much theory, they suggest that careful consideration should be given to other models before commencing analysis and, indeed, before collecting the data. Data fitting any of the models are capable of producing unexpected and unexplainable results in singular additive multiple regressions when ex post facto propositions in singular form are attempted. In all cases, it is possible to express the predicted effect by use of a single interaction term. This term varies from analysis to analysis, depending on the form of relationship predicted and/or found; for example, in the case of symmetric disequilibrium, the term is of the form $D^2 = (\sin\theta\, X_1 - \cos\theta\, X_2 + k)^2$.

Empirical examples show that the modes of analysis have practical utility and are not merely simple mathematical exercises. They appear to have particular relevance for theories involving dysfunction, disequilibrium, and conflict, but may be of broader relevance, particularly the conduciveness model. An appropriate use of the different models is the careful examination of theoretical propositions to ensure that they have not been implicitly and unconsciously stated in singular additive form when other forms appropriate to other models would be either possible or preferable.

APPENDIX

Simple Multiplicative Interaction: The Effect of Changes in Origin of X_1 and X_2

Any equation in (X_1, X_2, $X_3 = X_1X_2$) can be transformed into one in (X'_1, X'_2, $X'_3 = X'_1X'_2$) where $X'_1 = X_1 + c$ and $X'_2 = X_2 + k$; that is, the two equations

$$\hat{Y} = a + b_1X_1 + b_2X_2 + b_3X_3 \qquad (61)$$

and

$$\hat{Y} = (a - b_1c - b_2k + b_3ck) + (b_1 - b_3k)X'_1 + (b_2 - b_3c)X'_2 + b_3X'_3 \qquad (62)$$

express the same surface in the three dimensions (Y, X_1, X_2), this space being identical with that for (Y, X'_1, X'_2) but for a shift in the axes of X_1 and X_2. We assume that the first equation is the result of a multiple regression, which maximizes the proportion of variance in Y explained. Any new multiple regression of Y on (X'_1, X'_2, X'_3) which yielded an $R^2_{y(123)}$ larger than that for the first equation would provide an equation which could be transformed into one on (X_1, X_2, X_3) without change in this proportion of variance explained. By definition this is impossible. Similarly, multiple regression of Y on (X_1, X_2, X_3) cannot yield a multiple correlation higher than that of

Y on (X'_1, X'_2, X'_3). The two multiple regressions must therefore yield identical multiple correlations.

The multiple regression of Y on (X_1, X_2, X_3) produces a unique curvilinear surface in the trivariate space (Y, X_1, X_2) specific to this multiple correlation. The only difference between this trivariate space and that for (Y, X'_1, X'_2) lies in the difference of the origins of (X_1, X_2). The multiple regression equation of Y on (X'_1, X'_2, X'_3), when described as a curvilinear surface in the space (Y, X'_1, X'_2), should therefore describe a surface identical with that described by the multiple regression on (X_1, X_2, X_3) as it produces the same multiple correlation in what is essentially the same space. In the proof which follows, coefficients obtained by the multiple regression of Y on (X'_1, X'_2, X'_3) will be distinguished by the use of primes. It will be shown that the multiple regression equations are equivalent in that each can be transformed into the other. It will also be shown that while the partial coefficients for the three independent variables do not remain constant with changes in origin of X_1 and X_2, the partial correlation and unstandardized partial regression coefficients for the interaction term are constant.

First, as $R^2_{y(12)}$ will remain unchanged by any change in the origins of X_1 and X_2, it can be seen that $r^2_{y3.12}$ will also remain unchanged as

$$\begin{aligned} r'^2_{y3.12} &= \frac{R'^2_{y(123)} - R'^2_{y(12)}}{1 - R'^2_{y(12)}} \\ &= \frac{R^2_{y(123)} - R^2_{y(12)}}{1 - R^2_{y(12)}} \\ &= r^2_{y3.12}\,. \end{aligned} \tag{63}$$

It is possible by a tedious process of substitution to show that the two partial correlations are of the same sign. This is omitted here, but the basic equations, upon which other derivations also depend, are the following:

$$\begin{aligned} r'_{y3} &= \frac{\Sigma y X'_3}{N s_y s'_3} = \frac{\Sigma y (X_1 + c)(X_2 + k)}{N s_y s'_3} \\ &= \frac{r_{y3} s_3 + c r_{y2} s_2 + k r_{y1} s_1}{s'_3}, \end{aligned} \tag{64}$$

$$\begin{aligned} s'^2_3 &= \frac{1}{N} \sum \left[(X_1 + c)(X_2 + k) - \frac{\Sigma (X_1 + c)(X_2 + k)}{N} \right]^2 \\ &= s_3^2 + c^2 s_2^2 + k^2 s_1^2 + 2c r_{23} s_2 s_3 + 2k r_{13} s_1 s_3 + 2ck r_{12} s_1 s_2\,, \end{aligned} \tag{65}$$

$$r'_{31} = \frac{r_{31} s_3 + c r_{12} s_2 + k s_1}{s'_3}, \tag{66}$$

and

$$r'_{32} = \frac{r_{32} s_3 + c s_2 + k r_{12} s_1}{s'_3}, \tag{67}$$

where Y_1 signifies the deviation from the mean and s'_3 is the standard deviation of X'_3, etc. From these we obtain

$$R'^2_{3(12)} = r'^2_{31} + r'^2_{31\,1}(1 - r'^2_{31}) \tag{68}$$
$$= r'^2_{31} + \frac{(r'_{32} - r'_{31}r_{12})^2}{1 - r^2_{12}},$$

giving

$$1 - R'^2_{3(12)} = \frac{s_3^2}{s'^2_3}(1 - R^2_{3(12)}) . \tag{69}$$

Consequently, as

$$\beta'_{y3.12} = r'_{y3.12}\sqrt{\frac{1 - R'^2_{y(12)}}{1 - R'^2_{3(12)}}},$$

we obtain

$$\beta'_{y3.12} = r_{y3.12}\sqrt{\frac{1 - R^2_{y(12)}}{\frac{s_3^2}{s'^2_3}(1 - R^2_{3(12)})}} \tag{70}$$
$$= \frac{s'_3}{s_3}\beta_{y3.12}$$

and

$$b'_{y3.12} = \beta'_{y3.12}\frac{s_y}{s'_3} \tag{71}$$
$$= b_{y3.12} .$$

The coefficients $r_{y3.12}$ and $b_{y3.12}$ are therefore unchanged, but $\beta_{y3.12}$ is changed as the standard deviation of X'_3 differs from that of X_3. To obtain coefficients for X_1, we note first that, as a general property of partial regression coefficients, $\beta'_{y1.23} = \beta'_{y1.2} - \beta'_{y3.12}\beta'_{31.2}$, and as $\beta'_{y1.2} = \beta_{y1.2}$ and

$$\beta'_{31.2} = \frac{r'_{31} - r'_{32}r'_{12}}{1 - r'^2_{12}} \tag{72}$$
$$= \frac{s_3}{s'_3}\beta_{31.2} + k\frac{s_1}{s'_3},$$

we obtain by substitution:

$$\beta'_{y1.23} = \beta_{y1.23} - k\frac{s_1}{s_3}\beta_{y3.12} \tag{73}$$

and hence

$$b'_{y1.23} = b_{y1.23} - kb_{y3.12} . \tag{74}$$

The derivation of the formula for $r'_{y1.23}$ is tedious and does not appear to yield a simple formula. As it will later become clear that it changes in value with changes in the origins of X_2, no derivation or result is given here. The

values of $\beta'_{y2.31}$ and $b'_{y2.13}$ follow similarly:

$$\beta'_{y2.13} = \beta_{y2.13} - c\frac{s_2}{s_3}\beta_{y3.12} \tag{75}$$

and

$$b'_{y2.13} = b_{y2.13} - cb_{y3.12}\,. \tag{76}$$

The constant term in the regression equation, $a'_{y.123}$, can now be obtained:

$$\begin{aligned} a'_{y.123} &= \bar{Y} - b'_{y1.23}\bar{X}'_1 - b'_{y2.13}\bar{X}'_2 - b'_{y3.12}\bar{X}'_3 \\ &= \bar{Y} - (b_{y1.23} - k\,b_{y3.12})(\bar{X}_1 + c) - (b_{y2.13} - c\,b_{y3.12})(\bar{X}_2 + k) \\ &\qquad - b_{y3.12}\frac{\Sigma(X_1 + c)(X_2 + k)}{N} \\ &= a_{y.123} - cb_{y1.23} - kb_{y2.13} + ckb_{y3.12}\,. \end{aligned} \tag{77}$$

The unstandardized multiple regression equation of Y on (X'_1, X'_2, X'_3) can now be stated and transformed into an equation in terms of (X_1, X_2, X_3):

$$\begin{aligned} \hat{Y}' &= a'_{y.123} + b'_{y1.23}X'_1 + b'_{y2.13}X'_2 + b'_{y3.12}X'_3 \\ &= a_{y.123} - cb_{y1.23} - k\,b_{y2.13} + ck\,b_{y3.12} + (b_{y1.23} - kb_{y3.12})(X_1 + c) \\ &\qquad + (b_{y2.13} - cb_{y3.12})(X_2 + k) + b_{y3.12}(X_1X_2 + cX_2 + kX_1 + ck) \\ &= a_{y.123} + b_{y1.23}X_1 + b_{y2.13}X_2 + b_{y3.12}X_3\,. \end{aligned} \tag{78}$$

The two multiple regressions are therefore equivalent. It is now possible to see that if

$$c = \frac{b_{y2.13}}{b_{y3.12}}, \tag{79}$$

then $b'_{y2.13} = b_{y2.13} - cb_{y3.12} = 0$, and that if

$$k = \frac{b_{y1.23}}{b_{y3.12}}, \tag{80}$$

then $b'_{y1.23} = b_{y1.23} - kb_{y3.12} = 0$.

Given that a partial regression coefficient is zero, the partial correlation coefficient must also be zero. This shows that partial correlation coefficients for both X_1 and X_2 change with changes in the origin of the other variable. It should be noted that any change in origin of either variable affects values of the three coefficients only for the other variable. The unstandardized partial regression coefficient and the partial correlation for the interaction term are not affected, but the standardized partial regression coefficient changes due to change in the standard deviation of this term.

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Net Worth as an Aspect of Status[1]

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While sociologists have recognized the importance of wealth for analysis of political power, they have given little attention to wealth as a measure of economic status. Yet from both a sociological and an economic point of view, wealth is an important determinant of status and life chances, especially at the end of the life cycle. In this paper we discuss the role of net worth as a component of status, and, using data from the National Longitudinal Studies of Labor Force Participation, we estimate a status-attainment model for net worth. Net worth includes savings, home equity, business assets, and real estate holdings. We find that (*a*) the effects of family background are transmitted via education; (*b*) the effect of education is asymptotic rather than linear; (*c*) single and divorced persons possess substantially fewer assets, net of other characteristics, than married persons; and (*d*) net of all other variables, earnings have a substantial effect on net worth. The effects of family background and socioeconomic attainments on net worth suggest that expanded definitions of status may yield more understanding of the stratification system.

Recent research on social stratification in the United States has moved in two broad directions. One group of researchers (e.g., Mills 1956; Kolko 1962; Lundberg 1968; Domhoff 1971) has been concerned with the distribution of wealth[2] and power in American society. Whether writing from a Marxist or from a more eclectic perspective, these authors have called attention to the

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[2] Sociological analyses of wealth have focused on physical (e.g., land) and financial assets. More recent work has included the concept of human capital. In this preliminary discussion we use "wealth" in the traditional manner. Later we define "net worth" as physical and financial assets and "wealth" as net worth plus human capital.

concentration of extreme amounts of wealth in the hands of a few and the implications of this concentration for political, social, and economic power. Researchers operating in this tradition have paid little attention to what might be called "personal assets," that is, the distribution of forms of wealth such as home equity, small proprietorships, and savings accounts. Since they are concerned with class distinctions and studies of the capitalist class, they are not concerned with the relatively minor assets possessed by the vast middle class.

A second group of researchers has been concerned with social status and mobility. While study of these issues can be traced to the beginnings of empirical work in American sociology, most of the research dates from the introduction of the status-attainment perspective (Blau and Duncan 1967). Work done in this perspective has concentrated on education, occupation, and, to a limited extent, income. Status-attainment researchers have not been concerned with wealth; indeed, neither the original Occupational Change in a Generation Survey (OCG) nor its replication obtained data on personal assets (Featherman and Hauser 1975). Again, the logic of this decision is easy to understand; the OCG studies were concerned primarily with social status rather than class distinctions. Wealth, while it might be important for the economic elite, was of marginal interest in a survey designed to measure occupational mobility and the determinants of occupation and income.

In this paper we wish to argue that net worth (i.e., assets minus debts) is an important aspect of economic and social status, particularly for the elderly. While net worth is certainly a measure of class, it is also a component of status whether defined as life chances, life-style, or level of consumption. The vast majority of Americans over 50 have assets of less than $100,000; yet even relatively small amounts of wealth can affect life-style in important ways. Miller and Roby (1970, pp. 66–67), in their analysis of poverty, make this point quite convincingly:

> Current income is an inadequate indicator of the economic position of the family. First, it does not provide an adequate basis for comparing poor and nonpoor groups since some of the income of the nonpoor is received in ways that are purposely designed to appear as nonincome in order to reduce taxation (for example, capital gains). Second, current income inadequately reflects future command over resources. Savings and pension accumulations are important in the future picture. Further, they affect present satisfactions by providing confidence about the future. . . . Third, past expenditures affect present well being, as in the case of household furnishings. Fourth, income does not always adequately reflect the character of housing. Fifth, the aged with low incomes (and others who temporarily have low incomes) may have nonpoverty levels of living because of the use of their accumulated assets.

Miller and Roby's reference to the aged reflects a growing concern in American society with the economic status of the elderly. Given the inevitable changes in the age structure which will occur in the next quarter century, this concern can only increase. Leaving the labor force at retirement results in a large reduction in income, yet, as Streib (1976) notes, many retirees suffer no significant loss in standard of living. Part of the income reduction associated with retirement is offset by transfer payments not counted as income (e.g., food stamps and Medicare) and another portion of the decline may be covered by gifts from children and other relatives, but one's standard of living in retirement is also determined by the prior accumulation of assets (Weisbrod and Hansen 1968; Moon 1975). As Miller and Roby suggest, the ownership of a house and the concomitant reduction in monthly housing costs affect both the quality of housing enjoyed in retirement and the availability of discretionary income (Kain and Quigley 1973). Ownership of a home also provides a degree of independence not enjoyed by those who must live with children or rent. Although there is comparatively little research on the determinants of social status of elderly persons, it is likely that the older person who can continue to live in a single-family home is accorded a higher status by others than are those who live in less independent situations.

Other forms of assets affect quality of life in retirement as well. Savings accounts and other investments provide added income but are also an important source of security and confidence since they provide a buffer against the effects of ill health. Even in an age of low-cost medical care for the aged, liquid assets allow the purchase of goods and services which prevent or delay declines in health or which make recovery from an illness more rapid and pleasant. Assets also permit one to borrow for travel or other forms of recreation and provide a sense of financial well-being which permits a higher level of consumption.

This discussion of the implications of net worth for status in retirement makes two important assumptions. First, status is at least partly determined by level of consumption, and second, consumption is more highly affected by net worth as age increases. In the remainder of this paper we formalize these notions and then present a model for the determinants of net worth.

NET WORTH AS A COMPONENT OF CONSUMPTION

A standard view of consumption (Friedman 1957) holds that it is a function of wealth and preferences, not income, where wealth is defined as the sum of human and nonhuman capital. The major idea behind such a view is that consumption decisions are made on the basis of long-term considerations and not on the basis of current earnings. Borrowing heavily from Friedman, we can express these ideas more formally as follows:

Consumption (C) is a function of wealth (W) and the interest rate (r),

$$C = f(W, r) . \tag{1a}$$

Wealth is the sum of current income (Y_t) plus the value of income in future years discounted by the interest rate,

$$W = Y_t + \frac{Y_{t+1}}{(1+r)} + \ldots + \frac{Y_{t+n}}{(1+r)^n} . \tag{1b}$$

Income in any one year is the return on wealth in that year where wealth is the sum of net worth (NW) plus human capital (H),

$$Y_t = r(NW_t + H_t) . \tag{1c}$$

Substituting for Y_t in the right-hand side of equation (1b), we find that wealth is a function of physical and human capital,

$$W = r(NW + H) + \frac{r(NW_{t+1} + H_{t+1})}{1+r} + \ldots + \frac{r(NW_{t+n} + H_{t+n})}{(1+r)^n} . \tag{1d}$$

Thus, substituting for W in equation (1a) we find that consumption is a function of net worth, human capital, and the interest rate (Friedman 1957, pp. 9–10),

$$C = f(NW, H, r) . \tag{1e}$$

This formulation has important implications for the definitions of wealth and status over the life cycle, an issue to which we will return.

In any one year the amount available for consumption is the sum of one's savings plus the discounted value of all of one's future income. The total sum available, of course, will not be spent in one year. Under assumptions of perfect certainty (about future income, how long one will live, etc.), the major motive recognized for not consuming all in one year is the desire to spread consumption equally over one's remaining lifetime. Under assumptions of uncertainty (Friedman 1957, p. 16), which we prefer, uncertainty about the future or plans for a period of high consumption may be motives for saving more than would otherwise be necessary. Saving to deal with uncertainty is usually in the form of physical capital (net worth), not human capital (Friedman 1957, p. 17). In this way, preferences affect the level of an individual's net worth and the potential for future consumption.[3]

Seeing consumption as a measure of status is similar to sociological notions of life chances as defining status. While such an analysis may be insensitive to the problems of the poor, for whom the need for current consumption outweighs the desire to save for future uncertainty, the analysis does point out that poverty is most accurately thought of as low wealth, not low earnings

[3] Unless one accepts uncertainty assumptions, there is little to study about differences in net worth since then only income would affect saving.

in any one year. Conversely, our notion of the status of a newly graduated physician is more closely approximated by his total wealth (i.e., human and nonhuman capital) and perhaps his consumption patterns than by his intern's salary. Rainwater (1974, p. 1) makes this point explicitly, arguing that permanent income (what we have defined as W) is very close to traditional sociological notions of social status.

Consumption theory also implies many of Miller and Roby's comments about the importance of wealth. Capital gains, even if not realized, will be taken into account in determining consumption. Large net worth, in the form of pensions or assets, provides security in the form of continued consumption if earnings disappear. Finally, for those who are old, net worth can be a very important source of income.

STATUS OVER THE LIFE CYCLE

Viewing consumption as a component of status has important sociological implications for the understanding of status over the life cycle. For example, given the definition of wealth as the sum of human and nonhuman capital, for young persons education is potentially a very important measure of socioeconomic status because as a measure of human capital it defines the potential for future income, an emphasis found implicitly in the large existing literature on inequality of educational attainment (see Sewell and Hauser 1975). In fact, at this age there is little advantage to be gained in including net worth as a measure of status, since for most persons it is nearly zero. The 1962 Federal Reserve study of family finances showed the mean net worth of families with age of head under 25 as $557, compared to $22,237 for those 45–54 and $32,511 for those 55–64 (Projector 1968, table s17).

As these data indicate, with increasing age, measures of education and earnings (human capital or return on human capital) are less adequate as single measures of status. Consideration of the economic status of the older population, setting an arbitrary age at 50, needs to include another element: net worth. Savings cumulate and are near their maximum during the latter years of labor force participation; for many persons the rate of saving increases as retirement approaches and expenses associated with children decline (Kreps 1969). The relationship between net worth and age follows from the relationship of age and yearly savings:

$$NW = S_t + S_{t-1}r + \ldots + S_{t-n}r^n \,, \qquad (2)$$

where NW = net worth; S = saving or dissaving in year t; n = year of first saving; r = rate of return.

If we assume average saving is positive, there is a correlation between net

worth and age.[4] In addition, as one nears retirement the number of years in which a return on human capital will be realized declines, and therefore wealth becomes increasingly a function of net worth. As the potential return on human capital declines, net worth becomes very important in making decisions about consumption. This makes intuitive sense; a 63-year-old will make current consumption decisions partly on the basis of expected retirement income. Although net worth may be a result of earlier deferred consumption, this does not affect its real importance as a determinant of status beginning at this point. This conceptual argument is supported by various empirical results.[5] Earned income becomes a less significant part of total income, dropping to about one-third of total individual income after age 65. Income from net worth derived from dividends, interest, rents, etc., becomes more important at this time, accounting for an estimated 25% of income (Bixby 1970).[6]

A STATUS-ATTAINMENT APPROACH

To the best of our knowledge no analysis of net worth has been attempted within the status-attainment perspective, though at least one analyst (Bowles 1972) has argued that more attention needs to be paid to social class (wealth) as an exogenous variable. However, given the theoretical importance of net worth as an aspect of the economic status of older persons, it makes a great deal of sense also to analyze the determinants of this aspect of status. To this end we develop a status-attainment model with net worth as the dependent variable.[7]

[4] We have not included a term for inheritance. There is evidence that for the vast majority of the population net worth is an attained status. Four-fifths of families report they have inherited nothing. Another 14% report inheriting less than $10,000 (Jencks et al. 1972, p. 214). A study of the economic elite (one-fifth of all families, holders of three-fifths of aggregate personal net worth) indicated that about one-seventh of aggregate wealth came from inheritance and gifts. Inheritance as a proportion of assets was constant among those with assets of less than $500,000, but only 8% of this elite sample had assets over that amount (Barlow, Brazer, and Morgan 1966).

[5] Another implication of consumption theory is the explanation of the low correlation between earnings and net worth. The correlation between log net worth and earnings, for persons over age 50, based on data to be described below, is approximately .40. In the theory of consumption, two factors will affect saving. One is amount of income, since one motive for saving is to spread consumption over the life cycle. However, a given amount of income is consistent with a wide range of preferences for deferring consumption and, therefore, with varying status late in life. That is, one may want to save a great deal for the future or only a little. This, together with the transitory nature of some income, means that the two are not necessarily highly related. Thus income may be a poor guide to consumption, potential income in old age, or other kinds of behavior. In this way, earnings and wealth are conceptually different.

[6] Bixby's (1970) figures are corrected for underreporting.

[7] As our theoretical discussion suggests, a full accounting of status involves combining all sources of potential consumption. In this paper, we limit the analysis to net worth, an

Our data are from the National Longitudinal Studies of Labor Force Participation, one of the few national studies which provides data on assets and net worth. The study is described by Parnes (1975) in some detail. Specific information on study design, response rates, follow-up procedures, etc., can be found in monographs published by the Department of Labor (Parnes 1970, 1973). Although we use four waves of data in some cases, most data are from the 1971 survey when the respondents were 50–64 years old. Response rates for the various follow-up surveys were in the vicinity of 80%. Regression analysis of response/nonresponse to the follow-ups indicates that respondents do not differ from dropouts in any significant way. Our analysis is restricted to white nonfarm males. Nonwhites were eliminated to make the analysis comparable to previous research (e.g., Jencks et al. 1972), and farm laborers were omitted due to the difficulty of defining assets and income for this group.

Even if the interest in net worth stems from its effect on the economic welfare of the elderly, there are important theoretical reasons for choosing this age group for an analysis of the process of the attainment of net worth. Old age is a period of net dissaving, and an analysis of persons already retired would not, on average, allow an analysis of highest attainment. On the other hand, analysis of persons in their fifties and early sixties allows one to measure net worth when it is presumably near its maximum.

We define net worth, using 1971 data, as follows: *net worth* = home equity + liquid savings + business assets + other real estate − other debt, where *home equity* = estimate of value of home − present value of mortgage, and *liquid savings* = savings account balance + estimate of value of stocks and bonds + value of savings bonds + other bank deposits or investments. The business assets and other real estate assets are net of the debt applicable to them.

Approximately 20% of the respondents did not report one or more components of liquid savings. In such cases we estimated the missing component using the median value of the component for persons with the same education. Our findings are parallel for "raw" and "corrected" net worth, and we will only report the latter.

Our measure of net worth does not include equity in a pension or other

important but neglected aspect of status. However, the concentration on net worth leads to an anomaly if one is concerned with total status: While net worth increases slightly with age, if (in accordance with our theoretical analysis) potential earnings decline, possible consumption may also decline with age. However, for those at the age of the respondents to the Parnes survey, potential income from pensions, social security, and continued work also has effects on potential consumption. Satisfactory resolution of this problem awaits the gathering of proper data and the development of ways of estimating these things. This is an important problem for future research. For an analysis of total income in old age and a discussion of earnings, net worth, and pensions as transmitters of earlier attainments, see Henretta and Campbell (1976).

forced saving (e.g., life insurance). This is a common and potentially serious omission in net worth measures; however, reliable data on pension payoffs are difficult to gather in a survey. Among respondents in the Parnes survey many did not know how much they would receive in pension benefits. Since the prevalence of pensions and pension amounts increases with occupational status (Kolodrubetz 1973; Kolodrubetz and Landay 1973), it is likely that this omission will lead us to underestimate the net worth of those with high-status characteristics and thus underestimate the effect of status variables on net worth.

The only available aspect of net worth which we have not included in our measure is automobile value, since most respondents were unable to estimate it. The contribution of automobiles to variation in net worth is likely to be small.

DATA QUALITY

There is evidence of underreporting of asset holdings, particularly by those with substantial net worth (Feber 1966). This problem is not confined to reports of net worth but is also found in reports of income (Barlow et al. 1966). To the degree that underreporting is correlated with true score, it will bias all estimates of effects downward. While there is random error in reports of home value, both Kish and Lansing (1954) and Robins and West (1977) show that owner reports are almost as accurate a reflection of "true value" as assessor's estimates or recent purchase price.

Though there are no doubt errors of unknown extent in the reports of net worth, we would argue that these data are of reasonably good quality. The major sources of error that we can identify—lack of inclusion of pension and underreporting—suggest that we will be underestimating the effect of status variables on net worth. There is, in addition, a possible misspecification of the model because we have no data on amount of inheritance. Bowles (1972) has criticized earlier status-attainment models for occupation and income on these grounds.

THE MODEL

The equations of table 1 represent the model we shall use to explain variation in net worth. It is a standard status-attainment model modified to include variables which are important late in the socioeconomic career. The variables in the first stage of the model are exogenous and measure the family background of the respondent. Father's education and occupation are standard measures of this kind. In the present case, detailed information on net worth, business assets, and income of the respondent's father might have allowed estimation of inheritance, but these data were not collected.

Age is included in the model because we expect it to have an effect on net worth through the mechanism explained earlier. Age also measures cohort membership, and there is possibly a cohort effect on net worth as well. Since the age range of our analysis is narrow, we expect these cohort effects to be small. However, even within this narrow age range, respondents entered the

TABLE 1

REDUCED AND FULL-FORM EQUATIONS FOR LN NET WORTH
(White, Nonfarm Males, Aged 50–64, 1971)

	1	2	3	4	5
	Dependent Variable: Ln Net Worth*				
1. Father's education	.065 (.020)	−.007 (.019)	−.013 (.018)	−.012 (.018)	−.022 (.018)
2. Father's SEI	.018 (.003)	.007 (.003)	.003 (.003)	.003 (.003)	.001 (.003)
3. Respondent's age	.001 (.014)	.027 (.014)	.018 (.013)	.020 (.014)	.031 (.013)
4. 0–4 years education†	..	−3.95 (.316)	−2.94 (.307)	−2.84 (.310)	−2.60 (.302)
5. 5–8 years education	...	−1.22 (.166)	−.751 (.163)	−.727 (.163)	−.647 (.159)
6. 9–11 years education	...	−.470 (.176)	−.293 (.169)	−.284 (.168)	−.280 (.164)
7. 13–15 years education	...	.600 (.234)	.358 (.223)	.363 (.223)	.071 (.218)
8. 16 years education	. .	.654 (.288)	−.088 (.283)	−.094 (.283)	−.452 (.277)
9. 17+ years education	. .	.484 (.295)	−.181 (.290)	−.190 (.290)	−.559 (.284)
10. Respondent's SEI	...	...	.026 (.002)	.026 (.002)	.015 (.002)
11. Divorce‡	...	...	−2.96 (.326)	−2.93 (.326)	−2.75 (.318)
12. Other marital status	.	...	−1.87 (.222)	−1.83 (.223)	−1.47 (.219)
13. Household size	..	...	−.156 (.042)	−.157 (.042)	−.169 (.041)
14. Pension	..	...	...	.241 (.116)	.112 (.114)
15. Earnings§ ($1,000)	...	...	...	...	.120 (.010)
Intercept	8.05	8.04	8.20	7.93	6.89
R^2	.033	.124	.219	.221	.264
	Standardized Coefficients‖				
Father's education	.076	.008	−.015	−.014	−.025
Father's SEI	.137	.053	.023	.024	.006
Respondent's age	.001	.040	.027	.030	.045
Respondent's SEI	..	...	.219	.216	.127
Household size	...	...	−.077	−.164	−.084
Earnings	...	...	...	...	.259

* Standard errors (not corrected for sampling efficiency) in parentheses, $N = 2{,}125$.
† Each education category is a contrast with high school graduate.
‡ Each marital status category is a contrast with married, spouse present.
§ Increment of $1,000.
‖ Standardized coefficients are not reported for dummy variables for reasons reported in n. 11 to the text.

labor force in quite different circumstances, ranging from the 1920s to the middle of the Depression. Age and cohort effects are difficult to disentangle in this case.

Various arguments can be advanced about how the intervening variables in the model ought to be arranged. We have kept the interior of the model as simple as possible, arranging the various stages to allow convenient calculation of indirect effects of interest. Clearly, respondent's education intervenes between family background and later socioeconomic attainments for most respondents in this age range. For reasons to be explained below, education is represented as a set of dummy variables. While a small number of persons may have married and a somewhat greater number entered the labor market before completing their education, the configuration of variables at stage two and three seems most reasonable. Household size reflects the effect of education and background on fertility, while the inclusion of age in the model controls for the contradictory effects of cohort fertility and children leaving home.

Both marital status and household size can be expected to have an effect on the accumulation of net worth. For most persons a major component of net worth is home ownership, and we would expect married persons to be more likely to own a home than unmarried persons. Household size, on the other hand, may have a negative effect on one's ability to save. Respondents have been classified into one of three marital-status categories: (1) currently married; (2) divorced or separated; and (3) other, including widowed, never married, and married-spouse absent.[8]

The effect of divorce represents the asset transfer that typically takes place between a man and a woman when a marriage is dissolved. However, in these data, only those who are divorced and not remarried are in the divorced category; some unknown proportion of those coded "married" have been previously divorced. Males who remarry following divorce may begin to reaccumulate assets; thus the effect of divorce on assets may be exaggerated in these data. This, combined with a lack of data on duration of marriage, widowhood, or divorce, will lead us to treat findings as suggestive but not definitive.

The fourth stage of the model contains a dummy variable for existence of a pension plan to allow for an analysis of the effects of forced savings on accumulation of net worth (Katona 1965).

The last stage includes husband and wife's earnings as a proxy for lifetime earnings. Earnings play an important part in the model. The amount one earns clearly should affect what one saves; indeed, propensity to save, given income, is the major interest of economists (Katona 1965). Inclusion of earnings in our equation allows us to ask an important question: To what extent

[8] There were too few persons in some of these categories to divide them further.

is net worth as a measure of financial status different from current earnings? The earnings variable is the average of husband and wife's salaries and other earnings for the years 1965, 1966, 1968, 1970, years for which the NLS collected detailed earning data. When earnings data for one or more years are missing, earnings are averaged on the basis of years available. About 20% of all respondents failed to report earnings data for all four years.

Several variables which other analysts have suggested might be important have been tried and rejected for this analysis because they neither increase variance explained very much nor help specify the effect of the other variables. These include city size, region, and industry (as opposed to occupation). Our negative findings for city size and region parallel those of Jencks (1973).

All of the variables in the model thus far are standard status-attainment variables, and we have relegated their description to the notes for the table of means, standard deviations, and correlation in the Appendix.

METHOD OF ESTIMATION

The dependent variable in our analysis is log net worth.[9] If the purpose of accumulating net worth is to maintain social position, we would expect a declining marginal utility of an additional dollar of net worth in attaining status. Rainwater (1974, pp. 136 ff.), for example, argues that perceived social standing increases linearly with income below about $13,000 and increases monotonically at a declining rate as income increases above this amount. If this is so, at higher status levels, greater dollar increases in net worth are necessary for equal increments to status, and persons at high status levels should be motivated to save those amounts. The semilog form is a convenient representation for such a relationship. After transformation, our model is:

$$\ln Y = \ln a + \Sigma b_i X_i + v \quad (3)$$

where a is the intercept, X_i the ith independent variable, and v the error.

As is usually the case in a study of this kind, our model-building procedure did not flow as smoothly as the preceding section might indicate. Our tests for nonlinearities and interactions indicated that it would be useful to include education in the model as a series of contrasts to the category of high school graduate. The categories contrasted to high school graduate are: zero to four years, five to eight years, nine to 11 years, 13–15 years, 16 years, and 17–20 years.

There are two methods of interpreting regression coefficients when the de-

[9] Prior to logging, it was necessary to deal with approximately 3% of the sample who reported negative assets. These persons were coded zero. Given the relatively small number of persons in this category, alternative treatment is unlikely to change the analysis.

pendent variable is logged. First, it is possible to evaluate the net effect in raw dollars of any independent variable at any value of Y. In table 2, we evaluate the net effect of each variable at the antilog of the grand mean of ln Y (the geometric mean of Y) using the following:

$$\exp(\bar{Y} + b_i) - \exp(\bar{Y}), \tag{4}$$

where $\bar{Y}$ is the mean log net worth and b_i is the effect of a given variable on Y.[10]

A second interpretation of a coefficient in the semilog form is a rate of change. If a relationship is linear in the semilog form, the antilog of the regression coefficient is the predicted proportional change in $\hat{Y}$ for a one-unit increment in X. If the actual value of b is less than about 0.10, then b is a close approximation to its antilog (Theil 1967, p. 428, 1971, p. 117). When b is larger, the antilog must be obtained in order to interpret the coefficient. In the remainder of the paper we will rely primarily on the proportional change interpretation presented in table 2.

RESULTS

A series of equations corresponding to the various stages of the model for log net worth (LNW) are presented in table 1. Categories for marital status have been entered into the equations as dummy variables coded as contrasts

TABLE 2

PROPORTIONAL AND DOLLAR EFFECTS FOR EQUATION 5, TABLE 1*

Independent Variable	Proportional Change†	Dollar Effects‡
Father's education	.978	−194
Father's SEI	1.001	+9
Respondent's age	1.031	+282
Respondent's education (years):		
0–4	.074	−8,989
5–8	.523	−4,265
9–11	.755	−2,186
13–15	1.073	+659
16	.636	−3,256
17+	.571	−3,834
Respondent's SEI	1.015	+1,449 (10 points SEI)
Divorce	.064	−8,382
Other marital status	.230	−6,895
Household size	.844	−1,392
Pension	1.118	+1,061
Earnings	1.127	+1,142 ($1,000)

* Consult table 1 for standard errors of coefficients.
† Exp (b) = proportional change in predicted Y for one unit change in X.
‡ Computed as $\exp(\bar{Y} + b_iX_i) - \exp(\bar{Y})$. $\bar{Y}$ is the mean log net worth and equals 9.10, $\exp(\bar{Y})$ = $8,955.

[10] For a similar approach, see Taubman and Wales (1974, appendix 1).

with the married, spouse-present category. Standardized coefficients for dummy variables are not reported.[11]

We will begin with the final equation (eq. 5 of table 1) and then interpret the various direct and indirect effects. Net of other variables, family background variables are of no significant effect. We will discuss the possibility of indirect effects in the following section. The effect of age, within the restricted age range, is approximately a 3% increase in assets for each year. This is consistent with our earlier discussion of the relation of age and yearly saving.

The effects of status variables, net of earnings, provide an interesting interpretation in light of the earlier discussion of consumption. Clearly, earnings should affect net worth since they allow greater saving. In these data, an increase in average family earnings of $1,000 yields a 12.7% increase in assets. However, other variables have significant effects on net worth. The most surprising results involve education. Those with zero to four years of education have very low predicted assets compared to high school graduates —about 7.5% of the latter group's assets. Net of income and all other variables, those with 13–15 or more years of education have adjusted net worth which is not significantly different from the net worth of high school graduates. These results are somewhat counter-intuitive, and we will discuss them in detail below.[12]

Net of other variables, an increase of one point in occupational status leads to a 1% increase in predicted assets. This is a sizeable effect; the standardized coefficient for SEI of respondent is .127, or about one-half of the standardized coefficient for earnings (.259).

Marital status is also important in determining net worth. Relative to

[11] The use of dummy variables requires the use of metric rather than standardized coefficients since the value of the latter is determined by the proportion in a given education category (P_i), i.e., $\beta_{yx_i} = b_{yx_i}(s_{x_i}/s_y)$. Since s_y is the same for all X_i and the standard deviation for the *i*th category of a dummy variable is just $\sqrt{(P_i)(1 - P_i)}$, the value of β_{yx_i} depends on P_i.

[12] Readers of earlier versions have suggested that our findings for education may result from a misspecification of the earnings–net worth relationship. In fact, Friedman (1957, p. 46) suggests that the proper specification of the relationship between earnings and consumption is double-log. While our dependent variable is somewhat different from consumption and its obverse, yearly saving, we experimented with the double-log formulation and various other nonlinear representations of the earnings effect but found the relationship to be linear in the semilog form. We also examined possible effects of early retirement and its interaction with education. These alternate specifications did not remove the nonlinearity in education nor did they increase variance explained.

There is an additional theoretical difficulty with the earnings measure. Earnings measures both past earnings, and therefore opportunity to accumulate net worth, and potential future earnings, which may serve to discourage present saving. It is possible that this biases the earnings coefficient. However, our age range is relatively small, and there is no significant earnings-age interaction. Based on these observations, we conclude that there is no tendency for those with higher earnings to wait longer to accumulate wealth.

those respondents who are married, persons who are divorced or have never been married or are widowed are far less well off. Though one might expect never-married males to accumulate fewer assets because they lack the motivation to provide for a family (as they lack another potential earner in the household), the effects are larger than one might expect. The cost of bachelorhood or widowhood is a 77% reduction in assets relative to the married group. The effect of divorce is even more substantial; divorced males suffer a 93% reduction in assets relative to married persons net of other variables.[13] Despite the positive effect of marriage, household size is associated with an approximately 15% reduction in assets for each household member.

Equations 1 through 4 of table 1 provide the various reduced form equations for interpreting the model. The logic of the analysis follows Alwin and Hauser (1975). As shown in equation 1, the total effects of family background are significant—a 6.5% increase in LNW for each year of father's education and about a 2% increase for each point of father's occupation. After respondent's education is included (col. 2), the remaining effect of family background measures is of no significance. Since a direct effect of family background would be most easily interpreted as a result of inheritance, the absence of such an effect makes inheritance of wealth an unlikely explanation for variation in LNW. These data suggest that the effects of parental status are inherited only through measures of "human capital."

The effects of education are partly mediated by marital status, occupational status, and earnings. Equation 3 shows that the introduction of occupational and marital status accentuates the nonlinearity described previously, and the introduction of earnings in equation 5 does not change the finding. Net of family background, education increases variance explained by roughly 9%. A good portion of this effect results from the substantially lower assets accrued by those in the lowest education category, those with zero to four years of schooling. This group is distinctly lower than other groups, a finding which extends to other aspects of the model as well. For example, they have higher divorce rates and much lower income than all other education groups.

The correlation of occupational status with LNW is .338, while its total effect on LNW is .219 (standardized value for eq. 3). Thus, about one-third of the total association is due to the effects of causally prior variables. The effect of occupational status is reduced to .127 after earnings are introduced in equation 5. Thus about 40% (.127/.338) of the effect of occupational status is direct, a third is due to the correlation of occupational status with prior variables, and the remainder is indirect via earnings.

[13] A high proportion of the asset transfer associated with divorce probably involves the value of home equity. A more complete analysis of net worth requires examination of its components. We will handle this and other similar problems (e.g., the liquidity of assets held by persons with various characteristics) in another paper.

Of the total association between earnings and LNW (.399), about 35% is due to the effect of exogenous variables. Still, earnings have a substantial direct effect, as one would expect. Net of all other variables, variance explained is increased from .221 to .264 when earnings are added to the equation; and, as indicated in table 2, an increase of $1,000 in yearly earnings increases wealth 12.7% net of other variables in the equation.

DISCUSSION

The results for education require further comment. The findings are interesting but somewhat difficult to interpret, particularly because there is some evidence that amount saved in any one year increases monotonically with education (Solmon 1975).

Equation 2 of table 1 shows that net of background variables, assets increase monotonically with education, with a slight turndown among the highest group, those with 17 or more years of education. As table 3 shows, the gross effect of education on LNW produces the same result. Further, an analysis of medians, also contained in table 3, is consistent with this slight turndown in LNW. However, an examination of the arithmetic mean shows that for this group the mean is higher but is highly skewed. The high-education group contains persons with a mixture of occupations, some of which yield high net worth and others of which do not. For example, of 139 respondents with postgraduate degrees, 13 are lawyers with median assets in the vicinity of $100,000, 16 are teachers with median assets of about $22,000, and seven are clergymen with median assets of less than $5,000. In this case, the median and mean log (geometric mean) are closer to the central tendency of the group than is the mean. The log transformation is quite strong, but it does provide a better indication of central tendency and produces residuals which conform to the zero mean and equal variance assump-

TABLE 3

DESCRIPTIVE STATISTICS ON NET WORTH BY EDUCATION CATEGORIES

	Education (Years)							
	0–4 (N=92)	5–8 (N=633)	9–11 (N=430)	12 (N=571)	13–15 (N=183)	16 (N=112)	17+ (N=104)	Total (N=2,125)
$\bar{X}$	11,054	20,436	28,862	36,231	58,924	65,354	77,812	34,482
SD	22,478	25,808	51,460	51,829	89,535	80,942	118,466	59,209
Mean of lnX	5.62	8.35	9.14	9.62	10.27	10.38	10.19	9.10
Geometric mean*	275	4,230	9,320	15,063	28,853	32,208	26,635	8,955
Median†	2	13	19	25	33	43	37	21
75th percentile	12	28	33	42	63	81	77	40
90th percentile	35	47	56	74	136	112	162	72

* The geometric mean is the nth root of the product of N values. It is also the antilog of the mean of lnX.
† Median and percentiles expressed in thousands of dollars.

tions of ordinary least squares regression. It also has the advantage of leading us to attend to aspects of the data which otherwise might have remained hidden by the extreme effect of the outliers.

Equation 5 shows that, net of earnings and other characteristics, the net worth of persons with more than 12 years of education does not differ significantly from that of high school graduates. This may have an interpretation in the nature of the skewed distribution discussed above, but a more substantive interpretation might emphasize the role of preferences in wealth. The difference between high school graduates and persons with less education may represent the greater emphasis on planning by those with more education. However, given a certain amount of savings, there may be no reason that a reasonably prudent person would not stop valuing future consumption over present consumption. Thus these findings may represent the ceiling effect of the level of savings deemed sufficient.[14]

SUMMARY AND CONCLUSIONS

We began this paper by suggesting that earnings alone are not an adequate measure of status late in life and that assets need to be included in the measure. Perhaps the best way to summarize our findings is to note that, as the earlier theoretical analysis suggested, earnings have a substantial effect on net worth, but other factors have substantial direct effects net of earnings. At the age of the members of this sample, status differences lead to a difference in prospective status for the rest of life, *net of earnings*. For example, on the verge of entering old age those with little education are more disadvantaged than one would expect from their earnings. Those who are divorced or unmarried are also disadvantaged. Those with large households do not have as much accumulated wealth; those with high occupational status are more advantaged than would be expected from their earnings. While most of these effects are in the same direction as the income effect, a finding consistent with earlier work (Henretta and Campbell 1976), it is important to stress that these are differences which would not be expected on the basis of earnings over a four-year period.[15] In this very important sense, we have learned something new about the stratification system.

[14] We have constructed the same asset measure used here using the Social Security Longitudinal Retirement History Study. While the RHS data set lacks some important variables in our model, notably the information needed to code SEI scores, parental background measures, and a four-year earnings series, the pattern of education coefficients is quite similar to those we have described for the Parnes data. It should be noted that the pattern of education coefficients can be represented by the double-log as well as by the categorical coding we have used.

[15] These results could be caused by the earnings measure's not being a good proxy for lifetime earnings. That is, the direct effects of prior variables might be zero if we had a measure of total lifetime earnings. However, the use of average four-year earnings had virtually

Our analysis suggests that over the lifetime of the individual the effects of social class cumulate, since those of higher status, net of earnings differences, accumulate greater net worth. This does not just mean greater savings from earnings; those of higher status may have more capital gains, may make better investments, and may be more future oriented as well. Overall, these findings suggest that knowing net worth tells us more about status than earnings alone.

Our findings suggest needed extensions of the concept of status and refinement of measurement procedures. Measurement of net worth in this paper is quite crude when compared to theoretical notions of what can affect status and consumption: pensions, insurance, contributions of family members, and opportunity to continue working after "normal" retirement age are a few. These things all affect status, particularly late in life, and are probably as important as net worth in late life when earnings disappear. They have been ignored in most research because they are difficult to measure. The usefulness of examining net worth suggests an important agenda for future research on status.

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no effect on other coefficients compared to the use of one year's earnings (it did, however, increase the earnings coefficient). This would lead us to conclude that our results are probably not a result of specification error.

APPENDIX

TABLE 1A

MEANS, STANDARD DEVIATIONS, CORRELATIONS

	1	2	3	4	5	6	7	8	9	10	11	$\bar{X}$	SD
1. Father's education*	...	.419	−.041	.413	.278	−.028	−.049	.004	.046	.265	.134	7.53	2.91
2. Father's occupation	...	...	.061	.412	.347	−.031	−.041	−.016	.025	.294	.169	29.15	22.64
3. Respondent's age	...	...	...	−.122	−.056	.009	.049	−.287	−.122	−.113	.006	56.31	4.31
4. Respondent's education†	...	...	...	...	.570	−.066	−.087	.034	.187	.468	.331	10.44	3.46
5. Respondent's occupation	...	...	...	...	...	−.090	−.131	−.025	.145	.530	.338	39.25	24.27
6. Divorced‡	...	...	...	...	...	...	−.051	−.162	−.067	−.092	−.195	.032	.17
7. Other marital status	...	...	...	...	...	...	...	−.182	−.120	−.202	−.193	.073	.26
8. Household size	...	...	...	...	...	...	...	...	.063	.091	−.026	2.91	1.45
9. Pension	...	...	...	...	...	...	...	...	...	.194	.136	.56	.49
10. Earnings	...	...	...	...	...	...	...	...	...	...	.399	9883	6290
11. Ln net worth	...	...	...	...	...	...	...	...	...	...	...	9.10	2.92

* Father's education is measured in years; father's and respondent's occupation are Duncan SEI scales; household size is number of persons in household; pension is a dummy for whether respondent is covered by a pension plan. Earnings are for all family members. Other variables are explained in text.

† Education is included in its continuous form in this matrix.

‡ Dummies for divorced and other marital status; in table 1, contrasts are for married-divorced and married-other marital status.

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The Sectoral Transformation of the Labor Force in Seven Industrialized Countries, 1920–1970[1]

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This paper analyzes the transformation of employment in the major industrial capitalist countries over the past 50 years. Particular attention is paid to the growth of services and their differentiation. The data are taken from 38 national censuses during the 1920–70 period. The results show three distinct patterns of the sectoral transformation of the labor force that can be identified with three regions: Western Europe, North America, and Japan. Furthermore, the internal reorganization of the service sector favored social and producer services which emerged as the most dynamic sectors of employment in all countries. Finally, the implications of this study for currently industrializing countries are discussed.

All countries, in the course of economic development, undergo changes in the composition of the labor force. These changes are reflected most prominently in both the occupational and the industry structures. Since the study of occupational changes aims directly at such aspects as status, prestige, and social stratification in general, it is not surprising to find that sociologists have been more concerned with occupational changes. On the other hand, there is a close interrelation between the occupational and industry structures of employment. In fact, a recent study of the U.S. labor force showed that during the 1960–70 decade, two-thirds of all changes in the occupational structure resulted directly from differential growth of industries (Browning and Singelmann 1975, chap. 5). An analysis of changes in the industry structure of a country thus should add an important dimension to the study of social change.

An examination of long-term changes in the industry structure of industrialized countries leads to two broad generalizations. First, agricultural employment is steadily declining, and second, all countries have experienced an increase in employment in service industries. In some countries, the proportion of agricultural employment has become so small

[1] This article is a revised version of a paper presented at the annual meetings of the Population Association of America, April 1974, New York City. I have had the benefit of comments on earlier drafts of this paper from Harley L. Browning, Richard A. Peterson, and Mayer N. Zald.

that the rate of decline is leveling off, but this is not yet the case with the rate of increase of services. A recent comparison of employment shifts in 10 industrialized countries revealed that in 1970, service employment accounted for over 50% of the total labor force in six of these countries (Sorrentino 1971).

In contrast to the extensive documentation of the decline of agricultural employment (e.g., Barkin 1970; Rostow 1960; Kuznets 1966; Schultz 1964; Thomas 1961), much less is known about the growth of service industries. Besides the classic studies by Fisher (1935) and Clark (1940), the works by Bell (1973), Fuchs (1968, 1969), Graf (1968), Greenfield (1966), and Menz (1965) are notable exceptions. But with services providing a steadily increasing share of employment opportunities, their analysis becomes important both for economic and sociological concerns.

Allan G. B. Fisher (1935) and Colin Clark (1940) were among the first to call attention to changes in the industry structure of the labor force in the course of economic development. Working with a three-sector model consisting of *primary* industries (agriculture, fishing, forestry, mining), *secondary* industries (manufacturing, construction, utilities), and *tertiary* industries (commerce, transportation, communication, services), they arrived at two basic conclusions: (1) An increase in per capita income historically leads to the movement of employment out of agriculture into secondary industries and, subsequently, to a shift from secondary to tertiary industries. (2) A high level of tertiary employment requires a high level of per capita income.

The Fisher-Clark thesis has been criticized on several grounds, two of which merit special attention. First, historical and contemporary evidence demonstrates that a decline in agricultural employment is not always accompanied by a growth in primarily secondary industries. It can result in a direct increase in tertiary employment (Oshima 1971). Second, Bauer and Yamey (1951) noted that the relationship between per capita income and the proportion of tertiary employment lacks a theoretical base because of the heterogeneity within the tertiary sector. (See the debate between Fisher [1952, 1954] and Bauer and Yamey [1951] and Triantes [1953] for a detailed discussion of this issue.)

This basic ambiguity of the tertiary sector indicates the need for a new industry-allocation scheme which would permit a higher degree of differentiation than is possible with the traditional three-sector model. Aside from Katouzian (1970) and Singer (1971), very few attempts have been made to overcome that deficiency. Thus, the purpose of this article is to analyze the sectoral transformation of the labor force in seven advanced capitalist countries during the 1920–70 period. Utilizing the most detailed

industry information that could be obtained from each national census bureau, the paper has the following objectives: (1) to demonstrate the importance of differentiation of the tertiary sector by introducing a new sector-allocation scheme; (2) to examine the changes in the industry structure of employment historically as well as cross-nationally; and (3) to identify the general pattern(s) of the sectoral transformation in the course of economic development.

THE SAMPLE AND DATA

The following seven countries were selected for this analysis: United States, Canada, England and Wales, Germany, France, Italy, and Japan. All these countries began to industrialize before the turn of the century, and today they are the major industrial capitalist nations of the world. It was possible to obtain detailed industry data going back to 1920 for all seven countries, and the sectoral transformation therefore will be examined for the 1920–70 period. All data for this study were obtained from the official national censuses of the seven countries. In contrast to the many differences among countries in the way that they classify occupations, there exists much wider agreement on the classification of industries. But in order to guarantee the highest possible degree of similarity of the industry categories among the seven countries and over time, the most detailed industry classifications were used (as given by the national census bureaus) to arrive at 37 industries that form the basis of this study. Thus, the reclassification of all 38 censuses into an identical industry-allocation scheme was performed to eliminate the possibility that the results are affected by definitional differences among the countries.

A New Industry-Allocation Scheme

Due to the inadequacy of the traditional three-sector model for this analysis, a new industry-allocation scheme was developed.[2] This schema permits a greater degree of differentiation of service industries than the three-sector model, while at the same time it preserves the simplicity of that earlier formulation. This new schema is presented in table 1, with Germany used as an example. For all countries, the same 37 detailed industries have been used to arrive at the six sectors. For reasons of clarity, the analysis has been carried out on the levels of sectors, although attention will be called to individual industries when merited. These sec-

[2] An earlier version of this industry-allocation scheme was introduced by Harley L. Browning in a sociology seminar in 1971. Subsequently, it was jointly developed further toward the present form.

TABLE 1

GERMANY: PERCENTAGE OF DISTRIBUTION OF THE LABOR FORCE BY INDUSTRY SECTORS AND INTERMEDIATE INDUSTRY GROUPS, 1925–70

Sectors and Industries	1925	1933	1950	1961	1970
Extractive:					
Agriculture	30.9	29.1	12.9	6.8	3.8
Mining	2.6	2.4	3.2	2.2	1.3
Total	33.5	31.5	16.1	9.0	5.1
Transformative:					
Construction	5.3	6.1	9.3	8.5	8.0
Food	4.3	5.1	4.6	3.1	3.8
Textiles	3.7	3.5	3.5	5.1	2.2
Metal	3.7	4.5	2.3	3.7	3.7
Machinery	2.9	3.4	3 0	5.0	4.8
Chemical	1.1	1.1	1.7	2.4	2.7
Miscellaneous manufacturing	17.3	14.0	22.0	22.3	23.0
Utilities	.6	.6	.8	1.2	.8
Total	38.9	38.3	47.3	51.3	49.0
Distributive services:					
Transportation	4.0	4.2	5.1	4.5	3.9
Communication	..	..	...	.5	...
Wholesale trade }	7.9	8.6	10.6	3.9	4.4
Retail trade }				7.5	8.6
Total	11.9	12.8	15.7	16.4	16.9
Producer services:					
Banking	.7	.6	.7	1.2	1.7
Insurance	.4	.6	.8	.7	1.0
Real estate	.0	.6	1	.3	.4
Engineering	.1	.1	.2	.4	.6
Accounting }	.5	.3	.3	1.0	.7
Miscellaneous business service }					
Legal services	.3	.6	.5	.6	.8
Total	2.1	2.7	2.5	4.2	5.1
Social services:					
Medical, health services	.4	}1.3	2.4	2.5	3.2
Hospitals	.6				
Education	1.1	1.2	1.5	2.1	3.0
Welfare, religious services	.5	.8	1.0	.9	.4
Nonprofit organizations	..	.	..	.	.4
Postal service	1.1	1.1	1.5	1.7	1.8
Government	2.1	2.2	4.1	5.3	8 6
Miscellaneous social services	.1	.2	.6	...	...
Total	6.0	6.8	11.5	12.9	17.4
Personal services:					
Domestic	4 4	4.0	3.2	1.5	.5
Hotels }	2.1	2.4	2.2	2.6	2.9
Eating, drinking places }					
Repair service	...	...	. .	...	1.1
Laundry	.2	...	. .	.6	.5
Barber, beauty shops	.4	.7	.8	.9	.9
Entertainment	.4	.5	.1	...	.4
Miscellaneous personal services	.1	.2	.6	.8	.4
Total	7.7	7.8	6.8	6.4	6.5
Grand total	100.1	99.9	99.9	100.2	100.0

NOTE.—Percentages may not add to 100 because of rounding.

tors are: extractive, transformative, distributive services, producer services, social services, and personal services. Since this scheme has been discussed at great length elsewhere (Browning and Singelmann 1975; Singelmann 1974), it needs no further elaboration here.

The Sectoral Transformation of the Labor Force

In table 2 the proportions of employment in each industry sector are given for the seven countries during the 1920–70 period. Despite the large differences in the industry structure among these countries, some general trends emerge from these data.

Extractive sector.—The employment share of extractive industries declined in all countries over the last 50 years. In 1920, the share of employment in the extractive sector accounted for over one-third of employment in five countries, and even 10 years later no countries, with the sole exception of England, had less than one-fourth of their total labor force in extractive activities. By 1970, however, these industries accounted for at maximum one-fifth of total employment, and this proportion had dropped to below 10% in England, the United States, Canada, and Germany.

The changes in the extractive sector are overwhelmingly due to the decline of agriculture; mining employs too few people to affect the sector in any significant ways. The decline in the need for agricultural labor must remain one of the greatest achievements in the economic development of industrialized countries, particularly in the case of the United States. In 1970, only 3.7% of the U.S. labor force was engaged in agriculture, yet these 2.7 million people not only provided food for 205 million U.S. Americans but for numerous other countries as well. This situation demonstrates how mechanization, innovation in fertilizers and seeds, credit opportunities, and concentration of land ownership have enabled continuous increases in productivity to take place.

Transformative sector.—In contrast to the dramatic decline of the extractive sector, transformative industries increased their labor-force share between 1920 and 1970 in all countries. But this growth was not continuous throughout the 50-year period, for the world economic crisis of 1929 and the outbreak of World War II imposed great strains on the national economies. With the end of World War II, however, the transformative sector continued to expand its share of employment, and by 1970 it accounted for one-third or more of the total labor force in all seven countries.

The relatively small size of the transformative sector in Japan, particularly prior to 1970, comes as a surprise in view of the fact that Japan

TABLE 2

PERCENTAGE DISTRIBUTION OF THE LABOR FORCE BY INDUSTRY SECTOR: SEVEN INDUSTRIALIZED COUNTRIES, 1920–70

Sectors and Countries	About 1920	About 1930	About 1940	About 1950	About 1960	About 1970
Extractive:						
United States....	28.9	25.4	21.3	14.4	8.1	4.5
Canada.........	36.9	34.4	31.7	21.6	14.7	9.1
England.........	14.2	11.8	*	8.9	6.6	4.2
Germany........	33.5	31.5	*	16.1	9.0	5.1
France..	43.6	38.3	40.2	31.9	23.0	17.0
Italy............	57.1	48.1	*	42.9	29.8	†
Japan...	56.3	50.9	46.3	50.3	34.1	20.0
Transformative:						
United States....	32.9	31.5	29.8	33.9	35.9	33.9
Canada.........	26.1	24.7	28.2	33.7	31.2	30.0
England.........	42.2	39.3	*	45.4	46.0	43.8
Germany........	38.9	38.3	*	47.3	51.3	49.0
France..........	29.7	32.8	29.6	35.2	37.7	39.3
Italy.......	24.2	29.2	*	32.0	40.0	†
Japan......... ..	19.8	19.8	24.9	21.0	28.5	34.3
Distributive services:						
United States....	18.7	19.6	20.4	22.4	21.9	22.1
Canada... . .	19.2	18.4	17.6	21.7	23.9	23.0
England...... ...	19.3	21.6	*	19.2	19.7	17.9
Germany..	11.9	12.8	*	15.7	16.4	16.9
France....... . .	14.4	13.3	15.1	14.4	16.4	15.5
Italy.........	8.6	10.1	*	10.6	13.1	†
Japan..	12.5	15.6	15.2	14.6	18.6	22.7
Producer services:						
United States..	2.8	3.2	4.6	4.8	6.6	9.3
Canada... ...	3.7	3.3	2.8	4.0	5.3	7.3
England... ..	2.6	3.1	*	3.2	4.5	5.6
Germany. .. .	2.1	2.7	*	2.5	4.2	5.1
France.... ..	1.6	2.1	1.9	2.7	3.2	5.5
Italy...	1.3	1.8	*	1.9	2.0	†
Japan...... ..	.8	.9	1.2	1.5	2.9	5.1
Social services:						
United States...	8.7	9.2	10.0	12.4	16.3	21.5
Canada...... .	7.5	8.9	9.4	11.3	15.3	21.1
England..... .	8.9	9.7	*	12.1	14.1	19.4
Germany	6.0	6.8	*	11.5	12.9	17.4
France. .. .	5.3	6.1	6.8	9.4	12.3	14.8
Italy...	4.1	5.2	*	7.9	9.4	†
Japan..	4.9	5.5	6.0	7.2	8.3	10.2
Personal services:						
United States....	8.2	11.2	14.0	12.1	11.3	8.6
Canada........	6.7	10.3	10.3	7.8	9.6	9.5
England.... ...	12.9	14.5	*	11.3	9.0	9.0
Germany..	7.7	7.8	*	6.8	6.4	6.5
France..... .	5.6	7.2	6.4	7.4	7.4	7.9
Italy...	4.6	5.6	*	4.7	5.9	†
Japan...	5.7	7.3	6.3	5.3	7.6	7.6

* The European countries did not take a census during World War II (the French census was taken in 1946).
† At the time of this writing, the data for Italy are not yet available.

is one of the leading exporting nations of the world. It is true that Japan's export expansion occurred primarily after 1960. According to Lockwood (1968, p. 595), it was only around 1958–59 that the volume of Japanese exports regained the 1934–36 level, but by 1963 it was almost twice that size. Yet in relation to gross national product, exports are much more important to Japan than to the United States or even Canada. One could have expected, therefore, that the size of the transformative industries would have been larger. (The relationship between exports and employment in the transformative sector is discussed in more detail in Singelmann [1974].)

Distributive-services sector.—Overall, the share of employment in distributive services increased in all countries between 1920 and 1970, although at various times this sector decreased in relative terms in most of the seven countries. The changes of employment in the distributive-services sectors are the product of two diverging trends: in all seven countries, transportation has been declining continuously, while trade has shown a similarly steady increase. The increase of employment in trade is mostly the result of the expansion of retail trade; wholesale trade expanded only moderately during that time period. An interesting finding of these data is the large share of employment in trade for the United States, in comparison with France or even Germany and England. France, and not the United States, could have been expected to have the largest share of employment in trade because of its many small stores which are more labor intensive than the U.S. supermarkets. Apparently, the higher emphasis on service in the United States—such as much longer hours that require two and sometimes three work shifts—leads to its large share of employment in trade.

Producer-services sector.—Although producer services make up the smallest sector of the labor force, they nevertheless have shown a very remarkable increase since 1920 in all seven countries. In relative terms, this sector did decline in some countries during the 1920s and the 1930s, but since 1940 employment in producer services has continuously expanded everywhere. The dynamic of this sector is demonstrated by the fact that its share of employment doubled from 1920 to 1970 in all seven countries; it even tripled in the United States and France and increased six times in Japan. The data in table 2 show, moreover, that the rate of growth of these services has not yet slowed down very much.

Social-services sector.—One of the main findings of this paper is the fact that social services have been the least affected by economic crises such as the Great Depression or by World War II. Social services increased in all seven countries continuously from decade to decade, without any declines at all. No other industry sector showed a similar ex-

ansion of its share of employment. It is particularly noteworthy that
iost of this increase occurred after 1940, and the rate of growth of social
:rvices has not yet slowed down. (But for the United States, Canada,
nd possibly England, further increases, if they are going to occur at all,
ill be at a slower rate of growth, since in these countries the social-
:rvices sector is already quite large. Also, the current concern over "the
scal crisis of the state" represents a countertendency to a further ex-
ansion of social services.)

The growth of employment in social services is perhaps the most im-
ortant aspect of the shift toward a service economy. These services are
irectly concerned with the social welfare of the population (even though
me encounters with governmental bureaucracies often suggest the op-
osite). Increasingly, individuals have access to social services regardless
f their ability to pay for them. The sociological implications of the
owth of services require an analysis in their own right, but one might
nsider the size of the social-services sector as an indicator of the emer-
ence of a service society.

Personal-services sector.—During the 50-year period, the relative num-
er of personal-services workers increased overall in the seven countries,
cept in the case of England. But most of this increase occurred prior to
960. During the 1960s, this sector only expanded in Germany and
aly, and these increases were very small.

To fully understand the changes of the personal-services sector, one
eeds to distinguish between domestic service and the remaining personal
:rvices. Domestic service declined between 1920 and 1970 from one of
ie largest individual industries to an insignificant size; only France had
iore than 2% of total employment in domestice service by 1970. The
her personal services, however, increased rather substantially during
iose 50 years. The most important expanding services are automobile
pair shops and lodging and eating and drinking places. The latter in
articular have expanded substantially since 1950, reflecting an increase
i disposable income and a further shift of production from the house-
old to the market economy.

atterns of the Sectoral Transformation

Iuch of the preceding discussion concerns the relative employment
ianges over time in the industry sectors, and not the respective size of
iese sectors in the seven countries. But since the sectoral transformation
f the labor force entails a structural shift from an agricultural to a
:rvice economy, this section addresses the question, How did the sectoral
ansformation take place in the seven countries? And, furthermore, did

the decline of agriculture result in similar industry structure of employment among the industrialized countries? In other words, this section discusses the patterns of sectoral transformation in the seven countries and the extent to which their industry structures have converged.

The continuous decline of agriculture in the seven countries has not led to the same growth of transformative industries (see table 2). In the United States and Canada, the transformative sector expanded to a maximal proportion of about one-third of total employment. In Canada, this maximum was reached in 1950, and since then the transformative sector has declined in relative terms. In the United States, the sector continued its expansion through 1960, but it began to decline during the 1960s.

In the European countries, the growth of the transformative sector reached a much larger proportion: about two-fifths of total employment in France and Italy, and one-half in England and Germany. Although this sector has continued its expansion during the 1960s in France, it appears that the transformative sector in Europe, too, has reached its maximum share of employment.

The Japanese experience is a very interesting case, for only recently did the transformative sector account for about one-third of total employment. Considering that Japan is one of the leading industrial nations of the world, the employment share in transformative industries is quite small. Since Japan's industrial boom came after 1950, it may be too early to speculate on the maximum size of the transformative sector. But given Japan's past ability to become a major export nation without expanding the transformative sector to the European level, it can be expected that this sector in Japan has more or less reached its maximum as well. Japan thus is one illustration for Oshima's (1971) contention that in some countries, the decline of agriculture is followed immediately by a growth of service industries, without a prior domination of secondary industries.

As a result of the agricultural decline, tertiary industries expanded throughout the 1920–70 period in the seven countries. But as was noted before, this expansion was due primarily to the growth of producer and social services; distributive and personal services, in contrast, changed much less during the 50-year period. The size of both the producer-services sector and the social-services sector is related to per capita income: among the seven countries, the greater the per capita income, the greater the proportion of employment in producer and social services (Singelmann 1974). In sum, three patterns of the sectoral transformation can be distinguished:

1. In the four European countries, the decline of agriculture resulted primarily in the growth of transformative industries. Throughout the 50-year period, these industries have remained the dominant sector for employment. Even Italy, which is the most agricultural of the four European countries, has a much larger transformative sector than the three non-European countries. Only recently have social and producer services begun to expand in the European countries, although the beginning decline of transformative industries should lead to the further expansion of these two service sectors.

2. The United States and Canada represent the second pattern of the sectoral transformation. Compared with Europe, by 1920, these two countries had already much more employment in services than in the transformative sector, and the gap has continued to widen ever since. In both countries, the decline of agriculture led to a concommitant growth of transformative and service industries, with services overtaking the transformative sector by the turn of the century. It is interesting to note that these two countries followed the same path, although Canada continued to have a much larger extractive sector than the United States. In both countries, the growth of producer and social services has reached its highest levels among the seven countries, and social services in the United States have replaced distributive services as the second largest industry sector (after transformative industries) and the largest service sector.

3. The final pattern of the sectoral transformation is represented by Japan. In this country, the decline of agriculture has led directly to the expansion of services. Although its transformative sector has grown to about one-third of total employment in recent years, it has yet to reach the dominance it did in the European countries.

DISCUSSION

This discussion has revealed that among the countries studied here, only the European ones followed the Fisher-Clark thesis of employment shifts from primary to secondary industries and, subsequently, to tertiary industries. The United States and Canada do not conform to their model, and Japan's experience has been completely different. The Japanese case has some important implications for the situation of currently industrializing nations. It demonstrates that the time period of industrialization is a crucial factor that must be taken into consideration, for it provides the conditions under which a country industrializes. The key variables here, aside from per capita income, are technology, urbanization, international migration, and the international division of labor as reflected in the system of international trade. These variables are important factors

in the process of a country's transformation from an agricultural economy to a service economy, and they help to explain the different path of the sectoral transformation in developing countries compared to the European experience. (These relationships will be examined in a separate paper.)

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Scientific Productivity, Sex, and Location in the Institution of Science[1]

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Although sex differences in scientists' careers are often explained in terms of hypothesized productivity differences, little is known about the extent or bases of such differences. Comparisons for a sample of chemists indicate that men outpublished women, but to a smaller extent than is commonly supposed. Regression analyses document sex differences in the determinants of productivity, with women's productivity more responsive than men's to prestigious postdoctoral fellowships, employment in tenure-track university positions, and collegial recognition.

Recently the status of women in science has received considerable attention (for a comprehensive review, see Zuckerman and Cole [1975]). Explanations for women's lower status vis-à-vis men often hinge on hypothesized differences in productivity (Brown 1967, p. 47; Cole and Cole 1973, p. 150; Zuckerman and Cole 1975, p. 98), but the evidence is mixed and inconclusive (Simon, Clark, and Galway 1967; Clemente 1973; Cole and Cole 1973, p. 137). Moreover, explanations proposed to account for hypothesized sex differences in productivity (Cole and Cole 1973, pp. 135–38; Zuckerman and Cole 1975, pp. 92–93) have not been tested. Using longitudinal data for doctoral chemists, this paper first assesses sex differences in several measures of productivity. Then multivariate analyses, including measures of predoctoral background, early performance, collegial recognition, and institutional location, estimate sex differences in the determinants of productivity.

DATA AND METHODS

This study is based on data for chemists who obtained their Ph.D.'s at U.S. universities between 1955 and 1961. The research population includes 229 female and 221 male chemists. Reskin (1977) gives details on sampling and measurement. The productivity measures include predoctoral publication (the sum of all articles published prior to the Ph.D. year), early productivity

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(the sum of articles published in the three-year period beginning with the Ph.D. year), and decade productivity (the sum of articles published in the chemists' ninth and tenth post-Ph.D. years). Early collegial recognition is measured by the number of citations to the chemists' early work listed in the 1961 and 1964 volumes of *Science Citation Index* (*SCI*).[2] The residuals from regressing (*a*) total number of citations received between 1961 and 1971 and (*b*) number of citations received in 1969 and 1970 on the total number of articles published through 1970 provide measures of publication quality independent of quantity.

RESULTS

Sex Differences in Productivity

Table 1 shows that on most indicators the men were slightly more productive than the women, but the differences were generally too small to attain statistical significance at the .05 level. The men significantly outpublished the women in their fifth post-Ph.D. year, and the sex difference in decade productivity was also statistically significant. Taken individually, the slightly but not significantly higher mean scores for the male sample could result from sampling error. However, the consistent pattern of small differences suggests a true but small sex difference between the populations. The sex differences increase over the first professional decade: the correlation between annual differences in the means and years since the Ph.D. is .43. Consistent with their greater output, the men averaged significantly more citations than the women over the 11-year period, but none of the differences for the measures of publication quality which were adjusted for quantity was statistically significant.

Sex Differences in the Determinants of Productivity

Separate analyses for men, women, and women who were employed continuously permit estimating the impact on women of a career interruption

[2] I standardized all of these productivity measures to adjust for underreporting by *Science Citation Index* of article and citation counts for the earlier Ph.D. cohorts. After this paper was scheduled for publication, Paul Allison called my attention to error introduced by using transformed variables in interaction terms in multiple regression. He has demonstrated that certain unstandardized and standardized coefficients are not invariant to scale transformations of the components of interaction terms expressed by products (Allison 1977). While including such a product variable does not affect the unstandardized regression coefficients for either the transformed variables or the product terms (or the standardized coefficient for the former), the coefficients for all other variables are affected. In these analyses I used several product terms that involve early publications or citations in order to test for interactions by setting and sex. To assess the bias this introduced, I replicated the analyses using unstandardized productivity measures and including Ph.D. year to control for degree-year differences. Although the exact results differed from those reported here, the general patterns were similar. Results of the analyses using the unstandardized measures are available to interested readers.

TABLE 1

MEAN SEX DIFFERENCES IN SELECTED MEASURES OF SCIENTIFIC PRODUCTIVITY

Productivity Measure	Men	Women	t-test
Predoctoral publications[a]	.85	.76	.55
Early productivity[a]	.57	− .39	.80
First post-Ph.D. year	.51	.41	1.11
Third post-Ph.D. year	.51	.46	.58
Fifth post-Ph.D. year	.74	.48	2.46*
Seventh post-Ph.D. year	.86	.91	− .16
Ninth post-Ph.D. year	.92	.72	1.32
Decade productivity[a]	.087	−.092	1.91*
Early citations[a]	.012	−.012	.26
Total citations	40.14	26.35	1.78*
Recent quality score[b]	.57	− .59	.54
Total quality score[b]	2.27	−2.39	.79
N	229	221	...

[a] Standardized by Ph.D.-degree cohort.
[b] Measures described in text.
* $P < .05$, one-tailed test of null hypothesis that male scientists' productivity is greater than that of female scientists.

and comparing similar equations (without the term for continuous employment) for the men and the continuously employed women. Table 2 shows the results of regressing decade-productivity scores on several independent variables. Overall, the variables included were more effective in explaining the men's decade productivity ($R^2 = .41$; compared to .31 for the women and .34 for the continuously employed women). The greater indeterminacy of the women's productivity mirrors the greater unpredictability of their careers; they often held extended postdoctoral fellowships (in effect, permanent nonfaculty positions; National Academy of Sciences 1969; Reskin 1976), changed jobs more frequently, and moved up and down in both position and institutional prestige without the usual compensations for such shifts (Hargens 1969). Luck—being at the right place or exposed to the right colleagues at the right time, having a supportive senior colleague, being married to the right person, or not being married—probably played a large role in whether many of the women engaged in research.

Socialization.—Neither the productivity of the chemists' sponsors nor pre-Ph.D. collaboration with the sponsor significantly affected decade productivity for either sex, but the caliber of the Ph.D. department showed a small significant effect on the men's decade productivity. For both samples of women the coefficients were negative (b's = −0.014 and −0.049) but too small to be meaningful. Caliber of the Ph.D. department may reflect predoctoral ability rather than socialization. This could explain the negative slopes for women, if personal considerations (such as part-time employment or marriage) or discrimination in admissions or financial support constrained

TABLE 2

REGRESSIONS OF DECADE PRODUCTIVITY SCORES ON SELECTED INDEPENDENT VARIABLES FOR MEN, WOMEN, CONTINUOUSLY EMPLOYED WOMEN, AND COMBINED SAMPLE OF MEN AND WOMEN

	Equation (1) (Men)		Equation (2) (Women)		Equation (3) (Continuously Employed Women)		Equation (4) (Men and Women Combined)	
Independent Variables	b	β	b	β	b	β	b	β
Ph.D. caliber	.119*	.115	−.014	−.016	−.049	−.046	.127*	.129
Postdoctoral prestige	.002	.006	.051*	.154	.065*	.176	.004	.011
University first job (men)	.452*	.144	...	...	...	...	.231**	.092
University tenure-track job (women)	...	...	2.383*	.337	2.586*	.351	...	...
Early publications	.108	.109	.030	.030	.003	.003	.109	.109
% first-authored publications	.374*	.149	−.006	−.003	.070	.028	.378*	.162
Early citations	.468*	.434	.739*	.829	1.045*	1.165	.418*	.420
University × early publications (men)	1.146*	.639	...	...	...	...	.980*	.532
Tenure-track university × publications (women)	...	...	8.381*	.346	9.086*	.360	...	...
University × early citations	−.467*	−.345	−.555*	−.605	−.798*	−.829	−.379*	−.337
Early publication × early citations	−.191*	−.508	−.072	−.114	−.140*	−.230	−.149*	−.339
Decade university job	.520*	.178	...	...	...	...	.302*	.130
Any unemployment	...	...	−.439*	−.190	...	...	−.421*	−.122
Sex	...	...	...	...	...	...	.533*	.268
Sex × Ph.D. caliber	...	...	...	...	...	...	−.173*	−.300
Sex × postdoctoral prestige	...	...	...	...	...	...	.066*	.124
Sex × tenure-track university first job	...	...	...	...	...	...	.949**	.083
Sex × early publications	...	...	...	...	...	...	−.053	−.034
Sex × % first-authored publications	...	...	...	...	...	...	−.419*	−.167
Sex × university × early publications	...	...	...	...	...	...	−1.090*	−.344
Sex × early citations	...	...	...	...	...	...	.198*	.141
y-intercept	−.571*	...	.048	...	.209	...	−.550*	...
R^2	.407*	...	.314*	...	.338*	...	.322*	
N	229	...	221		144	...	447	

* $P < .05$.
** $P < .07$.

their choice of a school, thereby producing a weaker association between ability and Ph.D.-department caliber for the women.

Finally, a postdoctoral fellowship experience (coded to show both receipt and prestige of a fellowship) showed no direct effect on the men's decade productivity; the zero-order association between these two variables was mediated entirely by the effect of a fellowship on the type of first job. For the women, the type of first job did not interpret the relationship between fellowship prestige and decade productivity. The small, positive direct effect may represent the impact of postdoctoral training but may also reflect the potential of a postdoctoral fellowship to integrate women into the discipline.

Organizational setting and rewards.—University employment usually implies rewards for productivity; however, the women who began their careers in universities were located disproportionately in nontenure-track positions, usually as research associates (62%, compared to 37% of the men). The rank distributions were even more dissimilar at the end of the first decade. Two-fifths of the women were still in nontenure-track positions, while virtually all the men were tenured or at least on a tenure track.

For the men, both first and decade employment in a university significantly increased decade productivity; neither variable affected the women's productivity. However, the pattern was reversed when first job was coded to show whether it was a *tenure-track* position at a university. For the men, working in a university was far more important than rank. Turner's (1960) distinction between contest and sponsored mobility applies here: university employment automatically entered men in the contest regardless of initial rank, and they performed accordingly. University employment (measured as tenure-track positions for the women and any position for the men) was more important for the women's productivity than the men's (b's = 2.38, 2.59 for continuously employed women, but only 0.45 for the men). Because the women were seldom promoted during their first decade, their decade position (regardless of how I coded it) made no contribution beyond that of their first job toward explaining their decade productivity. (Results are not shown in table 2.)

Early productivity.—The independent effect of scientists' early productivity on decade productivity may reflect a stable propensity to publish or stability in other unmeasured variables that affect productivity. Thus, the observed sex difference in the impact of early post-Ph.D. publications on decade productivity signals sex differences in either the propensity to publish or other kinds of instability. The negligible impact of the women's earlier publications suggests that their productivity depends more on external supports and rewards, which are often absent. It probably also reflects basic instabilities in their careers, which show more frequent job changes and, for 18% of the women, some unemployment. As Zuckerman

and Cole (1975, p. 92) observed, initially productive women may disappoint their sponsors, but slow starters who later surprise their sponsors with their performance are equally likely. Apparently, women depend more on intervening variables such as employment setting, collegial recognition, continued informal support, and professional rewards for performance.

We see the importance of such factors for both sexes in the significant interactions between early productivity and early job context. Among chemists of both sexes whose first jobs reinforced publishing, early productivity showed a moderate effect on decade productivity.[8] Although the coefficients for women are not very reliable because so few women held tenure-track jobs, they suggest stable productivity among women who were presumably rewarded for publishing.

Formal collegial recognition.—For both sexes the regression results support the hypotheses that (1) recognition from colleagues in the form of citations maintains productivity, and (2) the productivity of scientists who lack other rewards (due to either their position or their sex) depends particularly on the recognition that citations provide. Citations are more reinforcing for chemists employed outside universities and who presumably receive fewer rewards for publishing and for women (the *b*'s for this interaction term are −0.56 for the women, −0.80 for the continuously employed women, and −0.47 for the men). Note that these results reveal a second-order interaction between sex, employment setting, and citations; the difference in the effect of citations for chemists located in universities and those employed elsewhere was greater for the women than for the men. Despite their concentration in nontenure-track positions, university women are probably better integrated than nonuniversity women. Both their sex and their position place the latter on the periphery of the scientific community; thus formal recognition of their work, by symbolically expressing their ties to the scientific community, may help to maintain their productivity.

The effect of citations on decade productivity also varied with the chemists' early productivity: chemists who were more prolific at the beginning of their careers were less influenced by citations (or early productivity was less important for highly cited chemists). However, the strength of this interaction also varied by sex. Productive female chemists responded more to citations than their male counterparts did, although the coefficient for the continuously employed women was closer to the men's (*b*'s = −0.140 for the continuously employed women, −0.191 for the men).

Professional commitment: marriage, children, and continuous employment.—The partial effects of marriage and childrearing (both coded as dummy

[8] Since the sexes differed in typical positions within universities and thus in exposure to rewards for publishing, the components of the interaction term were varied accordingly. For men, I used the product of early productivity and university employment; for the women, the product of early productivity and a tenure-track university employment.

variables, with the single and childless coded 0 on these measures; the results are not presented in tabular form) were insignificant for the men; for the women, being married and having children each had negative but weak effects (*b*'s = −0.186 and −0.179, respectively, for all women and −0.026 and −0.017 for the continuously employed women). Thus, at least among chemists, neither variable was an important cause of women's lower productivity.[4] For continuously employed women, the attenuated effects of marriage and parenthood suggest that career continuity may affect the women's productivity more than family demands do. Any unemployment did reduce the women's productivity substantially ($b = -0.44$). Moreover, this effect was independent of variables that might have prompted women to interrupt their careers; including both marriage and children in the equation did not alter its magnitude (regression equation not shown).

Career continuity might reflect professional commitment[5] (although involuntary unemployment is more common for women than men; see *Chemical and Engineering News* [June 23, 1975, p. 23]), but opportunities for advancement undoubtedly affect both career continuity and commitment. In fact, among academic chemists, women whose first jobs were not on the tenure track were more likely to interrupt their careers than were women in tenure-track positions. Alternatively, human-capital theorists (e.g., Polachek 1975) suggest that women's productive value depreciates during periods of "home time"; thus, the longer a woman stays home, the more her research skills obsolesce and the lower her later productivity becomes. But the length of time a woman was out of the labor force had a weaker effect on productivity than did the dummy variable "any unemployment" rather than the stronger effect implied by the depreciation hypothesis. The data were also inconsistent with a third alternative, that the decade jobs of women who were ever unemployed were less favorable to high productivity (the regression equation including both decade job and career continuity is not shown). Regardless of its interpretation, continuous employment was not an important determinant of women's productivity: it accounted for less than 4% of the variance in decade productivity and was less important than first job or recognition.

A Statistical Test for Sex Differences in the Determinants of Productivity

Analysis of covariance achieved through dummy-variable multiple regression with sex-interaction terms provides statistical tests of sex differences

[4] However, see Hargens, McCann, and Reskin (1977) for an analysis of the effect of marital fertility on the productivity of married research chemists.

[5] This indicator obviously does not tap the variation in the professional commitment of chemists of either sex who were continuously employed.

shown in equations (1)–(3) of table 2. For this analysis I combined the samples of approximately equal numbers of men and women. Since women constituted 5% or fewer of the chemistry doctorates (Harmon and Soldz 1963, p. 52), the combined sample does not represent the population of chemistry doctorates. Thus, the estimates of the main effects of any variables whose effects differ by sex (e.g., caliber of the Ph.D. department) are biased. However, the estimates and the statistical tests for the partial effects of sex and the sex interactions are not biased. One final caution—because several of the sex interactions are statistically significant, the main effect of sex is not readily interpretable, and its significant positive partial coefficient does not mean that the direction of the original sex difference was reversed by taking into account other variables.

In general, the results shown in equation (4) confirm the prior conclusions that the determinants of scientific productivity differ by sex in several ways. Briefly, caliber of the Ph.D. department was more important for the men's productivity than the women's, while the opposite is true for a prestigious postdoctoral fellowship. Women benefited more than men from employment in a tenure-track university position, whereas men's early productivity had a bigger impact on their decade productivity, and university employment accentuated the impact of early productivity on later output for men more than for women. Finally, women were more influenced by citations than men were.

In conclusion, male chemists—at least those who finished graduate school 15–20 years ago—probably outpublished female members of their cohorts, although to a smaller degree than is commonly supposed, and the determinants of productivity differ for the sexes. But why should sex, functionally irrelevant for scientific performance, be associated with differential conformity to productivity norms? And why should the determinants of productivity differ for the sexes? Implicit in the claim that women have less professional commitment is the assumption that performance differences stem from sex differences that are prior to and independent of the institutional structure of science. Yet attempts to understand other group differences within science (e.g., disciplinary differences) look to organizational features of science for explanations rather than hypothesizing a priori group differences. Kanter's (1977) work illustrates the value of structural analysis for understanding sex differences in organizational performance. Such analyses may also help to account for differences in scientific productivity.

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Review Essay: The Sociological Economics of Gary S. Becker[1]

The Economic Approach to Human Behavior. By Gary S. Becker. Chicago: University of Chicago Press, 1977. Pp. vi+314. $17.00.

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The comparison between economic and sociological perspectives on human action has long been a fruitful one. Since Weber's studies of the social background of economic activities and Parsons's (1937) criticism of economic theories, sociologists have recognized the topic as significant. More recently, however, a number of economists have applied their analytical tools to the study of problems related to sociology. Sociologists have too often ignored these developments, which might constitute both a challenge and a source of fruitful innovations.

An outstanding contributor to these developments is Gary S. Becker, of the University of Chicago, whose studies in various fields relevant to sociologists have cumulated over two decades and are brought together selectively in *The Economic Approach to Human Behavior.* His role has resembled that of a theoretical physicist elaborating elegant theoretical schemes, drawing precise inferences from them, and testing them with data gathered largely by others. The scope and confidence of his analysis are suggested by the following statement: "The economic approach is a comprehensive one that is applicable to all human behavior, be it behavior involving money prices or imputed shadow prices, repeated or infrequent decisions, large or minor decisions, emotional or mechanical ends, rich or poor persons, men or women, adults or children, brilliant or stupid persons, patients or therapists, businessmen or politicians, teachers or students. The applications of the economic approach so conceived are as extensive as the scope of economics in the definition . . . that emphasizes scarce means and competing ends" (p. 8).

Becker's Approach

Becker has applied the reasoning of microeconomics to social phenomena, extending it into the realm of "nonpecuniary motivation" ([1957] 1971*a*,

[1] This review essay is a revised version of a paper presented at the annual meeting of the American Sociological Association, New York, September 1976. The term "sociological economics" refers to the study of social phenomena by means of economic concepts and methods. In compound terms of this sort, as sociologists will recognize, the second word typically indicates the base discipline and the first the subject matter under study.

p. 11). Microeconomics is concerned with "the *system* of market prices which strikes an equilibrium among people's tastes for different goods and the scarcities of total resources that can produce them" (Samuelson 1970, p. 357). And as Becker (1971*b*, p. 2) describes the field, "Our main interest . . . is in the market behavior of aggregations of firms and households. Although important inferences are drawn about individual firms and households, we try mainly to understand aggregate responses to changes in basic economic parameters like tax rates, tariff schedules, technology, or antitrust provisions." He summarizes the approach, as applied to human behavior in general, thus: "The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it" (p. 5).

In a perfectly competitive market under certain conditions, such as those of scarcity (increasing costs or diminishing returns), given tastes, and technology, the interaction of firms and households can be shown to lead to a general equilibrium, with determinate prices and allocation of factors of production to productive processes. The mathematical statement of the conditions and nature of this general equilibrium was first presented by Walras (1874). But the special case of "partial equilibrium theory," more frequently used, considers a subsystem of the economy such as the market for a single commodity, under the assumption that other variables do not vary; the familiar diagram of supply and demand curves is of this sort.

The responses of a competitive market to exogenous changes are estimated by the method of "comparative statics": the configuration of the equilibrium is considered before and after the exogenous influence, and the nature of the resulting change is inferred. The length of time required for the resulting change to occur, and the detailed processes by which it occurs, are not usually investigated.

The theory of market equilibrium also has implications for the choice of desirable policies. A perfect market not only has a point of equilibrium but at this point provides a Pareto-efficient allocation of factors of production and of goods to consumers (Bator 1957). If we accept the Pareto criterion of desirability, we can then identify instances of "market failure," when certain assumptions of the model fail to obtain (as in the case of externalities and collective goods), and corresponding possibilities of remedial action. Economists sometimes go beyond the Pareto criterion and formulate "social welfare functions" as criteria for choice among policies; Becker does so in his work on crime. This valuative economic approach, together with a model of the system, leads economists to propose new policies (vouchers, effluent charges, fines, bonds for enforcers, new voting systems) as well as to criticize existing ones (e.g., rent control, excise taxes). But as Becker pointed out in an early essay (chap. 3), government behavior may also involve imperfections as great as those of the market.

The aspects of microeconomics, and of other fields on which Becker draws, have a diversity that is not adequately suggested by my brief description of the field. He draws on a rich mathematical literature related to other fields of economics. The reader who wishes to follow his discussions in detail should

be prepared to deal with problems of constrained maximization of functions of several variables; partial derivatives and Lagrange multipliers appear frequently.

I shall summarize the selections in this volume, supplementing them by reference to Becker's other work. The topics will be rearranged in sequence from relatively simple to complex in terms of the sociological issues involved. I consider first nonmarket inputs into market processes, then aspects of social structure, and finally crime and punishment. For each of Becker's topics of analysis, I shall consider related sociological problems, selected findings, policy recommendations (if made), and suggested lines of sociological research.

Nonmarket Inputs

Human capital.—An innovation in Becker's work has been to consider consumer or household activities that are not directly involved in the market. One such activity is the use of time and goods not for consumption but for the development of later earning capacities. The production of "human capital" in this way is closely related to social stratification. For example, the model of the competitive market contains a much more precise variant of the functional theory of stratification. Factors of production are rewarded in terms of prices associated with their marginal productivity. Thus if we compare the productivity of persons with various sorts of skills, those whose skills permit greater marginal productivity are rewarded more in this sort of market. "... Some persons earn more than others simply because they invest more in themselves" (Becker 1975, p. 231).

The individual's calculation as to when to devote resources to his own human capital leads to variation in this investment over the life cycle. Investment at earlier ages allows returns to be realized over a longer period (p. 123). Variation of wage rates with age leads to an inverse variation in time devoted to consumption (pp. 118–21, 142), paralleling variations among income groups. The choice whether to receive earnings now or to postpone them in order to receive greater earnings later is seen in the economic approach as strictly a calculation of expected gain, discounted for future years. It is not seen as resulting from values characteristic of different age groups, social classes, or ethnic groups. Thus Becker states (p. 5) that preferences are assumed not "to be very different between wealthy and poor persons, or even between persons in different societies and cultures."

Becker explains by the theory the observed fact that age-earnings profiles are steeper for the skilled and educated (Becker 1975, p. 81). Such changes in earnings during one's lifetime may be related by sociologists to social mobility within a given occupation or to the effects of human capital on mobility between occupations. He also deduces that groups with a higher expectation of participation in the labor force tend to have a higher expectation of return on investment in education. Thus women are less likely to

continue schooling than men, but nonwhite women, who have a higher expectation of labor-force participation, are more likely than white women to continue from high school to college (1975, pp. 178–79).

When investment in human capital is considered by a firm for its employees or by a society for its members, the costs of this investment must be weighed against the returns. The opportunity costs of training must include "the time that a person spends on his training, time that could have been used to produce current output" (1975, p. 19). And a firm that provides on-the-job training must calculate its expected losses from job turnover of those whom it has trained. If the training involves general or transferable skills, there are presumably gains to other firms and no loss to society; but if the skills are specific to the first firm, there is a social loss from job turnover. Such losses might well be considered if sociologists were to make judgments as to the optimum rate of social mobility.

The contribution by education to general social benefits may be underestimated if only the private returns and not the possible externalities are counted. But it might be overestimated if "college graduates earn more partly because their productivity was systematically overestimated," in which case "private returns would tend to be larger than social ones" (Becker 1975, p. 195). This problem is closely related to that of credentialism, which has been the subject of sociological research. Becker also suggests that further research is desirable on "the quantitative effects of different kinds of ability on earnings and productivity" (1975, p. 235).

Another valuative question concerns the perpetuation of families' income advantages through their ability to finance their children's education (Becker 1975, p. 79). Alternative policies include guaranteeing education with subsidies—which might not be efficient—or devising means whereby less affluent students could pay for it from their subsequent earnings—means which face the problem as to whether collateral could be offered (1975, p. 80).

A major potential contribution by sociologists to the analysis of human capital lies in the measurement of the capacities that are produced by such investment. Whether it be capacities for production or for consumption, actual skills or social labels, cognitive abilities or attitudinal changes, capacities for private gain or for contribution to the good of others through public action—all are topics slighted by the economic approach, which might be illuminated by direct measurement. These and other possibilities for measurement will be summarized in table 1.

The allocation of time.—A more complex analysis of the use of time and goods for nonmarket purposes is provided by "the new theory of consumer behavior" (chap. 7). In this theory, households are seen as not merely purchasing goods in the marketplace, but are "assumed to combine time and market goods to produce more basic commodities that directly enter their utility functions" (p. 91). Examples are "the quality of meals, the quality and quantity of children, prestige, recreation, companionship, love, and health status." These commodities "are not marketable or transferable

among households" (p. 207).[2] Preferences for these commodities are assumed to be more nearly invariant than preferences for market goods, with respect to time and individual differences (pp. 5, 145 n.).

Becker introduces this notion in "A Theory of the Allocation of Time" (chap. 5). In place of the customary budget constraint on consumer expenditure, based on total income, he substitutes the more general notion of "full income," the total money income that would be "achieved if all the time available were devoted to work" (p. 93). This potential income—measured in monetary terms but devoted to achieving both monetary and nonmonetary returns—is then the basis of rational allocation by the consumer.

Related sociological problems include the study of time budgets, but as Becker points out (p. 114), a theoretically guided approach would gather information simultaneously on the "expenditure" of both money and time, which are interrelated. Time is assumed to be exchanged for money in terms of the wage rate, but sociologists might also examine the conditions under which an individual places a given value on time. Variations among individuals in their valuation of time relate many of Becker's findings to the lifestyles of different social classes. As wage rates increase, the value of time taken from earning is greater, though unearned income does not have this effect. One consequence of increased wage rates, relative to the prices of goods, is to make people more economical of time and less so of material goods; these are also features of America in contrast to poorer countries (pp. 105, 109–10).

Becker's analysis of "leisure" leads him to substitute for this concept a theoretically more relevant notion—that of "unproductive and time-intensive commodities" (p. 104). Various uses of time can in fact be arrayed on a continuum according to their contribution to consumption and to income. The relative contributions of time (foregone earnings) and of expenditure to various forms of "leisure" also vary, and corresponding variation in the effects on them of changes in wage rates or prices can thus be expected (p. 100). This refinement and modification of a concept drawn from everyday usage is a frequent feature of scientific advance. Schooling itself, even if primarily a contribution to later earnings, can also contribute to household productivity, and Michael (1972) has estimated the magnitude of this latter return (p. 143).

Becker draws few policy conclusions from his analysis of the allocation of time. One may be that queuing, a form of allocation of resources that may promote equity more than do prices, nevertheless produces social costs, "because time is a cost to demanders, but is not revenue to suppliers" (p. 112 n.). We shall see that he makes a similar observation with respect to imprisonment of offenders as an unproductive use of their time, suggesting fines instead.

Fertility.—Becker applied an early version of the "new theory of consumer

[2] The categorization of "basic commodities" parallels efforts by social scientists to list basic human motives, such as W. I. Thomas's "four wishes" and other statements of human needs.

behavior" to the analysis of fertility in 1960 (chap. 9). In this analysis, the family is treated as a productive unit, but the internal structure of that unit is not yet explored. Children are treated as "durable consumer goods" (p. 169); it is assumed that expenditures on children may be devoted partly to their number and partly to their "quality," defined by their expense (p. 173) rather than by any directly measured characteristics. As for "almost all other consumer durables, such as cars, houses, or refrigerators," families are expected to "purchase more units as well as better quality units at higher income levels, with the quantity income elasticity usually being small compared to the quality elasticity" (p. 174). In these terms he analyzes the relation between fertility and income.

The applicability of this theory depends on the spread of contraceptive knowledge, making the production of children controllable at less psychic cost. Becker shows that for subgroups of the American population who are more likely to plan their families, fertility increases with income. For the population in general, the observed inverse relation between fertility and income was narrowing over time, presumably due to increases in contraceptive knowledge; and the relation was positive or less negative for number of children desired, in comparison with actual numbers of children. Birthrates were related to business cycles but responded less to these cycles than did consumer durables (p. 187). Becker provides a plausible account of this difference in terms of the measurement of numbers of children and not their quality, the spreading of expenses for children over a longer period than those for consumer durables, and other theoretically relevant variables. In a subsequent article with Lewis (chap. 10), he analyzes the relative response of quantity and quality of children to change in income and prices, relating these to increases in the education of mothers and to advances in birth-control knowledge (p. 200).

He also considers the allocation of time as an influence on the relation between fertility and income. Over and above consideration of the quality of children, "one other factor that may be more important, however, is the increase in forgone [*sic*] costs with income. Child care would seem to be a time-intensive activity that is not 'productive' (in terms of earnings) and uses many hours that could be used at work. Consequently, it would be an earnings-intensive activity, and our analysis predicts that its relative price would be higher to higher-income families." He presents additional evidence that this may be the case (p. 106).

Social Structure

The topics treated so far involve individuals who make choices without considering their relations to others. Even the analysis of fertility ignores interpersonal relations, treating the family rather as a unitary decision-making entity. Another set of Becker's writings, however, deals with activities in which the actor explicitly considers others.

Discrimination.—The first of these is *The Economics of Discrimination*

(excerpted in chap. 2), in which he considered the effects on markets that would occur if participants placed a value on *not* associating with other persons having given characteristics. He defines discrimination as "the use of nonmonetary considerations in deciding whether to hire, work with, or buy from an individual or group" (Becker [1957] 1971*a*, p. 11). Here, as in the case of "leisure," he modifies a customary definition in favor of one that better fits the theoretical scheme. He dismisses notions of inaccurate perception or stereotypes as not having a precise logical relation to market decisions affecting productivity. An employer, then, "faced with the money wage rate π of a particular factor . . . is assumed to act as if $\pi(1 + d_i)$ were the *net* wage rate," where d_i is his "discrimination coefficient." Similar additive coefficients are introduced for employees and consumers ([1957] 1971*a*, pp. 14–15). In the case of "nepotism" the coefficient is negative, anticipating a possible symmetrical treatment of positive social relations.

Becker finds that when discrimination by one group against another is introduced into a competitive market, both groups lose (p. 18). When discrimination occurs, minorities suffer more than majorities. And when a minority tries to retaliate by counterdiscrimination, it loses still more (p. 26).

The overall effects of particular tastes for discrimination depend not simply on individual tastes but on the configuration of the market. For example, if the relative supply of two groups (N and W) changed, the resulting market discrimination would change even though individuals' coefficients of discrimination did not. The extent of market discrimination in competitive industries is affected by the dispersion of the distribution of individual coefficients of discrimination; in monopolistic industries, on the other hand, only the median coefficient of discrimination matters (Becker [1957] 1971*a*, pp. 43–47).

Analyzing data on income, schooling, and racial proportions for Southern SMAs in 1949, Becker concludes that "the proportion of non-whites in a SMA does not seem to have an important effect on tastes for discrimination that operate through the market." But "since most education is publicly administered, . . . in the South political discrimination against non-whites is positively associated with their relative number" ([1957] 1971*a*, p. 126). He also estimates from economic data that "individuals in the South appear to have had, on the average, slightly less than twice as much taste for discrimination as those in the North" ([1957] 1971*a*, p. 121). Conceivably, if sociologists formulated survey questions in terms that would fit Becker's model, they could make independent estimates of such ratios.

From a sociological point of view, it is important to measure directly the variables whose effect Becker measures only indirectly through market behavior. Studies of "social distance" and of intergroup attitudes should throw light on these same questions, especially if designed so as to refer to monetary trade-offs. Further research might also test whether the coefficients d_i enter in additive, multiplicative, or some other form.

Social interactions.—Becker's "theory of social interactions" (chap. 12) extends the model of the chooser to include possible changes that he may produce in the characteristics of other persons or their relations to himself.

A central innovation of this theory is the notion of "social income," which parallels that of "full income" in the allocation of time. This is "the sum of [one's] money income and the value to him of his social environment," the latter being defined in terms of "characteristics of other persons" (pp. 256–57), including the utilities (in monetary units) that he receives from others' welfare or diswelfare.

Interesting inferences from the theory concern the interactions of individuals with a social or political system that counteracts or complements their efforts. For example, stratification depends on the extent to which income or wealth is transferred selectively from one generation to the next. But according to Becker's model, parental expenditures on their children may vary far more with parents' income than do the children's incomes themselves. "Indeed, contributions would be more responsive to parental income the stronger are the basic forces producing mobility because parents attempt to offset these forces" (p. 273). Similarly, parents will adjust their "bequests" (including gifts and transfers to children for education) to compensate for governmental efforts to alter the income distribution. Thus "increased taxes on future generations would be matched by increased bequests to them," but "increased public investment in education would be matched by reduced private investment in education" (p. 267).

An interesting theorem predicts the behavior of members of a family in the presence of one member who "cares sufficiently about other members to be the head. . . . Sufficient 'love' by one member leads all members by 'an invisible hand' to act as if they too loved everyone" (p. 270). Similarly, a charitable person and the recipients of his charity form a synthetic "family" each of whose members is "partly 'insured' against catastrophes because all other members, in effect, would increase their giving to him until at least part of his loss were replaced. Therefore, charity is a form of self-insurance that is a substitute for market insurance and government transfers. Presumably, the rapid growth of these latter during the past 100 years discouraged the growth of charity" (p. 274). He applies this theorem to the behavior of altruistic individuals with regard to their genetic fitness (chap. 13), noting that altruism produces effects on recipients' behavior which may have been neglected in the sociobiological literature (p. 293).

He also derives hypotheses as to the relations of crime to income. Crimes against persons are taken as examples of envy or malice and considered "predatory" as distinguished from crimes against property, which may be more economically motivated in the conventional sense. He concludes that a rise in one's own income relative to that of others would reduce these "predatory expenditures," while a general increase in incomes would be less likely to have such an effect; the data seem to bear out this expectation (p. 278).

The research possibilities suggested to a sociologist by this theory are very fundamental. If Becker can obtain results of such interest and diversity without directly measuring the social bonds and configurations that constitute social structure, how much more powerful might be the results *with* such measurements! Social bonds within the family can vary among his-

torical periods and stages in the life cycle, and these variations should have effects of the sort Becker considers. Bonds outside the family also affect economic behavior. A combination of Becker's analysis with the detailed measurement characteristic of sociology should thus facilitate a more complete delineation of social structure and provide an opportunity to test hypotheses concerning that structure.

Marriage.—Becker's "theory of marriage" (chap. 11) deals not with social structures as given but with their formation. He approaches the problem of mate selection from the viewpoint of the equilibria of the marriage market—a market that does not involve monetary exchange. In this market, as in any other, the behaviors and choices of individuals depend not only on their own preferences but also on the entire configuration of opportunities that are provided to them by the choices of others. Thus, "the marriage market chooses not the maximum household commodity output of any single marriage but the maximum sum of the outputs over all marriages, just as competitive product markets maximize the sum of the outputs over all firms." But Becker stresses "that the commodity output maximized by all households is not to be identified with national output as usually measured, but includes conversation, the quantity and quality of children, and other outputs that never enter or enter only imperfectly into the usual measures" (p. 216).

Diverse types of inequality result from the operation of the marriage market. "Positive assortive mating . . . increases the inequality in traits, and thus in commodity income, between families." But Becker goes on to point out that "the effects on inequality in commodity and money incomes may be very different; indeed, if wage rates, unlike most other traits, are negatively sorted, assortive mating would reduce the inequality in money earnings and increase that in commodity income" (p. 241).

Other conclusions deal with the likelihood of marriage. The principal commodity for which marriage provides a gain through complementarity of inputs is "own children"; "Hence, persons desiring relatively few or low-'quality' children either marry later, end their marriages earlier, or do both" (p. 212). The likelihood of marriage is also influenced by income: "Our analysis predicts that a rise in property income, necessarily, and a rise in wage rates, possibly, increase the incentive to marry." Even though the data appear to contradict this prediction, "when years of schooling and a few other variables are held constant, higher-wage persons appear to marry earlier than others" (p. 213). In addition, higher wage rates for women, relative to those for men, decrease the incentive to marry, as they would lead to a decrease in the net productivity of marriages. Cross-sectional data on American states support this inference (p. 214).

A second set of conclusions deals with the conditions for assortive mating. Becker concludes that "the association of likes is optimal when traits are complements and that association of unlikes is optimal when they are substitutes, . . . since high values of [a given trait] reinforce each other when they are complements, and offset each other when they are substitutes" (p. 218). He first considers the case in which males (M) and females (F)

differ only in market wage rates, and concludes (with the aid of empirical evidence) that "a negative correlation between [wages of *M* and *F*] maximizes total output because the gain from the division of labor is maximized" (p. 220). This conclusion runs counter to some observed positive correlations between spouses' wage rates. But further analysis suggests the possibility that inclusion of wage rates for wives not in the labor force might support the inference (p. 224). For traits that raise nonmarket productivity, in contrast with market productivity, Becker suggests that they are complementary between spouses and therefore should be expected to be positively associated (p. 222).

A third type of conclusion concerns the influence of the marriage market on intrafamilial allocation of "output" between spouses (p. 231). The theory "does not take the division of output between mates as given, but rather derives it from the nature of the marriage market equilibrium. The division is determined here, as in other markets, by marginal productivities, and these are affected by the human and physical capital of different persons, sex ratios, that is, the relative numbers of men and women, and by some other variables" (p. 233).

The allocation of output within the family is modified, however, by the possibility that spouses care for one another. Caring (i.e., interdependent utilities) induces transfers within the family and reduces the effects of the external market on the resulting internal distribution of output (p. 235). But "output is generally less divisible between mates in marriages with caring than in other marriages because caring makes some output a family commodity, which cannot be divided between mates. One implication of this is that marriages with caring are less likely to be part of the optimal sorting than marriages without caring that have the same total *income* (and thus have a greater total output)" (p. 238).

Although Becker's model of marriage formation is potentially very fruitful, it also contains a limitation that will be conspicuous to sociologists. The role of "caring" in the formation of marriages is taken as that of a preference fixed in advance; this limitation of the microeconomic approach seems unrealistic.

Crime and Punishment

From a sociological perspective, the creation of a social structure such as a marriage involves the creation of social norms, which include inter alia the change of preferences. The apprehension and punishment of criminals is similarly intended to influence preferences by instituting or maintaining norms. Becker's analysis of crime and law enforcement, however, departs conspicuously from the sociological perspective; it assumes that the only effect of law enforcement on potential criminals is to change the situation in which they operate by altering the effective prices placed on various activities. Within this perspective, Becker deals with both positive and valuative questions: how enforcement actually occurs and how it should be conducted.

He formulates a loss function L (a negative social welfare function) depending on the net damage to society, the cost of apprehension and conviction, the social cost of punishment, and the supply of offenses. The two decision variables that may be altered from one policy to another are taken to be the degrees of certainty p and severity f of punishment. "Their optimal values are found by differentiating L to find the two first-order optimality conditions . . ." (p. 51). From the resulting equations Becker again draws interesting conclusions, including the possibility that under some conditions the authorities would increase the severity of punishment while decreasing its certainty, thereby hoping to provide the same deterrent effect at smaller cost.

Becker also deduces that as the marginal damages per additional offense increase (as from one crime to another), "the optimal values of both p and f would increase" (p. 55). He shows that the actual severity and certainty of punishments for various crimes vary in the way expected (p. 57). He also advances a plausible argument to explain "why the secular improvement in police technology and reform has gone hand in hand with a secular decline in punishments" (p. 58).

A policy proposal issues from consideration of the effects on the number of offenses of changing p and f: "the results . . . imply that . . . the total loss would be reduced by 'charging' *lower* 'prices'—that is, lower p's and f's —in markets with lower elasticities." This conclusion corresponds to the allocation of resources to uses where they are more effective. Becker concludes that this principle is manifested in the giving of lower sentences for unpremeditated crimes, the perpetrators of which would be "relatively unresponsive to the size of punishments" (p. 59). But his conclusion is based on the elasticities of response, not any difference in possibilities of learning.

A major policy conclusion is that fines should be used wherever feasible. Becker notes that "probation and institutionalization use up social resources, and fines do not, since the latter are basically just transfer payments, while the former use resources in the form of guards, supervisory personnel, probation officers, and the offenders' own time" (p. 63). Moreover, "fines provide compensation to victims," as other sorts of punishment do not (p. 64).

In addition to testing the propositions deduced by Becker, sociological research on this topic might well pursue two courses: (1) the measurement of variables that Becker leaves implicit or measures only indirectly, including social welfare functions; and (2) the development and testing of models in which changes of preference (for or against crime at given prices) are included as endogenous and directly measured.

Microeconomics and Sociology

Becker's analyses demonstrate the value of economic reasoning over a wide range of sociological topics. But sociologists will ask whether the detailed assumptions of the analyses are true. Microeconomics ignores the changes of preference that sociologists believe to occur through communication,

socialization, and social control. The microeconomic model is sparse and austere (and thus parsimonious), abstracting away from the rich diversity of cultural, historical, and social-structural differences that sociologists are accustomed to stress. It is static—a problem to which economists have responded by developing macroeconomics. But even if it should hold true only under limited conditions, it has advantages in its elegance, precision, and verifiable implications as compared with nonmathematical sociological theories.

Measurement.—A major question concerning the truth of the microeconomic model is whether the individual units in the economy do in fact maximize as they are assumed to do. The question here is not whether they maximize material things—Becker's treatment is far more general—but what, when, and how much they do maximize. Most sociologists will be inclined to go directly to the persons constituting these units, observe their behavior, and ask them questions about their motives and their perception of the world. The approach of Herbert A. Simon and his co-workers to the decisions of the firm illustrates such an effort to be accurate in detail, and has led to the notion of "satisficing" rather than maximizing. In a review of the literature on this topic, Cyert and Hedrick (1972) suggest that most economists have judged the actual process of organizational decision making to be irrelevant and have simply adhered to the dominant model of maximization. They state that "the problem is clearly difficult, but we wonder whether economics can remain an empirical science and continue to ignore tne actual decision-making processes of real firms" (1972, p. 409).

A model with the power and accomplishments of the microeconomic model will thus not be discarded easily, even if the process of individual maximization should obtain only in a general fashion. The processes by which scientific paradigms resist change (Kuhn 1970) are well exemplified in this case. The maximizing model may also be defended on the ground that similar behavior can result from a variety of motivations. Becker, for example, postulates impulsive or random behavior (chap. 8) and shows that, because of the constraint of scarcity, negatively inclined demand curves result from these assumptions as well as from rational behavior.

Another aspect of the economic paradigm, however, may be clearly more of a liability than an asset: the restriction to market data rather than survey data. Becker justifies the use of data provided by the market, as contrasted with direct reports of motivation by respondents, in this way: ". . . The economic approach does not assume that decision units are necessarily conscious of their efforts to maximize or can verbalize or otherwise describe in an informative way reasons for the systematic patterns of their behavior. Thus it is consistent with the emphasis on the subconscious in modern psychology and with the distinction between manifest and latent functions in sociology" (p. 7). Becker's theories thus contain numerous unmeasured variables, most of which center about individuals' choices: indifference curves, elasticities, utilities, and their dependence on other variables. If there is to be a synthesis between the two approaches—and it would be of immense

value—it must include a contribution by sociologists and others to the direct measurement of these variables.

Throughout this review I have referred to variables that can be measured more directly by sociologists; these are summarized in table 1. Each of the topics I have reviewed involves one or more significant nonmarket variables. While Becker's analyses typically make inferences about these variables from market data, or other observables related to them, sociologists can also measure them directly. Three types of variables seem particularly appropriate to measure: (*a*) characteristics or behaviors of individuals (skills, time budgets, production); (*b*) trade-offs or monetary valuations made by the individual (evaluations of time, of basic commodities, of associating with others); and (*c*) changes of preferences. A review of my previous discussion together with table 1 will show that these measurements can be central to Becker's theories as well as interrelated with one another.

Policy analysis.—The measurement of trade-offs between values is important not only for testing Becker's models but also for the analysis of policy choices. Insofar as individuals' preferences are our basis for judging the merits of policies, we need to ascertain them; and if external effects of market processes are to be estimated for decisions and planning, we need to measure them in quantitative terms. Attitude measurement in terms of comparison between objects in which the individual is interested, as Coleman (1957) once suggested, is relevant for these purposes.

TABLE 1

VARIABLES MEASURABLE BY SOCIOLOGICAL RESEARCH

Topic	Nonmarket Variable	Measurements
Human capital....	Human capital	Skills; kinds of ability; credentials; effects on individual's production
Allocation of time.	Time; household production	Time-money budgets; identification and measurement of basic commodities; monetary valuation of time; household production functions
Fertility..........	Quality of children	Skills; nongenetic aspects of intelligence; values and behavior patterns of children
Discrimination....	Discrimination coefficients	Intergroup attitudes measured in monetary units; test for additivity of d_i
Social interactions.	Nonmonetary components of social income	Monetary evaluations of characteristics of others and of social bonds; their dependence on chooser's actions
Marriage....	Basic commodities and household production	Monetary valuation of basic commodities; household production functions; changes in "caring"
Crime...........	Nonmaterial costs of crime	Measurement of externalities in monetary terms; valuation of normative order; changes in behavior tendencies due to punishment

Any ethical system that is concerned with the maximization of some quantity must be concerned with trade-offs. Thus for such an ethic the value of particular states of affairs for individuals must always be assessed in comparison with other states of affairs. The basic measurements of value for an individual then center about what things, in what quantities, are valuable for him or her relative to other things. One may imagine a multidimensional surface describing the welfare received or experienced by that individual in terms of his possession of various goods, services, or other characteristics of his situation. In Becker's formulation the dimensions of such a surface might correspond to "basic commodities." With consensual assumptions about yardsticks for interpersonal comparison, these individual functions may be aggregated (MacRae 1976, pp. 126–29).

This welfare surface for the individual should not, however, be confused with the social welfare function that the individual entertains. The welfare surface for the individual expresses the sources of that individual's welfare, in terms of the particular ethical system that we may choose as a standard of measure. The ethical system in terms of which we assess the individual's welfare is *our* social welfare function; the individual in question may have one as well, referring to all members of society and not merely to himself.

Nor does the welfare surface for the individual necessarily correspond to that individual's preference surface (often referred to as a "utility" surface, a usage that unfortunately embraces both these meanings). For certain types of utilitarian ethics the correspondence is close; for the economic ethic of preference satisfaction, welfare and preference may be identical, and the term "utility" may then be used in its double meaning; but for other ethics such as perfectionism, the two may diverge greatly. Even for some types of utilitarian ethics, the welfare and preference surfaces for an individual differ in important ways (MacRae 1976, pp. 138–45).

My review of Becker's work has aimed to show its potential benefits for sociology in valuative as well as positive research. The power and generality of his theories are a challenge to sociologists to test these theories by their own procedures, to extend them, or to develop analogous theories of systems in equilibrium—or even dynamic systems. The directness of application of microeconomics to policy choice poses the question whether sociology might be applied similarly within a precise valuative framework.

These advantages and challenges are presented by Becker and by other economists working along similar lines not merely to sociology but to all the other social sciences. We cannot be sure which disciplines will take the most advantage of them. These lines of investigation may continue to be developed largely within economics, but they will not thereby attain their greatest potential. One or more other disciplines may reach out to make contact with them; my review is intended to persuade sociologists to make that effort. But if this synthesis is too strongly resisted by existing disciplines and their paradigms, perhaps it can find a place in the emerging applied discipline of policy analysis (MacRae 1976, chap. 9)—and provide a needed theoretical base to supplement the analysis of particular policy choices.

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Review Essay: Social Roles and Economic Firms: The Sociology of Human Capital[1]

Human Capital. By Gary S. Becker. 2d ed. New York: National Bureau of Economic Research, 1975. Pp. xvii+268. $10.00.

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> It is my belief that economic analysis is essential in understanding much of the behavior traditionally studied by sociologists, anthropologists and other social scientists. This is a true example of economic imperialism! [GARY BECKER, *Economic Theory*, p. 2]
>
> Human capital is strictly an economic concept. Although it pertains to particular attributes of man, it is not intended to serve those who are engaged in analyzing psychological, social, or cultural behavior. [THEODORE SCHULTZ, *Human Resources*, p. 5]

It is unusual for one academic discipline to openly challenge another on its home terrain. Confrontation mobilizes forces of resistance and raises the ante needed to wage successful conquest. Intimidation, however, can have salutary consequences as well by opening up new intellectual opportunities and sharpening a field's sense of collective identity and professional mission. Recent developments in human capital economics challenge sociologists to place the study of human behavior on a more "scientific" footing. Human capital theory is a prototype of axiomatic reasoning in the social sciences; it begins with general propositions about the behavior of economic firms and deduces testable hypotheses about the earnings and investment behavior of individuals in human resource markets.

Much of the subject matter covered by the theory already is familiar ground to sociologists, the dominant conceptual accent falling on productivity as a form of goal-oriented behavior. Human capital theory conceptualizes individuals as economic firms, a notion analogous to the sociological one of social roles. The analytic contrast between economic firms and social roles provides a suitable point of departure for assessing the sociological merits of the theory and in turn challenges sociologists to defend the need to abstract from individuals in order to understand human behavior.

Conceptual History

Throughout its history, sociology has maintained a lively dialogue with economics about the "true" nature of human behavior and the place of the

[1] I wish to thank Clifford Clogg, Joel Gibbons, and Paul Hirsch for helpful comments.

individual in modern society. Major conceptual advances in both fields have resulted from attempts to reconcile human wants with social needs, scarce resources with competing ends. Industrialization and the decay of moral rule have sharpened the conceptual boundaries separating the two fields down to the present day. The resulting professional rivalry, however, has not been without redeeming consequences. We can thank the classical economists—notably Smith and Ricardo—for much that is distinctively sociological in Marx's analysis of this new industrial order; Menger and the Austrian marginalists for much of Weber's work on the social structure of economic action; the early utilitarians and later Spencer for Durkheim's attempt to extract the conceptual core of modern sociology from the precontractual bases of economic exchange; and more recently, Marshall and modern price theory for Parsons's efforts to reintroduce purposive action into this conceptual framework.

Marginal utility and the value-resource paradox.—The past half-century has been a gestative period in the history of both fields. Many sociologists turned to middle-range theories of social order to graft empirical flesh onto the skeletal framework of modern society erected by Marx, Weber, and Durkheim. Classical sociology had reinterpreted the old Hobbesian problem of order as a breakdown in the meaning of life. Though differences in emphasis abounded—"alienation," "disenchantment," "anomie"—all generally agreed that industrialization somehow had separated the goals of individuals from their means of social attainment. Many of their students, however, when surveying the contemporary scene, failed to find the chronic disorder implied by this earlier vision. It became the job of middle-range theory to make sense of this apparent paradox—that is, to explain the origins of social order in "mass society."

Economists meanwhile struggled to explain observed rises in national income unaccounted for by traditional factor models of economic productivity. For most, industrialization had posed the value problem much more narrowly than for sociologists. Economic development creates a voluntary contractual society, liberating the individual to enter freely into economic exchange. Free and competing tastes were assumed to be a sufficient condition for the orderly expression of self-interest. In the classical theory of value, labor scarcities and social constraints accent the role of production costs in the rate of exchange of economic commodities. A modern, salaried work force, by contrast, is free to express its subjective preferences through the buying power its labor produces. All commodities have a value in use, as determined by their utility in satisfying consumer tastes. This new world of marginal utility remade laborers into consumers and replaced the cost functions of commodities with the objective functions of those consuming them.

The early utilitarians had also argued for the free expression of individual wants but feared its repercussions on the orderly conduct of social life. This ambivalent strain in economic thought disappeared from the theory of marginal utility developed by Jevons, Menger, and Walrus around the turn of the century. Economic tastes, it was argued, do not directly compete, since individuals place different values on the same commodity. Thus, while

human appetites may be insatiable, the utility of a specific commodity diminishes in value with increasing use, a key notion which was subsequently carried over into the microeconomic theory of the firm.

For the marginalists, human values were revealed through the price of economic goods and services. It did not matter why a specific commodity was valued, only that individuals had the resources and freedom to express their subjective preferences. Economic exchange, in other words, required no ultimate purposes to sustain itself. Marshall later reconciled this radical individualism with the scarcity principle in classical economics to lay the cornerstone for modern-day supply-demand analysis. If commodity prices, however, were the result solely of the revealed values of those consuming these commodities, what explained the widely varying profits of the individuals and nations producing them?

Human capital theory sought to trace these differences to the productive value of people. As Becker noted in the first edition of *Human Capital:* "The origin of this study can be traced both to the finding that a substantial growth in income in the United States remains after the growth in physical capital and labor has been accounted for and to the emphasis of some economists on the importance of education in promoting economic development" (p. viii).[2] His approach was to revert to the scarcity principle in classical theory while shifting the emphasis from the commodity to the individual producing it. The main difference lay in "the recognition that labor cannot be measured simply by man-hours since trained persons are more productive than other persons" (Becker 1972). In contrast to conventional labor economics, it is not labor force participation per se, but rather "the *quality* of the members of the labor force that plays an ever larger part in determining the effective labor input in a modern economy (Schultz 1972, p. 9). The chief way of improving the "quality" of labor is for individuals to invest more in education and other "productivity augmenting" forms of human capital. Schultz (1961) drilled this point home at his presidential address before the economic association in 1961, insisting ". . . that people acquire useful skills and knowledge . . . that these skills and knowledge are a form of capital, that this capital is in substantial part a product of deliberate investment, that it has grown in Western societies at a much faster rate than conventional (nonhuman) capital, and that its growth may well be the most distinctive feature of the economic system" (quoted in Schultz 1972, pp. 9–10).

According to Becker's (1961, p. 347) diagnosis, scarcity conditions arose in the United States during the pre-Sputnik period, for the "rate of return [on college education] was significantly higher than the rate earned on tangible capital [which was] evidence of underinvestment in college education." His analysis at the time "did not, however, systematically develop a framework to explain why rates of return and investments differ so greatly among persons" (p. 96), nor the implications of these differences for the changing social value of education.

Social roles and the means-ends dilemma.—Durkheim had earlier criticized

[2] Unless otherwise indicated, all isolated page references are to the book under review.

utilitarianism for neglecting to supply a guiding purpose for human behavior. Traditional society fettered the expression of self-interest but also sanctioned cooperation through established beliefs in the meaning of life contained in personal duties and social obligations. How then, after conquering this moral order, was industrial society to ensure a level of cooperation equal to its emerging division of labor?

Many sociologists believed that social roles explain the problem of order, and so reunited social means and individual ends. Durkheim stressed that however the reconciliation ultimately was achieved it would have to allow for voluntary compliance, while at the same time ensuring the collective expression of social needs. Middle-range theory sought to fulfill both criteria by defining social roles as functionally interrelated sets of internalized expectations transcending specific individuals. First, though real people occupied social roles, they learned but did not define these expectations. The distinction was critical, for it ascribed the content of role functions to externally derived values rather than subjective preferences. Roles were both prior and posterior to the individuals who occupied them.

Second, social roles not only defined the ends of human behavior but also controlled the means by which these ends could be achieved. Resources, like expectations, were embodied in the roles themselves, not the individuals who (temporarily) occupied them. Role incumbents were conferred rights to utilize these resources in the execution of assigned responsibilities but were denied personal sovereignty over their disposition. When a social role was vacated, their rights were automatically rescinded, and the control originally delegated the individual over these resources was revested in a new role incumbent. By embedding performance rights in social roles, middle-range theory was believed to have uncovered the institutional origins of accountability in modern society.

Like the firm in economics, the social role was the basic unit of production in society. Middle-range theory explained this productive role in terms of "the behavior of status-occupants that is oriented toward the patterned expectations of others" (Merton 1967, p. 41). But this "oversocialized conception of man" (Wrong 1961) left little room for the free expression of self-interest. It suffered as a general mode of analysis as well, for as Coleman stated, "Such a strategy views norms as the governors of social behavior, and thus neatly bypasses the difficult problem that Hobbes posed. Why, in short, should there be norms at all?" (Homans 1964, p. 813). By sidestepping the problem of order in this way, middle-range theory failed to specify where role incumbents obtained the resources needed to discharge their social obligations. The resource allocation problem continues to plague efforts at conceptualizing how social structure evolves.

If sociologists have tended to overemphasize the constraining effects of social roles, alternative approaches run the opposite risk of ignoring issues of social control altogether. After faulting this tendency in the "structural functional school," for example, Homans (1964, p. 817) concludes "that there are no general sociological propositions, . . . and that the only general propositions of sociology are in fact psychological." Coleman has developed

the notion of "corporate actors" as a way of tempering this radical individualism, while at the same time allowing for a voluntaristic interpretation of social order: "The overall conception is this: all resources reside individually in natural persons, and these persons make investment decisions. The investments are investments in corporate actors, ranging from a corporate actor that has only one other member (such as a married couple, or a pair of friends) to the encompassing corporate actor of the state" (Coleman 1974*a*, pp. 759–60).

Human capital theory closely resembles this conception of human behavior, tracing resources invested in social roles to the self-interested motives of individuals occupying them. Unlike Coleman's corporate actor model, however, this conclusion is reached without recourse to any supplemental propositions about social structure. Does it follow from this that individual behavior generates social order? Is sociology, in other words, reducible to economics?

Self-Interest and Social Structure

The issue is really twofold. First, is self-interest sufficient to explain the social structure of economic exchange? Sociologists would deny this, arguing that social facts are sui generis and irreducible to elementary principles of human behavior. If so, are social facts not then necessary to explain at least certain aspects of individual behavior? The first issue describes a necessary condition for reducing social facts to economic motives. The second provides a sufficient condition for rejecting this reductionist fallacy. Much of middle-range theory "begins with an image of how social status is organized in the social structure." To assess these two conditions, I will draw upon the related conception of social status as "a position in a social system, with its distinctive array of designated rights and obligations" (Merton 1967, p. 41).

Human capital theory focuses upon the structure of earnings and investment, using rate-of-return analysis to explain how human capital is distributed and who profits from it most. "Essentially all that is in involved is the application to human capital of a framework traditionally used to analyze investment in other capital" (p. 95). In a separate essay on the "personal distribution of income," Becker uses this framework "to explain why rates of return and investments differ so greatly among persons" (p. 96). Investment differentials and earning inequalities are central to the study of social stratification as well. The framework, therefore, has wide-ranging implications for the conceptualization and analysis of status structures in sociology. But, is it sufficient to explain the social structure of resource investment and job earnings? Can it, for example, explain the genesis of social statuses?

The economics of social status.—Job earnings in the human capital model are conceptualized in terms of returns on prior investment activity. Employment characteristics—job complexity, autonomy, bureaucratization, etc.—do not explicitly enter the analysis, "total returns" depending only "on the

amounts invested and their rates of return" (p. 96). Human capital is assumed to be "homogeneous in the sense that all units . . . add the same amount to earnings" (p. 96). Among other things, this precludes people from pooling their human capital which would cause its value to vary systematically if pooling produced cost savings or additional productive capacity. The individual, in other words, is the direct recipient of the benefits accruing from his investment activity, not the firm for which he works or any corporate actor of which he is a member. This view contrasts sharply with the "basic" Blau-Duncan model of occupational achievement (Duncan, Featherman, and Duncan 1972, p. 39), where the effect of investment (in education) on earnings is largely mediated by status differences among work roles. Human capital theory ignores these "structural" effects, deriving propositions about resource inequalities from the value of production factors and these alone. This oversight is understandable though, for to admit such effects into the analysis would jeopardize the conceptual utility of treating the individual as the basic unit of production.

There are three remaining ways of explaining role status consistent with the model as it stands. First, status might be conceptualized as a form of human capital in its own right. In this view, individuals invest in social status much as they do in education, thus reducing status structures to individual behavior. The empirical adequacy of this conception can be tested in one of two ways. First, as Becker (p. 98) notes, human capital is subject to declining marginal productivity—that is, its economic value diminishes with further accumulation. If so, status as a form of human capital should increase earnings, but at a decreasing rate. Declining marginal productivity results from the fact that human capital is "embodied in the person investing" and that individuals—as opposed to collectivities—are physically and mentally "limited" in their capacity to efficiently utilize increasing volumes of resources—much as growing "size" constrains the "entrepreneurial capacity" of firms.

Mincer (1974, p. 92) has experimented with several quadratic functions of this form, finding a significant drop in the earning power of education with increased schooling. Similar estimates for status, however, are not readily available.[3] Fortunately, the homogeneity assumption furnishes an alternative (if somewhat weaker for the additional restrictions imposed) test of the hypothesis. According to this assumption, status (as a form of human capital) is a "perfect substitute in production" for education and other forms of human capital. Assuming, therefore, that marginal productivity declines with human capital accumulation, those investing more in one resource should have less incentive to invest in the other. In other words, if status is a form of human capital, according to the stipulations of the model it should covary negatively with education. All available sociological evi-

[3] It is a little ironic that while the two fields share much subject matter in common economists have yet to learn that "hard" measures now exist for many of the "psychic" benefits constantly alluded to in the literature, while sociologists appear equally unaware of the many insightful statistical formulations which economists in turn have developed.

dence points in the opposite direction—education and status[4] have consistently been found to have a significant positive correlation. There is, however, sufficient disagreement over current methods of measuring social status to justify further investigation of the hypothesis.

A second possibility is to conceptualize social status as a form of "psychic capital"—once again, individually produced but this time representing an earning as opposed to a resource substitute. As Becker (p. 46) notes, " 'real' earnings are the sum of monetary earnings and the monetary equivalent of psychic earnings." An individual, in other words, can take the profits from his investment activity in whatever form he chooses. According to this conception, "observed" (i.e., measurable job earnings) return rates will vary among individuals with comparable stocks of human capital if some possess a greater preference than others for status over monetary benefits. If the former is a true earning substitute, those with more social status should have correspondingly lower observed job earnings than those with comparable human capital but less status.

In statistical terms, the hypothesis predicts a negative partial correlation between status and job salary. Available sociological evidence discredits this hypothesis as well—consistently showing a significant positive correlation between status (as measured by Duncan's SEI) and earnings, controlling for years of education. The possibility remains, of course, that education alone underestimates an individual's total stock of human capital, introducing confounding effects which cannot be sorted out a priori. As a result, this hypothesis also merits further attention with respect to other possible ways of measuring earning substitutes as well as operationalizing human capital.

Finally, social status may be a specialized form of noneconomic capital limited in productivity to activities which compete for time otherwise spent accumulating job earnings. In this view, status measures psychic benefits associated with a style of life which requires time and possibly other economic inputs to be realized. One way of evaluating this hypothesis "is to permit human capital to enter utility functions," with the expectation that those with more human capital will expend a greater fraction on status benefits. However, "given the difficulties in measuring, quantifying, and comparing utilities, this [approach] does not seem too promising" (p. 67).

A much simpler approach for present purposes is to test the conditional effect of prior investment on job earnings for those with differing amounts of social status but comparable stocks of human capital. If status affects how individuals allocate their earning benefits, then the sign on this joint status-investment term should be negative. Conversely, a positive sign or an insignificant interaction effect would cast doubt on the hypothesis. Elsewhere (Sawyer 1977), I have tested this hypothesis on a restricted sample of about 300 full-time employed males with four years or more of higher education, and have obtained a positive sign on the interaction term with education standing as a proxy for total human capital investment. Though these results fail to support the hypothesis, the same caveats apply here as before.

[4] As measured by the Duncan SEI. See Blau and Duncan (1967) for details.

The sociology of economic exchange.—What then is job status, if not a resource or an earning substitute or a form of noneconomic capital? For sociologists, it is a structural characteristic of economic organization which "complements" the earning capabilities of individuals by combining personal resources so as to increase their overall productivity. In economic theory, "persons investing in human capital can be considered 'firms' that combine such capital perhaps with other resources to produce earning power" (p. 102). Treating individuals as economic firms precludes them from capitalizing on these structurally produced economies of scale.

In Becker's model, the "profits on investments in human capital" are measured by the "rate of return" individuals earn on a unit of human capital (given by the slope of its demand curve) adjusted for the "rate of interest" on investment opportunities (given by the slope of its supply curve). In principle, every individual has a unique demand-supply function which accounts for observed differences in earnings and levels of human capital accumulation. The strictly analytic advantages of the theory over comparable sociological models depends, therefore, on the distribution of individual differences within and between identifiable structural categories.[5]

Demand conditions vary because of differences in "ability" in the broad sense of the term—including motivation and other personality "traits" as well as narrowly defined differences in "intelligence." It is not so broad though as to include structural characteristics of learning and work roles which enhance investment and earning capabilities (e.g., job autonomy). Supply conditions vary largely because of "segmentation" in human capital markets, which is traceable to background differences among individuals ("family wealth," "willingness to forego consumption while investing," etc.) as well as market imperfections ("special subsidies or taxes, transaction costs, legal restrictions on lending or borrowing, etc."). Conspicuously absent are institutional arrangements which might have economizing effects on investment costs—particularly, "patterned expectations" about investment opportunities (in the case of job seekers) and technical competence (in the case of employers).

According to the model, "those with higher demand or lower supply curves [or both] invest more than others" (pp. 105–6). Individual productivity is maximized when the benefits of producing an additional unit of human capital coincide with its cost—that is, "when the marginal rate of return equals the marginal rate of interest" (p. 104). Since "all persons are rational and . . . neither uncertainty nor ignorance prevents them from achieving their aims, . . . actual accumulation paths are always the same as optimal [i.e., "desired"] paths" (p. 105). To maximize societal productivity, on the other hand, requires that the "total investment in human capital" be allocated so as to make "the marginal social rate of return . . . the same for all persons" (p. 129)—to ensure that each additional unit of human capital produced benefits everyone equally. In order for this to occur, no one must "earn more partly because their productivity was systematically overesti-

[5] Its ability to generate significant theoretical propositions is, of course, another matter.

mated," nor must "earnings greatly understate the social productivity of anyone" (p. 195)—that is, job earnings must reflect each worker's "true" productivity.

Treating social statuses as investment outcomes is a neat way of conceptualizing this allocative process and should command greater attention among sociologists. For one thing, it gets around the major criticism against functionalist theories of stratification which explain (away) status differences among social roles in terms of "system requirements." With suitable modifications, it can also be made consistent with Coleman's corporate actor model thereby providing "the basis for a kind of 'portfolio theory' analogous to the portfolio theory for financial investments, in which the investor can maximize certain objectives through carrying an appropriate portfolio" (Coleman 1974*a*, pp. 760–61).

Its main deficiency is the failure to explain how these "equilibrium conditions" can be met (i.e., how personal resources can be optimally "combined" so as to maximize productivity). Social roles differ in status because of the way role incumbents allocate their time and other resources. But, as the previous section showed, there is reason to believe that these differences in turn have "structural" consequences for the organization of economic exchange and the expression of subjective choice. This raises the question whether it is not then more analytically expedient to conceptualize production in structural as well as (or perhaps even instead of) behavioral terms.

Complex economic (and political) functions commonly exceed the productive capabilities of individuals, making it personally (and socially) profitable to combine one's resources with others in the interest of more efficient management and greater overall benefits. Social statuses, therefore, need not nor seldom do correspond directly to the earning power of a single person, which is an important (but largely neglected) reason why the returns on human capital investment frequently vary across work roles. Social statuses confer rights of access to a whole range of supplemental resources which role incumbents may use—without charge—to augment their earning power. These rights of role occupancy define how and whose resources are combined, giving rise to performance standards and authority relations in accordance with established rules of resource exchange. When personal resources are pooled for collective gain, the person or institution (e.g., employers, clients, regulatory agencies, or accreditation bodies) conferring these rights (e.g., to hire and fire employees, make medical diagnoses) largely determines how fully personal abilities are utilized and the extent to which productivity and earnings coincide. Consider, for example, the economizing effects of institutionalizing professional standards on the social control of technical expertise.

In Becker's model, an individual's earning power is geometrically analogous to the area beneath the point (of equilibrium) on his investment surface where ability and opportunity curves intersect—as given by the formula, "$E = \bar{r}C$, where E is earnings, C the total capital invested, and $\bar{r}$ the average rate of return on C" (p. 108). The higher this point (i.e., the greater his marginal rate of return) and the further to the right of his investment surface (i.e., the larger his stock of human capital), the greater his

earning power. The individual's ability curve is negatively inclined, it will be recalled, because of diminishing marginal productivity. His opportunity curve by contrast is positively inclined, "primarily" because investment funds are scarce, which causes individuals to "shift from less tt more expensive sources" (p. 103) as additional human capital is accumulated. Thus, there "is a positive relation between the height of a demand (ability) curve, the amount of capital invested and the marginal rate" (p. 111).

In Becker's analysis, the individual's ability (curve) is more or less fixed—that is, "the variables influencing opportunities are more easily and immediately influenced than those influencing capacities" (p. 123). Thus, to maximize his earning power (productivity), the individual must choose that "investment path" or career track which intersects optimally with his ability function—which his prior ability and resource constraints (including "ignorance" and "uncertainty" about future career opportunities) force him to follow. Recent work by Freeman (1971) has shown the "segmenting" effects of specialization on the optimizing qualities of occupational choice. Funding differentials across work fields account for some of the resulting variance in earnings and investment. Training standards exert similar effects, increasing the costs of admission to certain work fields well above expected "break-even" points. In Becker's model, "compulsory minimum investments" produce "a less efficient allocation of the total investment in human capital" (p. 127) by forcing some individuals to acquire more resources than they can utilize efficiently, thereby widening disparities between productivity and earnings.

High-status (particularly, "professional") work fields commonly require much "compulsory" training of this kind, among other reasons because of the increased complexity and thus risk associated with social roles (e.g., physician, lawyer, scientist) where task variability and thus potential for error is great. Task complexity in this sense increases investment costs by raising the informational requirements of performing high-status work roles —an argument sometimes used to justify government subsidies to certain work fields such as medicine. To the degree, then, that "ability" remains constant across work fields, inefficiency in Becker's sense should rise with occupational status, raising serious questions about the management of professional expertise. The problem could be resolved—in part at least—were high-status occupations somehow able to compensate for the inherent limitations of their members. Many, however, would take exception to using recruitment policies to accomplish this purpose because of the invidious implications of allocating life chances on the basis of "inherited" inequalities. But restricting the concept of ability to individual competencies seems to leave no choice.

Sociologists (Freidson 1970) have noted the tendency for occupational power and personal autonomy to rise with job status, furnishing a way of widening this concept and of resolving the seeming paradox it creates. Power and autonomy are structural attributes of social statuses, and when legitimized under institutional auspices become earned "rights" of role occupancy. Among other things, they give role occupants access to the personal re-

sources of others participating in a production process. In Coleman's (1974*b*, p. 43) corporate actor model, the individual surrendering this control does so in exchange for "the hope and expectation that his loss of control over these resources will be more than balanced by the greater benefits they will bring him when combined with others." To the degree that one person's human capital complements another's, both stand to profit from the increased benefits of pooling their resources. Interoccupational status differences reflect these economies of scale, power and autonomy being (two of) the "terms of exchange" by which these resources are combined: "The attainment of ego's goals then becomes dependent on the relational context in a double way. What he gets depends not only on what he himself 'produces' in the sense in which this is independent of what the alters do, but on the 'terms of exchange,' that is, the patterning of his relationship in certain respects to the relevant alters" (Parsons 1951, p. 70).

This "reciprocity" principle increases in importance with the differentiation of production processes (e.g., health care delivery) which increases resource complementarities and investment differentials. The ("patterned") expectation that loss of control will produce greater benefits generates cost savings for "lower-order participants" such as medical technicians by relieving them of the managerial responsibility of deciding how their resources can most efficiently be utilized. In Becker's model, willingness to relinquish control lowers the investment costs of participation (i.e., of acquiring the necessary managerial expertise), thus increasing the earning power of those with a "taste" for health occupations but insufficient ability (motivation, tolerance for risk, IQ, etc.) or opportunities (family wealth, willingness to forego consumption, etc.) to become physicians.

Their willingness to relinquish resource control in turn augments the productive capacities of physicians, enabling them more efficiently to utilize the human capital required for occupational admission. In Becker's model, structural differentiation and resource pooling increase the overall "entrepreneurial capacity" of health delivery firms, moving their ability curve upward toward equilibrium and maximum productivity. Among other things, this structural model of investment behavior would seem to explain the observed trend away from private fee-for-service health firms toward larger, more differentiated delivery systems better able to efficiently utilize modern medical technologies.

Much the same argument can be used to explain the previously observed tendency for status to increase the economic value of education. Above, it was shown how economists might conceptualize social status in relation to investment behavior. The third hypothesis treated status as a form of noneconomic capital requiring time taken away from earning activities. A positive or nil interaction between status and education was given as grounds for rejecting this interpretation. This result can also be shown to recommend a structural interpretation of social status.

From an employer or consumer standpoint, job complexity entails risk, thereby increasing the cost of supervising professional performance. If professionals however were willing (and able) to supervise themselves, as effi-

ciently as or more efficiently than their employers or clients, the latter would have an economic incentive either to pay them to be autonomous or at least not to charge them for the privilege. In the former case—where autonomy is repaid—there would be a positive interaction between status and education, reflecting payment for the managerial costs of task complexity; in the latter—where autonomy is granted and assumed freely—the effect would be insignificant, perhaps because of compensating benefits derived from a more flexible work style. The observed result is consistent with this interpretation, providing support for the structural approach. By implication, it also supports the traditional model of professional-client relations which emphasizes the efficacy of socializing mechanisms and collegial forms of social control as methods of institutionalizing technical expertise.

Reasonable grounds exist, then, for believing that human capital theory neither subsumes nor even seriously competes with sociological models of human behavior. The two fields share substantial subject matter in common which is cause in itself for encouraging greater cooperation. Moreover, many conceptual features of the two models appear to be complementary, providing hope that combining resources would more than compensate each for the costs of furthering mutual understanding.

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Review Essay: The Rationality of Political Man

The Politics of Rational Man. By Robert E. Goodin. London and New York: John Wiley & Sons, 1976. Pp. viii+210. $14.95.

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Robert Goodin's book represents political theory in the rational choice genre pioneered in the fifties and early sixties by writers such as Buchanan and Tullock, Riker, Schelling, Boulding, Harsanyi, and Arrow, who built on the 200-year-old foundations of Bayes and Bernoulli, and on the more recent and mathematically sophisticated theory of games of von Neumann and Morgenstern. Moral philosophers were introduced to such tools by R. B. Braithwaite, and since Rawls wrote "Justice as Fairness" in 1958 a veritable industry has developed in social philosophy and political theory for the manufacture and servicing of rational choice models.

Common to all such models is a sparely elaborated notion of a rational agent ready equipped with a well-formed preference ordering—one which is complete and transitive, its preferences asymmetrical and its indifferences symmetrical. He is equipped too with a disposition to optimize, that is, to act so as to achieve from among the positions accessible to him whichever one he prefers. These are formal conditions. The one substantive constraint essential to such a theory is some variant of egoism, though often of a kind consistent with a limited altruism. Goodin works for the most part with Wicksteed's "non-tuism" postulate, that is, "that individuals do not take into account the interests of those *with whom they deal* [Goodin's stress] in constructing their own preferences" (p. 13); but he sometimes specifies a stronger requirement, namely, that "political actors pursue egoistic goals . . . [aiming] at maximizing realization of self-interest" (p. 15). Moreover, "self-interest is here narrowly conceived as the amassing of wealth and power" (p. 16). An agent's wealth is "his ability to purchase goods and services, and his power (in the broadest sense) consists in his ability to control events" (p. 16).

The attraction of the non-tuistic postulate is readily felt. Rational choice theory cannot handle an agent who is altruistic and non-tuistic by turns. Though we know intuitively that that is what many of us are, the theoretician of rational choice has to reduce either the altruism to egoism or the egoism to altruism. Without that reduction, the agent's behavior would be in principle unpredictable within the optimization model. Provided one accepts the interpersonal comparability of utilities, a coherent and wholly altruistic theory can be worked out; but we know enough about human

nature in politics not to expect altruism-based theories to fit the facts. Non-tuistic egoism is the more economical, as well, alas, as the more plausible option. There are two ways of deriving a model of political action from it. All a person's goals, including the selfless pursuit of the welfare of others, can be included among "his interests," in terms of which he attempts to optimize; pursuing his altruistic preferences is then just a special kind of egoistic optimization. Such a theory, though formally a theory of rational egoistic choice, rules out nothing; because literally anything might happen without disturbing the theory, it has no explanatory or predictive power. The other possibility is to take a tough line, ignore any occasional mutualities of concern, and see what happens. This is what Goodin does; and he claims that his model both seizes "the core of the notions of political morality . . . currently embraced in the Anglo-American linguistic community" (p. 93) and predicts pretty accurately the kind of institutions to be found in the United States and Britain.

Goodin sees himself as explaining the fact of "social co-operation from a basically conflict-oriented perspective," the key notion being that of "a coordination problem" (p. 198), that is, one that arises when "it is rational for *all* agents involved to prefer joint to independent decision-making" and when "independent decision-making involves risks of disagreeable outcomes for everyone involved" (p. 27) since the expected payoff for each from coordinated decision making is higher than the expected payoff without coordination. Coordination problems arise when decision makers are confronted with Prisoners' Dilemma or Chicken Game situations, in which, *supposing independent decisions*, the strategies required for the coordinated solution are not optimal for any individual player. By altering the expected payoffs, however, social organizations, and particularly coercive governmental institutions, provide each individual with a sufficient incentive to adopt the strategies of the coordinated solution, to conform, that is, to mutually beneficial rules rather than to act as a free rider.

Coordination is only one possible solution to such problem situations. Another, sometimes available, is domination of the weak by the strong. But "domination is an attractive policy only to someone who is secure in the knowledge that his greater power is sure to carry the day" (p. 53). A rational person could not have that certainty in a large and complex society in which, Goodin believes, no one can really be sure of the future and of the extent of his own power vis-à-vis countervailing forces. Moreover, no one can afford to get a reputation for untrustworthiness in dealing with some for fear of a backlash from powerful others on whom he relies. These uncertainties are Goodin's substitute for Rawls's "veil of ignorance."

But Rawls does not claim that the reasons that would motivate rational agents to accept the principles of justice in the "original position" apply also to rational agents in our own society who know very well who they are and can assess the probable outcomes of their available strategies here and now. Goodin, by contrast, makes the empirical and dubious claim that there are indeed crucial uncertainties that, in some modern societies at least, make it rational for non-tuistic egoists to accept norms of political

morality, in particular the "Aggregative Principle," the "Works Principle," and the "Fairness Principle." "The Aggregative Principle urges maximization of the total stock of goods available to a society as a whole" (p. 63). According to the Works Principle, "each individual should prosper according to his own works" (p. 64). The Fairness Principle presupposes "a hypothetical baseline in which no one's activities affect anyone else. . . . Movements from that baseline are fair if and only if appropriate compensation is made," either *to* the one who initiates the change, supposing him to confer a benefit, or *by* him to anyone who suffers by the change, the amount due being at least that which would restore the one who bears the cost to his previous condition (p. 64). Everyone must be made to internalize the external costs of his gainful operations. Goodin insists that one does not need interpersonal comparability of utilities to make this theoretically coherent. Each person is the proper judge of his own welfare; all that is necessary is that each should honestly declare the extent of his loss. So, if X fouls Y's water supply, X should pay Y what Y reckons would make up for it (pp. 64–65).

Goodin justifies the first part of the Fairness Principle by appealing to the opportunity costs borne by the benefactor. But this does not square with the claim that, since the beneficiary should not be required to pay back more than he considers himself to have gained, it is for him to assess the compensation due. If the reason for compensation is that the benefactor X has incurred a loss P in conferring the benefit, he would not be adequately compensated should Y assess the benefit he has received at no more than $P - N$. Dealing with the distribution of the profits of joint enterprises, Goodin calls the Works Principle to the aid of the Fairness Principle. But I see no similar move available to him to extricate himself from this difficulty. Another example of a somewhat slipshod interpretation of key principles is to be found in Goodin's claim that the principles determining damages for breach of contract recognize his Fairness Principle: "A man who breaks a contract must make a payment to the other party to the contract in an amount which will leave him as well off as he would have been had the contractual obligation been performed" (p. 86). But this does not square with Goodin's Fairness Principle, which seems to require only that the contract breaker pay so much as would restore the injured party to the state he would have been in had the delinquent party not disturbed his baseline position by contracting with him. It does not seem to require that the injured party also be compensated for the further benefits promised but not conferred.

Perhaps Goodin would argue, in his own defense, that in the contract case the baseline should be taken to be, not the situation immediately prior to the contract, but that brought about by the contract itself; the disturbance to be rectified would not be that from precontract to postbreach situations, but from postcontract (including all the benefits involved in contractual *entitlements*) to that consequent upon the breach. A great deal of the predictive and explanatory thrust of Goodin's theory will depend on what baseline is selected for any given purpose. One of the strengths of his

theory, Goodin claims, is that it does not entail the affection for property rights commonly thought to be a necessary feature of rational choice models. Conservative theorists, it is true, take the existing property distribution as the baseline, and for them government resumptions of title, for instance, would constitute disturbances that demand compensation. Radicals, on the other hand, criticize the existing distribution on the grounds that it does not satisfy the Works and Fairness Principles, and justify redistributive policies as compensating for earlier unfairness. Goodin claims that his model will fit radical politics at least as well as conservative; the differences require only a few deft adjustments to baselines, which are exogenous, not endogenous, elements of the theory.

It is always a bit of a problem to know what theories of this kind are really about. Goodin recognizes, in his preface, that his book will "find as many interpretations as it finds readers," and recognizes an author's "obligation to state his intentions." But he does little to meet that obligation. "The diversity of tenable interpretations," he says, "tells us something important about the rational choice model of politics, viz., that it is a powerful mechanism for theory-integration" (p. v). And that is about as far as he goes. One would like to know, for instance, whether Goodin sees his model as applicable only to the "Anglo-American linguistic community," whose linguistic practices he examines for evidence of the norms of political morality and whose laws he takes as evidence of the correctness of his postulates. The explanatory scope is clearly limited to communities in which an attempt by some minority to dominate the rest cannot reasonably be expected to pay off, since "all rational actors would prefer dominating others to coordinating efforts with them, provided they thought they could get away with it costlessly" (p. 53). In chapter 12, on the other hand, he examines "Radical Rejoinders" to the dominant norms of political morality and claims to find his key principles underlying them, too. That is not to accuse him of inconsistency; one can see, if somewhat obscurely, which bits of the theory are universal postulates (e.g., the rationality conditions), which are subject to special social conditions (e.g., the conditions presenting coordination problems), which are the conventional outcomes of those conditions (e.g., bourgeois political morality), and so on. It would have helped, however, to have had the methodological structure more plainly presented.

Radicals will hardly be satisfied with Goodin's claim that there is nothing essentially conservative about the theory. The only radicalism he caters to is of the individualistic kind that seeks to rectify unjust distributions. Even here he is incredulous of the brand of egalitarianism that values equality for itself. "Equalizing for the sake of equalizing cannot be the goal" (p. 113) since men differ so widely in their tastes, natural attributes, and so on. But there are surely some egalitarians who put a very heavy stress on the social determination of such differences and would regard it as a proper political objective to iron them out as far as possible. "Equal but different" is for some radicals the slogan only of the crypto-elitist; for them, societies are better, their solidarity greater, their interpersonal relations and under-

standings firmer, the more alike in tastes and life-styles their members are. Such a society may be no more to Goodin's taste than to mine, but we cannot simply dismiss absolute egalitarian policies as irrational.

A still more radical critique would challenge the non-tuistic postulate that lies at the root of the theory. In his final paragraph Goodin writes: "People *do* care about one another, or at any rate about certain specific others. Mutual affection often extends far beyond the family and sometimes manifests itself in the most political of ways. For anyone wishing to extend the present work from within the rational choice framework, this seems a logical starting point" (p. 200). I doubt, however, whether the rational choice theory can be extended to Gemeinschaft-like relations. Concern that there should be consensus cannot be treated as one goal among others. It is not only that tuistic values complicate the equations, but that Gemeinschaft relations are themselves a valued element. It is a common aim of kibbutz policy, for instance, that the caring relation between *chavcrim* shall not become a casualty to the pressures of individualist self-expression.

Rational choice theories are not only non-tuistic; they are also instrumentalist and consequentialist. That this constrains their explanatory capacity is evident at a number of points in Goodin's theory; for instance, in his reluctance to allow not only a substantive value to equality, but also any but an instrumental value to freedom (pp. 119–20). The notion that respect for personal dignity could be a rational constraint on action has no place in Goodin's theory; nor could it have, since even an altruistic utilitarian consequentialism would have no place for it. Respect for someone may demand not only that one accepts some outcome that is less than optimal for oneself, but also for him, if that is what he chooses. One may owe it to him not only to deny one's own self-interest in deference to his rights, but to deny his interests too; one may simply have to watch him make a fool of himself, knowing that no one will be the better for it. Goodin does less than justice to the deontologist. He claims that even a deontologist would recognize the need to trade off the social duty to punish the guilty against a variety of duties, like feeding the poor, that compete with it for resources. What he does not recognize is that a deontologist would not condone the punishment of the innocent, whatever the payoff.

A similar blind spot is evident in Goodin's discussion (chaps. 14–16) of environmental problems and policy. There is no hint of an understanding of the conviction of many environmentalists that some natural objects have an intrinsic, not an anthropocentric, instrumental value. How, for instance, would Goodin explain the "green bans" that Sydney builders' laborers put on environmentally objectionable projects? Or the environmentalists' concern for wilderness?

Of course, if one is content with a theory that accounts for macroscopic features of a particular range of societies at a particular historical epoch, these criticisms may be of little importance. If, for instance, deontological moral motivation is relatively weak, socially speaking, it will not be a necessary causal element in many political events, so that a theory that has

no room for it will not be greatly prejudiced. Such theories, however, are not free of ideological overtones. If Gemeinschaft-like societies, or policies putting ecological before human interests, or policies allowing for quirks of personality that interfere with socially "optimal" policies, are characterized as irrational, that is not simply the application of a theoretical term within an explanatory theory. The ambiguity of Goodin's book derives precisely from the fact that, despite his disavowal of justificatory intentions, one never knows for sure how far the characterization of an institution or a policy as one which a rational agent would accept or approve, amounts just to a description within a theory, how far to a commendation.

The dissatisfaction expressed in this review has more perhaps to do with the genre of rational choice theory than with Goodin's performance within it. Though one can pick holes in particular sections of the argument, and one may find some of the empirical postulates somewhat implausible, Goodin has not lost sight of the fact that political theory is about politics, and he has made a commendable attempt to show the relevance of the theory to policymaking, particularly in the field of environmental control. The book is commendably sparing, too, in its use of the kind of equations which so elaborately ornament the pages of many a similar book, and which add nothing to our understanding that could not readily be said in English prose, because no one tells us how to assign values to the variables. It would be unfair, however, to readers of the book not to alert them to two pitfalls which did waste my time. In equation 1 on page 89, the first minus should be a plus sign. Goodin avoids assigning a negative utility to U_p (the utility of a penalty) by specifying that all the utilities, including U_p, are to be taken as absolute values and therefore greater than zero. Nevertheless, the expected utility of the criminal strategy is not the difference but the sum of the utilities of its possible outcomes, success and failure (U_c, U_p), each discounted by its respective probability (p_s and $1 - p_s$). For Goodin's analysis of rational crime to go through, this sum must exceed the expected utility of acting legally (U_l). Second, in matrix 15.1 (p. 166), the four sets of payoffs must be diagonally transposed if the game is to be Environmental Chicken, as claimed.

Book Reviews

Sociological Ambivalence and Other Essays. By Robert K. Merton. New York: Free Press, 1976. Pp. xii+287. $12.95.

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This mélange spans four decades, from "The Unanticipated Consequences of Social Action" (1936) to a paper on presidential commissions published in 1975. There are four essays each from the period up to 1948, the decade 1957–66, and the 1970s, with a new postscript on ambivalence among scientists making a "baker's dozen." They treat of race relations, physicians, voluntary associations, science, business firms, and general sociology.

Such a mélange can be read in different ways. One can take it as a sampler, skipping from piece to piece as taste inclines. If you read the present collection thus, be prepared for two things. Be ready for swings in levels of theoretical sophistication, but not of trenchant prose, as the author adjusts to audiences ranging from nurses and the lay public to professional colleagues. And be ready to find the older essays almost as gripping, if not so comprehensive, as the better-known early essays reprinted in *Social Theory and Social Structure* (New York: Free Press, 1968). You will find, for example, words written as early as 1948 by someone labeled occasionally with the "conservatism" of "functionalism" which depict the "ethnic liberal" as a person at spiritual ease, neither prejudiced nor discriminatory, but nevertheless "tending to contribute to the persistence of prejudice and discrimination through his very inaction" (p. 195).

One could, on the other hand, respond more actively by seeking to fathom some commonality of approach in the author's treatment of such a medley of themes. The volume's title almost invites us to do this, even though the author himself makes no effort to impose that kind of reading. I personally found the cue irresistible.

Arthur Stinchcombe has already blazed the trail for a hermeneutic scanning of Merton's work. In his contribution to *The Idea of Social Structure* (New York: Harcourt Brace Jovanovich, 1975), Stinchcombe argues that Merton was wrong to disavow claims to being a general theorist. Rather, Merton's work has been so empirically fruitful precisely because he has sustained, if only implicitly, an elegant and powerful general theory—one which transcends the tedious dichotomy between the "social" and the "psychological" by conceptualizing patterns of action in terms of "the choice between socially structured alternatives."

Taking the core idea from the title essay of *Sociological Ambivalence*, I would amend this formula to read "socially structured alternatives typically

presented in dualistic form, with persisting interest in the foregone alternative only temporarily if at all." Roles are thus to be analyzed no longer as coherent sets of normative expectations but as clusters of norms and counternorms which codetermine action. Scientists feel obliged to publish quickly and to avoid rushing into print, to value humility as well as take pride in originality; physicians are socialized to show sympathy as well as detachment; business leaders are expected to project a sharply defined vision of their firm's future and to avoid narrow commitments which distance their subordinates, to provide special facilities so departments can perform well, and to subordinate departmental goals to those of the whole organization.

The first part of this revised formula—that actors must choose between socially patterned dichotomous alternatives—bears some resemblance to Parsons's notion that actors face dilemmas of choice structured in terms of the pattern variables. For Parsons, however, as for Sorokin, social relations are to be characterized in terms of the dominant pattern alternative they embody, albeit in full awareness that some aspects of any concrete role are likely to be left out when it is so characterized. Merton differentiates his position from that of Sorokin and Parsons by stressing the need to analyze that residual domain, to delineate the minor counternorms which alternate with major norms in social roles. A small difference, perhaps, but one big with consequences (to paraphrase one of Merton's favorite quotes from Poincaré).

Is it fair to suggest that this attention to sociological ambivalence—the socially structured sources of dispositions to embrace and reject the same object or norm—informs the entire Mertonian corpus? Not really; the idea was first adumbrated in 1957 and formally articulated only in 1963. (It may be relevant to note that these formulations followed Merton's fine-grained reexamination of the work of Georg Simmel, an author distinguished for his emphasis on the dualistic character of all social formations.) It is fair to suggest that a pronounced—I would say extraordinary—sensibility to the dualities of social life informs Merton's work early and late. I say extraordinary because it ran counter to the overwhelmingly "dominant" tendency of American culture generally and American sociology in particular, to eschew dualistic constructs in favor of univalent statements. To say this is to raise more questions than can be addressed here, but the diverse roots of this cultural tendency may be said to include: residues of Puritan asceticism, populistic plain talk, the norms of a formally rationalized law and economy, bureaucratese, and the thoughtways of modern technology and much natural science. Its manifestations in American sociology include the search for dominant patterns, univalent metrics, monochromatic path diagrams, and unilineal logical derivations.

Whatever else nourished Merton's exceptional sensibilities in this regard, they clearly were fortified by his uniquely open commerce with the European sociological tradition (on this latter, see Lewis Coser's piece in *The Idea of Social Structure*). Merton read deeply enough into Marx, Durkheim, Simmel, Weber, and Mannheim that he could not forget the ways in which progress begets misery, crime promotes morality, intimate private acts are socially

induced, conflict enhances cohesion, cultural growth creates culturally impoverished persons, and rationality both derives from and stimulates irrationality. Far more than any other American sociologist, Merton incorporated this European awareness of the fundamental paradoxes of social life into the lens of his sociological vision.

If, then, the early pieces in this collection do not deal with sociological ambivalence, they can be seen to exhibit a concern with kindred matters—with attitudinal dualisms, with the oppositions between attitude and conduct, with the divergence between the intentions and consequences of conduct. In an early critique of Thurstonian attitude scales ("Fact and Factitiousness in Ethnic Opinionnaires" [1940]), Merton faults certain instruments for a number of methodological and substantive inadequacies, including the fallacious "suppressed premise . . . that subjects do not 'really' subscribe to 'logically' contradictory judgments" (p. 259). Perhaps contemporary survey research would benefit from reconsidering the point that internal consistency of social opinions is a rarity, not the rule. In a related vein, Merton elsewhere observes that studies on the social ratings of occupations, which consistently attest to the high prestige of the professions, have failed to tap the widespread hostility that has also been directed against the professions over the years, since their research tools "often do not expressly include statements of the negative as well as the positive bases of evaluation" (p. 20, n. 37).

It is a short step from acknowledging that people hold contradictory opinions to determining that their actions can and frequently do diverge from their beliefs. Myrdal et al. were wrong to suppose that because the conduct of Americans diverged from the American Creed a dilemma was produced which would have to be resolved willy-nilly in favor of the Creed. Unprejudiced people could act in discriminatory ways ("fair-weather liberals"), just as others could act in nondiscriminatory ways and yet personally reject the Creed ("fair-weather illiberals"). Attitudes are not irrelevant (*pace* Herbert Blumer) but a variable of intrinsic interest, even though they provide no reliable basis for predicting conduct.

Knowing the course of a particular action, moreover, is no clue to predicting its outcome. Intense pursuit of a given goal might well be the surest way to avoid reaching it. Merton's famous essay on this topic reached its goal, in a way—that of directing attention to the need for a systematic and objective study of the elements involved in the development of unanticipated consequences of purposive social action—but it cannot be considered seminal yet, for no one whose attention was so directed has gone on to fulfill that need. Merton himself, at any rate, would go on to write, four decades later, of presidential commissions that they are typically activated in response to some acute social problem, occasions which are not necessarily the most suitable times for carrying out the studies assigned, and that their format, one which provides for minority reports, paradoxically produces wider consensus than would otherwise obtain.

Engaged by this mode of analysis, the active reader may be moved to

demand further clarifications. What, he may wonder, is the relationship among those three recurrent pairs of concepts: norms and counternorms, contradictions and balanced opposites, functions and dysfunctions? Many readers, standing on Merton's shoulders or jogging by his side, are likely to find themselves stimulated by these pages to pursue such questions further.

It is, in sum, that multifaceted alertness to the dualities of things—including what have been called, perhaps all too loosely, the ironies of social existence—that I would identify as a key to reading this book as a more unified set of essays than readily appears. Not that Merton is ill at ease in handling irony in the proper, that is, literary sense of the term. His deadpan treatment of one of the critics of his work on science, a man whose remarkable inventive powers led him to perfect the technique of "expropriate-and-reproach," is alone nearly worth the price of the book. Such glosses only embellish the luster regularly displayed by a series of exposés of the fallacy of taking expressed univalent attitudes or clear-cut deliberate actions at face value. What Wayne Booth, in *A Rhetoric of Irony* (Chicago: University of Chicago Press, 1974), has written of the literary form—that irony risks disaster, but if it succeeds, it heightens our alignment with the author beyond what would be produced by any literal statement, even the most passionate—may have some bearing on the alignment we feel with one who has masterfully articulated the ironies of social action.

Young John Dewey: An Essay in American Intellectual History. By Neil Coughlan. New York: Free Press, 1976. Pp. xii+187. $10.00.

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Young John Dewey is offered by Neil Coughlan, historian turned law school student, as an "Essay in American Intellectual History." Coughlan covers the period of John Dewey's education and intellectual growth as well as his writings up to 1904 when he left the University of Chicago. The study is of real relevance to more than that group of spirited and energetic sociologists who think of themselves as students of symbolic interaction. John Dewey stands at the center of the American social science enterprise. As a result, this volume deals with the emergence of those philosophical themes which have shaped the development of sociology in the United States. Its richness and subtlety stand in contrast to much of the oversimplified contemporary writings by sociologists which deal with the intellectual ferment of this period. It serves to confirm my belief that the preparation of the history of the social sciences, including sociology, depends on the contributions of historians rather than sociologists.

Coughlan claims that he is not primarily interested in the sociology of knowledge—how the organization of the university or the social and political

currents in the larger society influenced the development of Dewey's thought. But, of course, he cannot completely escape these topics. Likewise, he does not focus on the psychological makeup of John Dewey, but again he cannot avoid an exploration of the moral setting and dilemmas in which John Dewey found himself.

Intellectual history for Neil Coughlan is the exposition of the sources of a man's or woman's ideas, that is, the natural history of their development and transformation. It encompasses the interplay of one set of ideas with another and the results of conscious efforts by intellectuals to reformulate these ideas and give them a new coherence. As a result, Coughlan emphasizes the element of autonomy in intellectual life, which is a profoundly useful perspective for those sociologists who are chronic reductionists. Moreover, the book contains a careful exposition of the academic and intellectual biographies of the men who influenced John Dewey. From these materials he offers a careful explication of the central themes and changing content of John Dewey's own writings. Neil Coughlan strains to be an effective stylist in the contemporary mode, which is a deliberate reaction to the presumed jargon of the behavioral sciences; however, let me quickly add that these efforts to be elegant and literary do not interfere much with the exposition of the book's substance.

And the book has plenty of substance for sociologists. In particular, it lays bare the complex and rich set of ideas which stimulated the development of pragmatic thought and the linkages between pragmatism and social research. It is particularly penetrating in its presentation of the slow and painful process by which John Dewey freed himself of his Hegelian background and fashioned those elements of pragmatism which have stimulated social research in a theoretical context. It is a real contribution to the ongoing—and never-ending—process of "deEuropeanization" of American thought which needs to be repeatedly pursued if sociologists are not to be trapped in the pretentious quagmire of the idealist-materialist posture which is destined to continue without end.

The striking thing about this study of John Dewey's monumental achievements is the range of his concerns. He persisted in thinking of himself as a philosopher in order to maintain an overview of the social sciences. It is just not the case that it took the events of the Great Depression of the 1930s to mature social scientists into a concern with the social consequences of their intellectual efforts. These issues were inherent in Dewey's mode of analysis and in his efforts to relate his academic goals to his civic responsibilities.

Likewise, it is striking that the development of his core ideas took the form of a series of gradual developments, of creative writings which had to be reformulated and reformulated again. Coughlan traces the step-by-step shift from moral philosophy to the epistemology of the social sciences. If there is need for additional evidence that Thomas Kuhn's paradigm of revolutional transformation does not apply in the social sciences, the intellectual biography of John Dewey should not be overlooked.

The study also confirms my belief that we should avoid simpleminded notions that religious background was a propelling force in the development of sociology and social psychology. Of course, there were men and women who thought that sociology was a natural extension of their Protestant social gospel background, and these impulses influenced their sociology and sustained their civic-mindedness. For them, social research was at least compatible with their religious motives and aspirations. But it was also the case—and perhaps more important for the intellectual as opposed to the professional development of sociology—that there were intellectuals for whom there was a clash, modulated or explicit, between religious thought and the development of the social sciences. John Dewey belonged to this category. He was one of those who struggled hard to work out for himself the compatibility and incompatibility of traditional thought with the knowledge generated by social inquiry. In a period in which some sociologists have not retreated into formalism or ideological rhetoric, it is invigorating to realize that the debate over the limits of the scientific method for creating a system of morality and social control coexisted with the intellectual concerns of the original pragmatists.

The study ends with a special effort to recount the intimate and complex relations between John Dewey and George Herbert Mead—a subject of topical interest because of the vigor of Mead's contemporary disciples. Coughlan unfortunately submits the brief and, in terms of his analysis, "ex cathedra" judgment that Mead's writings are probably more important in the intellectual world today than Dewey's are. The book neither analyzes Mead nor deals with the present influences of either man. As one considers the continuities and discontinuities in sociological thought and the present state of the corporate memory of the sociological community, I believe it is worthwhile to call attention to the assessment of this concluson by Lewis Feuer, with which I agree. Feuer wrote in the *Times Literary Supplement* review of Coughlan's book, "The statement seems to be dead against the evidence." He adds the pointed observation, "The sociologist W. I. Thomas, who worked with both Dewey and Mead in their Chicago years, once said that Mead has one simple idea which he kept repeating all his life as if it were a great profundity; Thomas found it rather obvious." By contrast, "he [Thomas] regarded Dewey as America's 'medicine man.' This was perhaps reductive anthropologism. But it is certainly true that Dewey's sociological pragmatism is still a major premise in much of the world's thinking."

It is not my intent to stir up or prolong intellectual controversy among colleagues, especially since Anselm Strauss himself has designated the work of my colleagues and myself in the study of urbanism as an expression of symbolic interaction, which I believe it is. It is rather that I wish to emphasize that the impact of philosophical pragmatism on sociology is multifaceted and profound and not the result of a single concept or a single figure.

Social Theory as Science. By Russell Keat and John Urry. London and Boston: Routledge & Kegan Paul, 1975. Pp. x+278. $21.75 (cloth); $10.00 (paper).

Evidence and Explanation in Social Science: An Interdisciplinary Approach. By Gerald Studdert-Kennedy. London and Boston: Routledge & Kegan Paul, 1975. Pp. ix+246. $20.25.

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Pundits in the social sciences, particularly sociology, have never left the problem of philosophical assumptions and implications to philosophers alone. From Saint-Simon through Max Weber and the recent phenomenological schools, the status of the social sciences as sciences has been continuously discussed, questioned, and reexamined. It is not that the social sciences are still young, as some like to argue apologetically; it is that the subject matter itself is too big and too close to home to be left undiscussed or to be left to any select group of philosophical specialists.

The two books under review indicate a recent surge of interest in the methodology of the social sciences writ large. Both of them delve into the epistemological, ontological, and logical assumptions of social theory. Keat and Urry assert that the debate over social science has been conducted in terms of only one conception of science, that of positivism. According to them, there are three views of science, distinct from each other: positivism, realism, and conventionalism. The authors are particularly concerned with the distinction between positivism and realism. Positivism, according to them, confuses explanation with prediction, that is, it claims that phenomena are explained when any statement about them can be subsumed by means of deductive necessity under another statement of a higher order of abstraction. This is supposed to explain the phenomena and at the same time to predict their recurrence. In positivism it is only observation which provides the logical foundations for scientific theorizing, and hence scientific laws do not express any causal or logical (*sic*) necessity but only nonnecessary, contingent relationships (p. 14). In effect, the positivist approach to science, the authors argue, enables us only to predict but not to explain.

The scientific realist, however, attempts to provide us with the why of phenomena, that is, causal explanations of the occurrence of events. Instead of attempting to establish the logical necessity between the explanans and the explanandum, as is the case with the positivist deductive-nomological models, the realist tries to discover the relations of "natural" necessity that exist in the physical world. The realist, according to the authors, assumes the reality of a structure behind an observable phenomenon, and it is this structure that is the explanation, or the cause, of the phenomenon.

Indeed, the problem raised by the authors—that of logical and ontological necessity—is an important one. Unfortunately, in their hurried attempt to divide scientists into two camps, they fail to spell out explicitly the philo-

sophical dimensions of the problem or to make reference to the old debates over it. The trouble is that the authors have built up straw-man conceptions positivism and realism. Only Hempel is quoted as an illustration of the positivist assumptions, and most of the time the argument is for or against a "camp." They certainly do not speak for me; I consider myself an epistemological realist, but would not subscribe to their description of it.

The next step is to fit names and theories into the camps. Unfortunately, force fitting is the consequence. Comte, Spencer, Durkheim, Mead, and Parsons are all lumped together into the positivist category. The discussion of these, in particular of Mead and Parsons, is amazingly superficial. One reason the authors take up Parsons in this context is to demonstrate the validity of their previous claim that much of American social science between the 1930s and the 1960s was positivist. No discussion is given of Parsons's empirical system, no treatment of the problem of abstraction, nor any indication of any understanding of the goals and problems of analytical sociology.

The authors issue themselves a warning that they must avoid interpreting realism as synonymous with Marxism. Yet, in effect, only the Marxist variety of realism is held to be of value. Lévi-Strauss, Chomsky, and Althusser are discussed, and a useful critique of their ideas is presented. Nobody else is given a chance. How about Simmel's, Sorokin's, Znaniecki's, Blau's, let alone Merton's and others' realist views of social structure? Even in discussing Max Weber's analysis of the causal relationship of the Protestant Ethic to capitalism, they forget that for Max Weber the Protestant Ethic was a necessary but not a sufficient condition for the emergence of capitalism.

In their attempt to refute positivism, the authors somehow ignore the fact that affirmation of ontological reality of social structure does not absolve one from formulating abstract concepts about it and operationalizing them for testing. To consider structure as an abstract formulation and to look for empirical indicators of this formulation does not of itself deny the ontological reality to which this formulation refers. On the contrary, for most practicing sociologists it is an attempt to discover the properties of this reality.

Had the authors conceived the problem in this way, perhaps they could have come to see the value of the methodological contributions made by many discerning students of positivism, such as Lazarsfeld and Coleman. The authors' emphasis on social structure as the phenomenon "underneath" all appearances and their neglect of the problem of abstraction lead them to appear to deny the empirical dimensions of social structure. This is more like a Kantian or idealist's than a realist's position.

There is, however, much interesting ratiocination in the book, and the authors' treatment of the problem of falsification and value freedom is quite useful.

Studdert-Kennedy addresses the issues which Keat and Urry take for granted, issues such as the nature of social structure: Is it theory or fact, explanation or evidence? Happily, the author builds no straw-man distinctions. In discussing these issues, the author selects a number of classical

studies in anthropology, those of Maquet, Radcliffe-Brown, Evans-Pritchard, Sahlins, and V. W. Turner, and analyzes critically the types of structural explanations offered by each. By the time all these studies are discussed, the various versions of structural explanation are presented. Two issues underlie the discussions throughout the book: how one comes to identify structure from observation and whether the concept of structure is feasible at all if the temporal, sequential (i.e., historical) aspects of behavior are the primary focus of inquiry.

The last issue, however, comprises two questions. One is whether structure makes any sense at all from the historical perspective, and the other is whether one has to take a long period of time into account to establish whether there actually is a structure. Somehow the two questions seem to be confused in the book.

Furthermore, Studdert-Kennedy, at least initially, places synchronic and historical explanations at two opposite extremes. He admits, however, that this distinction is too stark and simplistic and concludes by qualifying it considerably and recognizing value in both. Yet his initial dichotomy throws the argument somewhat off key. A possible result of it is his tendency to confuse the notion of structure with that of stability and the notion of historicity with that of change.

The author proceeds to argue quite rightly that the different theoretical alternatives regarding social structure are indeed subject to test by means of scientific inquiry, but that the success of scientific inquiry depends upon the requirements placed on it. Thus the approach to scientific testing will vary according to whether the result is to be a rigorous deductive theory of a narrower scope or a less rigorous yet perhaps more generally significant theory of a broader scope.

The author makes a controversial but thought-provoking claim that the appeal of even empirically based theories is ultimately of normative character. According to him, all grand theories ultimately argue that "if we desire to realize a given value or values, then it follows that we shall have to invent certain *kinds* of persisting relationship, which integrate the incentives, constraints and habits that sustain ideal patterns and regularities of behaviour" (p. 117).

Studdert-Kennedy argues effectively for convergence of theoretical approaches. He rejects the idea of progress in theory as a complete succession of one paradigm by another. This latter he says is an effective formula for confusion of theory and ideology. Theoretical convergence for him is conceptual innovation which results from theoretical encounter in a specific field of evidence of two not readily integratable frameworks. It is imaginative exploitation of these differences of perspective which leads to development of more inclusive and more penetrating interpretations of significant, empirically based questions.

One could criticize the book for the author's method of making his points by using specific examples and moving constantly from one example to another. But the discussions generated in this manner are quite thought provoking, creative, and well written.

Sociological Formalism and Structural-Function Analysis: The Nature of the "Social"—Reality Sui Generis? Form? System? By Daniel de Sousa. New York: Exposition Press, 1975. $7.50.

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This book is very difficult to review usefully. A few quotations may illustrate some of the reasons: "Structural-functional analysis is a type of analysis which is essentially associated with the concepts of *structure* and *function*" (p. 65); "as for *functions* and *values*, we may say that Parsons does not concentrate his theory specifically on them" (p. 98); "our minds only conceive as concrete what is spacial and as abstract what is, for example, temporal—sounds, action, interaction, etc." (p. 104); "the subject matter of sociology consists in abstract entities" (p. 134). And here is the author's statement of the "result" of his labors (p. 134): "Ultimately, our own view is that analysis should not be simply formal, structural, or functional, but rather systemic in the sense that . . . not only the social whole but also its parts must be understood in reference to the whole as such. On the other hand, one should bear in mind that any part of a social whole is only significant in relation to that whole as such and has a different meaning or no meaning at all outside the system" (p. 137).

It is perhaps helpful to know that this book is an M.A. thesis in sociology, submitted to the University of South Africa, written in English by one whose native language (and, apparently, whose principal present language) is not English and whose principal vocation is not sociology. But responsibility for a published book does not stop with its author; publishers and their editors rightly share in the goodies when a book is well done. This book, however, is a credit to no one connected with it. The literature of sociological theory was better off without the pompous and haywire inanities with which this little book is stuffed—regardless of how the responsibility for them is shared.

Nevertheless, oriented by the current mania in sociology for theoretical synthesis (a sickness of which I am at least as willing a victim as Daniel de Sousa); spurred by the more general faith that a pony may be hidden in every pile; and, finally, sustained by the most general of all faiths, that the book review editor must have had something in mind when he asked me to review this book, I have tried to make out the underlying structure of de Sousa's argument. I leave the reader to judge the truth of specific claims in that argument. In this case, it is hard enough just to say what they are without discussing how true they are.

De Sousa begins by asserting that his aim is "to show the combination of sociological formalism with structural-functional analysis" (p. 3). Although he later backs away from this aim ("It is obvious that we cannot now attempt a combination between formalism and structural-functional analysis" [p. 126]) for reasons that I do not understand, he nevertheless pursues it across the interim pages. In this pursuit, the main focus seems to be on the entity to be explained and secondarily on the entity that explains, which each type

of analysis is thought to emphasize (p. 21). (I must emphasize "seems to be," because nowhere does de Sousa explicitly distinguish between these two quite different theoretic roles of entities; he is content, instead, to speak vaguely of a theory's "entities of reference.")

Consider first the entity to be explained. De Sousa seems to identify formalism with collective entities (see p. 93) and seems to claim that, whereas functional analysis shares this collectivity reference, structural analysis refers to the individual: "structural analysis . . . describes how the individual, or individuals, uses his means for satisfying his goals alone . . . while the functional analysis conceptualizes the individual activities attributing them to the group or society" (p. 114).

Next, consider de Sousa's treatment of entities that explain (and it seems to me that despite his early listing of explanatory behavioral factors such as "thinking, feeling, and belief" [p. 21], he remains focused on entities—not only as explananda, but as explanantes as well [see p. 137]). I am not at all clear on which entities that explain are associated in de Sousa's mind with formalism and which ones are associated with structural-functional analysis. But at this point, if I have got the right overall interpretation of this book, I think I see a familiar though still unexorcised and untamed pony issuing from its mists.

My guess is that de Sousa is trying to engage the following question: Insofar as one takes the essential sociological explanandum and the essential sociological explanans to include behaving entities of different inclusivenesses what is the main direction of causal influence—from the collectivity at the top to the individual at the bottom, or vice versa? In my judgment, although de Sousa's survey of some of the theoretical literature on this question contributes to neither the question nor the answer, he is right that a proper answer (better yet, a proper answer to a proper formulation of the question) can yield an important theoretical synthesis, and that systems theory (especially, I would add, the theory of hierarchical structure) can be useful to this end.

But for such slim old pickings—much more readily, more rigorously, and more comprehensively available elsewhere—I can hardly recommend that anyone except the enemy undertake to digest this book.

Death of a Utopia. By Gianni Statera. New York: Oxford University Press. Pp. ix+294. $10.95.

Roberta Garner

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In May of 1968 Europe felt the birth pangs of revolution but brought forth no new society. Why not?

Gianni Statera, a professor of sociology and methodology of social research at the University of Rome, attempts to answer this question in his analysis of European student movements of the late 1960s and early 1970s. The

result is a complex, interesting, tendentious, and sometimes internally inconsistent book. It must be understood above all as an argument against Marxist-Leninist interpretations of the student movements, both the interpretations produced by the movements themselves and those of the European Communist parties.

Statera builds his analysis on three elements. One of them is a theoretical framework based on the work of Max Weber, Karl Mannheim, and Ralf Dahrendorf. This framework borrows from Marxism but is opposed to Marxism. It recognizes the importance of the material basis of human action but treats this basis as one element in a social formation and not as the determining element of that social formation. This framework furnishes Statera with a set of largely structural variables (class structure, political system, a nation's position in the world political economy, the interplay of educational institutions and other characteristics of the society, and so on). But the framework also contains a different set of concepts, namely, Mannheim's distinctions between utopia and ideology. These concepts refer largely to the realm of ideas, not structure. Statera does not clarify sufficiently the relationship between his structural variables and the utopia/ideology dichotomy. Thus his theoretical framework itself contains tensions and inconsistencies. These become more apparent when he uses the structural variables to explain the emergence of movements but shifts to the utopia/ideology dichotomy to document their exhaustion and decline. Statera's theoretical framework also already contains an argument against Marx, the running argument that has dominated European sociology since the turn of the century.

Statera's second building block is his empirical material—the actual cases of student movements, primarily in the Federal Republic of Germany, France, Italy, and Poland. His data are largely derived from the documents and manifestos of the movements themselves, sources that contribute to his emphasis on ideas and self-interpretations. He also draws on several secondary analyses of the movements and the general political situation, on media accounts, and on his own observations. The choice of secondary analyses seems fairly arbitrary; for example, why is there no mention of Andre Gorz and Serge Mallet?

Statera's third building block is his own political perspective, namely, sympathy to movements that are utopian and spontaneist and hostility to movements and parties that are disciplined and Marxist-Leninist. In particular, he is hostile not only to Marxist-Leninist groupuscules but also to the large Communist parties of France and Italy. This political perspective thus parallels and reinforces the theoretical perspective. It places Statera squarely in the European tradition of debate with Karl Marx's ghost (not to mention Marx's living heirs) begun by Max Weber and Émile Durkheim and continued by Karl Mannheim, Raymond Aron, and Ralf Dahrendorf.

The interesting feature of the book is the interplay between these three elements—the Weberian and Mannheimian theory, the interpretation of the empirical cases, and the political bias. The issues are complicated by the fact that the political actors—movements and parties—have their own

competing interpretations of the situation, against which Statera is arguing.

Statera soon finds himself caught in a dilemma. The development and decline of the student movements and the cross-national differences in their trajectories support a basically Marxist interpretation; movements that are spontaneist, utopian, rooted in the problems of educational institutions, and based on a mass of young people who are petit bourgeois in destination, if not social origin, cannot accomplish a revolution. Statera uses his structural variables to make a clear and convincing case on this point. But this analysis leads him straight to the dilemma itself. The analysis suggests that these movements of intellectuals must develop a disciplined party organization, stable leadership, and lasting ties with the industrial working class if they are to go beyond "superstructural revolt," university reforms, and eventual decline. They can try to accomplish this transformation either on their own (the solution of the groupuscules) or by subordinating themselves to the existing Communist parties. Since Statera finds neither option politically acceptable, he largely abandons his structural categories and turns to the utopia/ideology distinction for explanations of the movements' decline. He emphasizes their utopian aspects, their search for subjective liberation, and their antiauthoritarianism. He then defines any turns away from these positions as lapses into ideology in the Mannheimian sense. In other words, his own politics leads him to shift his concepts in the middle of the analysis, from structural concepts to more ideational ones. He sees the structural situation as having posed a dilemma for the movements—to accept utopianism and failure or to become ideological (i.e., Marxist-Leninist). But this dilemma is perhaps Statera's dilemma more than it is a dilemma for the movements themselves.

Another consequence of Statera's political perspective and his use of the utopia/ideology distinction is a refusal to see the student movements and the May 1968 events as part of a larger leftward shift in western Europe that has been underway since World War II. The concept of utopia leads to an overemphasis on discontinuity, total negation, and eventual failure. Thus, for instance, Statera points out how the policies of the PCF (and to some extent the PCI) helped to halt the student movements. But he therefore fails to make the longer-term analysis: Precisely in those countries where mass left-wing parties existed (France and Italy), the soil had been prepared for revolutionary action by workers and students. In West Germany, where no mass Communist party exists, the student movements remained almost totally isolated. Thus although in the short run PCF and PCI policies dampened the effects of student movements, in historical perspective these parties had provided a context in which movements could be a challenge to the class structure.

Similarly, the concept of utopia obscures other aspects of the leftward shift in southern and western Europe. It points to the isolation and death of movements, rather than to their integration with other institutions. For example, Statera has little to say about how Eurocommunism is a counterpart to the student movements of the 1960s, an alternative but related response to the same material basis; the problems of advanced capitalism

in Western Europe, the growth of new strata, especially a "new working class" or a "new salaried petite bourgeoisie" with long years of formal education; the position of Europe, caught between the Soviet bloc and America; and so on. Eurocommunism has tried to define itself as an heir and an alternative to the student movements and may in fact be gaining electoral support from the same social groups, now settled down to unemployment or routine lower-level superstructural jobs as teachers, journalists, civil servants, and so on. The concept of utopia is a barrier to observing such long-term behavior patterns.

The issues raised by Statera's theoretical and political perspective and its clash with Marxist-Leninist perspectives are the most interesting problems of the book. There are some minor difficulties, like the apparently arbitrary choice of Poland as the only socialist country. There are also some editorial mistakes.

Death of a Utopia would be suitable for courses in movements and collective behavior, youth and youth culture, the sociology of education, and western European politics. But the American reader should be aware that it is a political analysis with a distinct political perspective.

An African Dilemma: University Students, Development and Politics in Ghana, Tanzania and Uganda. By Joel D. Barkan. Nairobi: Oxford University Press, 1976. Pp. xvii+259. $15.75 (cloth); $9.00 (paper).

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The role of the intelligentsia in economic and political change in underdeveloped countries is both complex and substantially underresearched, yet of increasing salience: The postwar spread of higher education in the developing areas has spurred an accelerated growth by this stratum, while commercial and industrial expansion (combined in the new postcolonial polities with the pressures exerted by nationalism) have provided it with a greatly diversified occupational base.

Joel Barkan's wide-ranging survey of career aspirations and political orientations among some 1,900 students at three leading African universities is a useful addition to this field of interest. While the university in "underdeveloped" settings provides a key reservoir of trained manpower for development—indeed, university graduates represent prime agents of "modernization"—Barkan argues that the university's developmental contributions are problematic in important respects: The fulfillment of student aspirations regarding careers and living standards may tax limited public resources, their graduation into functional societal roles may also stimulate class formation (and potential class cleavage), and incipient value conflict with incumbent political leaders is fostered by elitist tendencies in their educational milieus. Already at the time of Barkan's fieldwork (between November 1966 and December 1967), the student communities at the

University College, Dar es Salaam (Tanzania), and the University of Ghana at Legon had experienced significant embroilments with their governments; the one at Makerere University College had not, though it hardly remained unaffected by Uganda's recent constitutional crisis.

Against such a background of concern, Barkan's analysis draws into focus student attitudes and experience on several related issues, including those of social origin and career destination, student perspectives on problems of change confronting their societies, patterns of political activism, dispositions regarding a democratic political order, and perceptions of various national- and local-level elites. A final chapter summarizes principal findings and explores their broader implications for economic and political development.

Space forbids more than a brief review of Barkan's materials. Notably, this student generation fails to reveal itself politically as very concerned, let alone as a presumptive ruling elite; available evidence suggests that in this it differs significantly from its predecessor at the time of independence. Inclined to define success in terms of security rather than of wealth, status, or power, the students emerge as organization men—aspiring members of a technocratic upper-middle class. Strongly *étatist* in career orientation, their preferred occupations are teaching, the civil service, medicine, and other professions, while politics, the military, and agriculture (and in Tanzania and Uganda, also business) vie for last place. Though the educational system still functions to channel upward mobility, the percentage of students from socially more advantaged backgrounds indicates it may do so far less than before (an argument, in passing, not all available research would support).

Considering their social origins, educational experience, and career dispositions, Barkan observes that the students show little enthusiasm for either entrepreneurship or innovation, and appear unlikely to provide impetus for radical change within their societies. Favoring a mixed economy against capitalist or socialist alternatives, they stress rural over industrial development, though few intend pursuing careers in rural areas. An "inevitable" elite-mass gap is accepted; of seven developmental objectives, the elimination of social inequality among Africans receives little emphasis. Their "modernism" is qualified by attachment to cultural aspects of traditional society and by ethnic friendship ties. Thus they emerge as a pragmatic, professionally oriented intelligentsia, with an underlying apolitical and even antipolitical character manifested in their limited political involvement, in the lack of esteem accorded the politician's role in their societies, and in a "civic" ethos more appropriate to an administrative than to a democratic state.

A context of underdevelopment and existing limits to career opportunities promote symbiosis between students and state, but they also foster bases for conflict. Student awareness of the latter's significance encourages political quietism, unless government policies infringe on career and status aspirations. The 1966 confrontation in Tanzania, Barkan argues, reflects a pattern common in Africa throughout the early 1970s. In broader perspective, while student dispositions toward public service (whatever the regime's nature)

suggest that their longer-term contributions toward institutionalizing a civil politics may be less than Western experience would promise, they will undoubtedly assist state formation by supplying essential administrative cadres. Further, though the emergent technocratic elite will advance processes of class formation, this impact would be greater were the private sector's importance much enlarged. On the other hand, the author feels its contributions to economic growth, mediated through bureaucratic settings, are likely to be of limited developmental benefit.

In a study of ambitious empirical scope and analytical span, questions inevitably arise regarding specific points as well as general themes in Barkan's argument. Among minor matters are several apparent inaccuracies which may be typographical errors (otherwise few). Tabular data are generally clear, though occasionally entries need further explanation. The conceptual infrastructure merits elaboration and, in places, firmer control.

Among more substantial problems, the questionnaire's breadth of coverage means particular topics are not always probed sufficiently—a difficulty the author acknowledges. With this are linked several weaknesses in his analysis. While the tendencies toward political conservatism are clearly marked in the survey materials, for example, Barkan's contention regarding a general conservatism—a lack of innovativeness and entrepreneurial drive—has less firm support. True, student bias in favor of low-risk occupations with acknowledged status is evident; associating this with a limited propensity toward entrepreneurialism is a suggestive hypothesis. Yet whether such tendencies reflect basic dispositions or merely pragmatic expectations regarding available career options remains uncertain. Moreover, the attempt to provide a foundation for such proclivities in the continuing influence of colonial educational traditions is problematic, not the least for appearing to tie innovative capability to so narrow a conditioning base.

Barkan's argument that students aspire to enter a technocratic upper-middle class is also only partially sustained by his data—a matter of some consequence, for the author uses this point in turn to explain student orientations on other questions. Student perceptions of class and their own status aspirations both need further investigation, while in this context the ambiguities of the concept itself merit attention.

The assertion of the primacy of formal education as a socialization experience for university students raises more complex questions. Without a control group of nonuniversity peers the proposition is, as Barkan notes, technically undemonstrable. It nonetheless receives support both from the presumed impact of education (given the length and intensity of students' exposure to it and, in these contexts, their heavy dependence upon it in preparation for adult roles) and from the broad consistency of response patterns across the three samples and the absence of more frequent correlations between student attitudes and other variables. Yet, in view of such support, it is surprising that the data are not made to reveal significant variations with duration of university experience controlled. In several instances, moreover, the findings seem susceptible to alternative explanations by reference to external factors. When framed in broadest terms, the

argument blurs any distinction between manifest and latent socialization processes, posits a doubtful insulation of the university from its societal milieu, and disregards the implications of postuniversity socialization (as in organizational settings where most students will pursue career aims).

The university's developmental contributions are in part queried from the vantage point of a model assigning the private entrepreneur, rather than the state bureaucracy, a key role in innovation, risk management, and capital formation. Yet historical and contemporary experience, in Africa and elsewhere, indicates a more variegated relationship here. In addition, the fact that the civil service in many new states has proved not insignificant as a recruitment source for entrepreneurial manpower and the incidence of private-tending behavior by public servants—corruption in numerous forms, even the ownership of businesses and farms (a type of parasitic entrepreneurialism)—indicate that an entrepreneurial ethos is not necessarily alien to role playing in public institutions, even if difficult to harness to public ends.

The above notwithstanding, *An African Dilemma* is a productive study with considerable relevance to a field where research remains in an exploratory phase. It is distinguished by unusual comparative scope and the valuable array of data it provides on student attitudes and aspirations. While Barkan's arguments sometimes press beyond the limits of his materials, the discussion poses important issues and offers much perceptive comment.

University Students and African Politics. Edited by William John Hanna. New York: Africana Publishing Co., a division of Holmes & Meier, 1975. Pp. xv+296. $22.50.

Maure Goldschmidt
Reed College

This volume analyzes the political attitudes of university students in selected countries of East and West Africa. The evidence collected by Professor and Mrs. Hanna and their associates suggests that African students constitute a slight threat to the political status quo. These studies thus confirm the view that the political importance of students has been much exaggerated as a result of the Berkeley rebellion and the May uprising in Paris. To be sure, African students have been interested in politics and have from time to time demonstrated on behalf of one cause or another, but their activities have not been decisive or even significant in determining the location or use of power.

Prewitt, McKown, and Barkan, discussing the role of students in Uganda, Kenya, and Tanzania, explain why this is so. Experiences in primary and secondary schools dampen tendencies toward political radicalism. These schools, organized on British models and staffed largely by expatriates, inculcate respect for authority and careerist ambitions. Those who survive the highly competitive examination system are likely to be convinced of its soundness, especially when they become aware of the rewards of success,

namely, lucrative places in the civil service or the modernized private sector.

Although African students have not posed a serious threat to the stability of the political status quo, the power holders have nevertheless viewed them with suspicion and have been quick to suppress any oppositionist tendencies. Since there is no tradition which legitimizes opposition, any articulate group, of which there are few outside the government, is suspected of subversive tendencies.

As Barkan points out, student protest is often concerned not with public questions but with their own economic interests such as allowances, food, and so forth or, as in the case of the Tanzanian student protest, disappointment that they would have to accept national service and thus postpone their enjoyment of high incomes as members of the civil service.

In examining the attitudes of Ivory Coast students, Zolberg suggests that intergenerational political tensions are likely to be high and to take the form of conflict between political organizations that are age homogeneous. He illustrates this with an account of the conflicts between students and government leaders, all of which were settled in favor of the latter. While such tensions no doubt exist and predispose to conflict, they are a phenomenon common to all countries undergoing rapid change, and it is doubtful that they provide much of an explanation.

Hanna concludes from the case studies which deal with the Ivory Coast, Ghana, Rhodesia, Kenya, Uganda, and Tanzania that "most African university students are interested spectators of the political scene, and a significant minority are highly active. Constraints from national, educational, political, economic and sociocultural systems appear to keep the rate of activism low and also to explain Pan-African variations. The incidence of activism appears partly to be explained by students' identity, professionalization, membership and political perspectives" (pp. 283-84).

The studies make considerable use of survey research methods and occasionally take their findings too seriously. Thus, McKown states that, on the issue of tribalism versus nationalism, "students opted for the (perhaps obvious) nationalistic answer." However, it may be doubted that their answers reveal their beliefs, as retribalization has made great headway among Kenyan students.

Some of the authors tend to labor the obvious. Hanna states that "an examination of occupational expectations and aspirations leads to the conclusion that African students will probably be members of the occupational elite: doctors, lawyers, teachers and government officials" (p. 37). Not surprising! Professor McKown predicts that "an elitist system increases the probability of an individual's recruitment into a high income-high status role" (p. 217). Of course.

On the whole, this profile of African students' attitudes suggests that they are not so different from students in other parts of the world, especially the United States. They are cynical, out for the main chance, politically interested but not very involved, and constitute little threat to the status quo. A comparative framework, as Professor Hanna suggests, might have produced more interesting results.

Change and Continuity in American Politics: The Social Bases of Political Parties. By David Knoke. Baltimore: Johns Hopkins University Press, 1976. Pp. xv+192. $11.50.

Heinz Eulau

Stanford University

If one assumes that so-called secondary analysis is, in fact, a meaningful enterprise, every secondary analyst and consumer of secondary analysis must ask whether a given work makes a contribution to knowledge beyond the contribution that a prior "primary analysis" may or may not have made. This requirement is more easily asserted than met, particularly in a field like American electoral behavior, where a large and rich body of survey data has precipitated, in the last 12 years or so, an avalanche of studies based on the data collected since 1952 by the University of Michigan's Survey Research Center (SRC) and more recently by the Center for Political Studies (CPS). A conservative estimate would be that well over 1,000 books, dissertations, articles, and papers have utilized the Michigan surveys. Moreover, because in recent years the CPS has generously made available the election data even before its own staff had a chance to exploit fully a new survey, it has become increasingly impossible to sort out what might be considered a primary from a secondary analysis. For instance, David Knoke, in this methodologically elegant book based on the Michigan presidential election data, saw himself performing a secondary analysis; but a book published almost simultaneously by CPS staffers Warren E. Miller and Teresa E. Levitin (*Leadership and Change: The New Politics and the American Electorate* [Cambridge, Mass.: Winthrop, 1976]), using the same time-series data, presumably represents a primary analysis.

In this context, then, it is difficult to assess the place of any particular study in the configuration of all studies that have come to constitute a cumulative, if at times controversial, corpus of knowledge. There are so many theoretical and methodological issues agitating the community of voting analysts that it is quite risky to be definitive, for it is not always clear whether differences in discoveries are due to theoretical reorientation; to methodological advancement; or, equally likely, to the fact that voting in elections is one of the rare patterns of political behavior that occurs periodically, that is periodically observed with great care, that is subject to periodic changes, and that is therefore a source of continuing incentive to falsify or revise earlier estimations and propositions.

Knoke chooses the "party identification" measured in the SRC/CPS studies since 1952 as a crutch to study electoral politics, at least as it is mirrored in the behavior of the mass electorate. His study of its "fate" as either a dependent or an independent variable between 1952 and 1972 gives him six time points for comparison of the cross-sectional presidential studies. Although his purpose, characterized as "modest," is "to describe in great detail and precision the social group composition of the mass electoral

parties and how they have changed or remained stable in the recent past" (p. xiv), the book hardly comes close to achieving this objective and cannot possibly do so, given the fact that it is based on survey data alone and ignores aggregate electoral statistics, relevant data about elites, and institutional variables. Rather, it is a well-executed and methodologically sophisticated "working over" of some of the Michigan data in terms of interests one would expect from a political sociologist.

After a somewhat truncated "theoretical" treatment of party identification as a concept and of competing models of voting behavior, Knoke examines in successive chapters the impact on party identification of religion, region, race, residence, and socioeconomic status. These analyses are rather straightforward and the findings are hardly startling. This itemistic approach is followed by an effort to explain party identification at the six time points in terms of a causal model that is conventional. A subsequent chapter in which the analysis is based on the SRC's 1956, 1958, and 1960 panel data is of more interest, for it gives more insight into stability and change in party identification and its relationship to voting at the individual level than does comparison of cross sections.

By far the most important contribution of the book is chapter 7, "Party and Voting Behavior," which centers in a cohort analysis of the time-series data. Indeed, it is probably through cohort analysis that the real-world macro problem of party realignment, so fascinating to political analysts, has the best chance of being successfully tackled as a scientific problem. Alas, it is in this connection especially that current development in voting research has already outpaced Knoke's analysis, though it has not made his analysis obsolete by any means. His analysis demonstrates and exposes the fallacy of the "life-cycle hypothesis," which predicts both greater conservatism and more intense party attachment as a function of aging, and it fails to falsify and gives more credence to the "generational hypothesis." But Knoke's work must here be placed into the framework of CPS staffer Philip E. Converse's critical explication of the cohort-analysis approach to party identification in *The Dynamics of Party Support* (Beverly Hills, Calif.: Sage, 1976), which stresses "period effect" as a variable cutting across both individual aging and generational transformation as latent harbingers of changes in party identification. Knoke must be pleased that Converse is more appreciative of his work than he is of the work of others who have tried their hands at the Michigan data in a cohort-analytical format.

This study, then, is more limited than its title suggests. It is an exceptionally careful reanalysis of some of the Michigan data in a 20-year time frame. It is likely to be referred to, at least for awhile, in the ongoing work on voting behavior, but it is also likely to be challenged, for both the conceptualization and the measurement of party identification will probably undergo radical revision in the next few years. In the context of such revision, Knoke's work will be at least one useful point of departure.

The Urban Threshold: Growth and Change in a Nineteenth-Century Community. By Stuart M. Blumin. Chicago: University of Chicago Press, 1976. Pp. xiv+298. $16.50.

Claude S. Fischer
University of California, Berkeley

Sociologists aspire to theories of process and change but usually end up relying on static evidence for those theories, citing a correlation, taken at one time, between one variable and another as proof that there is a causal association between the two. Historians have until recently been of limited assistance to victims of this dilemma, because they have been concerned primarily with unique events in the lives of extraordinary people rather than with the daily experiences of "ordinary" ones—the experiences sociologists seek to comprehend. In recent years, the trend has been for young historians increasingly to address important social science questions. Informed by sociological theories and armed with statistical methods used by sociologists, these historians are gathering rich data on the lives of ordinary people of earlier eras. Sociologists, particularly those interested in community, can experience some of the excitement of the now-mature "new urban history" by reading Stuart Blumin's *The Urban Threshold.*

Blumin's history focuses on the town and residents of Kingston, New York, between 1820 and 1860. At the beginning of this period, Kingston was a stagnant town of fewer than 3,000 people, two-thirds of whom were farm dwellers. A canal was constructed nearby in 1826, and by 1860 Kingston had become a small city of over 16,000 people, relatively few of whom were farm dwellers or workers. Blumin's goal is to understand how crossing this "urban threshold" affected community life and personal lives in the community. He undertakes this task with great skill, both theoretical and methodological. He is sufficiently comfortable with sociological theories of community not only to apply them (citations to such people as Louis Wirth and Roland Warren abound), but also to critique them with insight. He demonstrates command over a wide variety of data, ranging from personal diaries to census tabulations (some of which he subjects to log-linear analysis).

Most of the findings Blumin reports confirm the propositions sociologists have drawn—based on static correlations—about the consequences of urbanism. Blumin's contribution is that he confirms them with evidence over time and with detailed, rich, and concrete cases. Many changes occurred in Kingston as it crossed the "urban threshold." The economy was reorganized (e.g., commercial and occupational specialization increased, home production of consumer goods declined, and bartering was totally supplanted by a cash economy). New social conditions arose (e.g., an ethnically diverse proletariat developed, with accompanying conflict and violence; crime increased; social order became a matter of community concern; and the differentiation of community institutions gradually increased). The ecology of the town changed, in the form of sharpened differentiation of

space by economic function, residential segregation by income, multiple-unit dwellings, and separation of home and workplace. On top of all this, town politics changed, with increasing complexity and responsibilities of local government and a broadening of community leadership.

Blumin also offers a few new and intriguing arguments about the consequences of community growth. In one case, he challenges the conventional explanation for the proliferation of formal associations. He does not see them as substitutes for a natural intimacy lost together with the innocence of Kingston Village, for he finds little evidence that the personal social lives of the Kingstonians waned while the town's population grew. Instead, he suggests that these associations offered new opportunities for community life in addition to continued informal social activity. Another related example concerns Kingston's communal identity and spirit. Contrary to traditional views, Blumin argues that prethreshold Kingston lacked a substantial community identity and was instead composed of individualistic households, oriented more to New York State or to private concerns than to the collectivity of Kingston. Growth brought institutions and issues that "provided Kingston with a group life and perhaps even a collective awareness and identity that seems to have been but weakly developed in the country town of the previous generation. . . . [It was] a town that moved toward community as it grew toward urbanism" (p. 165).

Somewhat outside the mainstream of his argument, Blumin provides, largely through analysis of census material, descriptions of Kingston's social structure in the 1850s. Because comparable data are lacking for earlier decades, this analysis is only weakly integrated into the rest. It is, nonetheless, informative. For example, as with other such histories, this study describes a mass of "floaters" in American communities of the period. Most people moved often from town to town, forming a transient (and now forgotten) proletariat, who only in rare instances rose to positions of economic security.

As is inevitable with material of this kind, there are strong limitations and qualifications to Blumin's evidence. Reliable census data are unavailable for the earlier periods, and he must depend on the "softer" data comprised of newspapers, diaries, maps, mortgage records, and the like. Blumin's deftness with such records cannot compensate for the tentativeness of subtle interpretations based upon fragmentary evidence. For instance, Blumin makes much of the fact that early newspaper accounts of Independence Day ceremonies failed to report toasts by the celebrants to the community of Kingston, while such pledges were printed in later years. A more general caveat concerns the validity of conclusions drawn about urban growth from this single case. In particular, we must wonder whether the changes that occurred in Kingston were not common to most American communities, urban or rural, growing or not, in this era of great transformations. The explanation may lie not in the nature of the "urban threshold" but in the character of the times.

Concerns such as these are endemic to historical community studies,

particularly those that examine lives led over 150 years ago. Blumin recognizes these difficulties and handles them well. While such studies can never achieve the inferential confidence possible with contemporary data, they provide a critical and sorely lacking dimension to sociological understanding. And, I would add, they also provide a set of interesting and well-written books. Not only would sociologists learn much by dipping into this literature, I think they would also enjoy it. And Blumin's *Urban Threshold* is an excellent example of the genre.

Black Suburbanization: Access to Improved Quality of Life or Maintenance of the Status Quo? By Harold M. Rose. Cambridge, Mass.: Ballinger Publishing Co., 1976. Pp. xiv+288. $16.00.

Thomas M. Guterbock

University of Virginia

This is a book about the quality of life in old and new suburban areas of black residence in the United States: Are they communities of opportunity or communities of despair? It is a disappointing book for two reasons. First, Harold Rose's conclusions leave little room for optimism about the future of blacks in suburbia. Second, because the work is based on inappropriate data, inadequate analysis, and poorly specified theory, the informed reader gradually relinquishes all hope of gaining important insight from this book.

Most of the data pertain to a group of 15 "all-black towns," some of which were the subject of previously published work by Rose, who is professor of geography and urban affairs at the University of Wisconsin—Milwaukee. Secondary data are combined with data from a 1972 sample survey of residents to provide a basis for assessing the quality of housing, education, class-based culture, occupational opportunity, and local services in these communities. Rose also uses secondary data to assess some of these same conditions for a group of 35 "black suburbanizing communities," suburbs which gained "at least 1,000 or more [*sic*] black residents" during the 1960s (p. 58). The samples are very diverse. They include rural settlements and modern tract developments, middle-class communities and poor communities, areas which are all black, communities in racial transition, and large white suburbs with small black percentages. The study concludes that the quality of life in these places varies greatly but is in most cases worse than it is for whites and no better than it is for central city blacks.

The chief problem with the data is the lack of a comparative base against which quality of life in black suburbs might be assessed. There is no systematic treatment of white suburbs or of black central city neighborhoods which could have provided instructive contrasts. Therefore the data analysis usually yields little more than a ranking of some black suburbs on an

indicator variable or a comparison of the scores with a national average for the variable.

There are other deficiencies in the data, some of which are to be expected whenever aggregate variables drawn from secondary sources are used as indicators of such elusive concepts as "quality of education" or "adequacy of housing." The quality of the data is difficult to assess because Rose is vague and reticent in his description of data sources. It is not always clear whether or not the aggregate data for racially mixed communities in the sample pertain only to the black population. The survey questionnaire is not reproduced, few of the items are quoted, and no clue is offered about the fixed-response alternatives which were used. Rose does acknowledge some of the limits of his data, but he takes his analysis well beyond those limits. This is most apparent in the discussion of educational quality, where he considers the empirical relationships among variables such as faculty integration, racial atmosphere, and student achievement despite the fact that he has virtually no data from the schools in the sample communities.

When Rose moves beyond data description to causal analysis, there are serious problems of method. He consistently glosses over simple two-way relationships which would help to clarify patterns in the data. He alternates between tedious description of the range and distribution of a particular variable and multivariate statistical analyses that are poorly conceived and incompletely described. For example, he abruptly introduces a principal components factor analysis of data for 15 cases and 47 variables (p. 33). He describes in some detail a factor which accounts for only 10% of the variance and which remains incomprehensible because the factor loadings in the accompanying table disagree with each other and with the discussion in the text. The other factors are not described until later chapters, the remaining variables are not listed, and the theoretical grounds for the analysis—and its practical significance—are never discussed. Even more serious are the problems in a multiple regression analysis of data from Cincinnati schools (p. 106). Using 14 predictors simultaneously, Rose accounts for 97% of the variance in verbal achievement scores. But there are only 16 cases, which means that any set of 14 variables would generate results of equal utility. Without supplying any of the actual regression coefficients, Rose discusses the effects of the individual predictors, ignoring the fact that he is positing effects which operate while 13 closely related variables are simultaneously controlled. He concludes this analysis with an attempt to make sense out of meaningless quantities, that is, the deviations of the verbal achievement scores of particular schools from the scores which a 12-predictor regression equation would predict. Such misapplication of technique does not enlighten; it only confuses.

The statistical analyses also fail because they are not set in a coherent conceptual and theoretical context. Rose's terms are drawn from many disciplines, but he does not articulate their theoretical underpinnings. For instance, we read of the black mover's "awareness space" (p. 79) and of

"subliminal friction" between owners and renters (p. 35), but find that the social psychology of the black suburbanite is never explored. There is occasional mention of a suburb's distance from the central city, but spatial gradients of income, density, and age of housing stock are not systematically used. In his discussion of crime rates, Rose emphasizes the subcultural "modes" or "styles" of the communities (p. 190); this cultural determinism is not balanced with a consideration of the structural causes of crime. Eclectic theory can be useful in the kind of evaluative task which Rose has undertaken. But despite the eclecticism of his approach, Rose misses the actual social organization of black suburban life as it is manifested in neighboring and friendship networks; civic, political, and religious organizations; and mechanisms of social control.

An adequate treatment of the problem which this book addresses requires some coherent theory about how suburban residence might improve the quality of life for blacks. The beneficial (and detrimental) effects of different suburban conditions must be specified in advance and kept separate in the subsequent analysis. For example, one might distinguish analytically between the advantages that the black suburbanite gains from (1) lower density and heterogeneity of suburban housing stock; (2) proximity to suburban jobs and amenities; (3) separation from ghetto institutions; (4) political autonomy; and (5) the attitudes, behavior, and resources of other black suburbanites. However it is conceived, black suburbanization must be considered apart from other factors which affect black community life, particularly the spatial differentiation of black ghettos according to class and racial segregation in the housing market and other institutions. In the absence of coherent theory, the extensive data offered by Rose tell us what life is like for suburban blacks without resolving the issue of whether suburban residence itself is beneficial to them.

The book concludes with an unusual policy proposal: the institution of a formal mechanism of "allocation" of people to communities on the basis of their conformity to specified norms of conduct. Rose is vague about what problems such legal segregation according to "life-style" would solve, but he would seem to be motivated by concern for the black middle class, which has always experienced great difficulty in achieving lasting geographic separation from lower-class blacks. The dilemma is real, but an increase in class-based discrimination would only exacerbate the more fundamental problem of inequality between the races in our metropolitan areas. Before advocating such a policy, we should inquire whether black suburbs might simply employ the same exclusionary practices which middle-class white suburbs use to protect their perceived interests, and assess the costs and benefits for all if they were to do so.

In short, this book fails as an analysis of black suburbanization. The descriptive data successfully underscore the fact that few black suburbs are "nice" middle-class communities, but the discussion sheds little light on the causes of this situation or on possible remedies.

Neighborhood Organization and Interest Group Processes. By David J. O'Brien. Princeton, N.J.: Princeton University Press, 1976. Pp. xii+263. $13.50.

Harvey Molotch

University of California, Santa Barbara

David O'Brien has a problem: he wants the poor to have more, but he recognizes that the well-to-do, particularly the business elites of local communities, will not let them have more. He seems to have a two-pronged solution: make the poor more effective in providing for their welfare through the mechanism of community organization and make the well-to-do more likely to facilitate such community organization by giving them something in the process as well.

O'Brien's problem is further complicated by his recognition that community organization of the poor is, by evidence of the record, a very difficult thing to accomplish, and this is true, in no small part, because the rich have seen relatively little to gain in allowing such groups to prosper. O'Brien's conceptual solution to these sorts of problems begins with the argument that there must be individual incentives present if collective goals are to be achieved. This means that potential participants in community organizations must see a direct connection between their participation and some external private gain; it means that the rich must similarly have a direct incentive to tolerate or provide for such organizations if they are not to use their considerable power to effectively destroy them. O'Brien hence relies on two important and kindred schemas in making his arguments: the "public-goods" approach of Mancur Olsen and the "resource-mobilization" approach to the study of social movements as encouraged in the work of Mayer Zald.

The public-goods problem sees any sort of collective action, based on purely moral or collectivist community sentiment, as sociologically absurd. O'Brien is on strong ground when he argues that traditional community organization effort, in the social work tradition, has been ineffective. He argues that community organizers typically work from the assumption that participation is itself rewarding, both as individual therapy and as a sufficient basis for sustaining the organization. But, O'Brien argues, people participate to effect external goals, and without success in actually reaching such goals, organization cannot be sustained. Community organizers have also failed to incorporate into their conceptual apparatus a realization that organizations exist in a political context of other institutions with which they must compete for the scarce resources which are needed to sustain organizations. Only by working out some modus vivendi with at least some external organizations can such resources be gathered and, hence, the community organization effort survive.

All of this is sound. But O'Brien goes wrong in two ways. First, he pushes his "individual incentive" approach much too far. He has trouble conceiving of the possibility that meaningful external goals can be perceived as collective goals which can be accomplished through collective behavior. It may be

that while within the rationalist system of Hobbes, Malthus, and O'Brien it is irrational for some to sacrifice in order to accomplish a good they will share with others, it is nevertheless the case that people do it. They have stormed the Bastille, fought wars and revolutions, died in the American South and in Vietnam under conditions which conform to the public-goods model only by the most tortuous reasoning. The historical record on this is certainly as clear as the historical documentation of the failure of community organization work in U.S. cities. The lesson from the social workers is not that individual incentives are necessary, but rather that meaningful goals are necessary as well as (and O'Brien is with me on this) a reasonable chance of accomplishing them. In connection with CO's past failure in the United States, it is reasonable to argue that the goals of new stop signs or community control of garbage pickups is not relevant to people whose occupational status provides them with no opportunity for a decent life, regardless of the quality of local public services. Hence community organization is irrelevant; there are no important private or collective goals to be achieved.

O'Brien's second error derives from his naive assertion that the reason community organization has failed to provide appropriate resource mobilization for the poor is that the framers of such organizations failed to understand that CO work is political work. In applying this viewpoint to those behind Mobilization for Youth and the War on Poverty efforts to which it led, he is, I think, just dead wrong. O'Brien thinks that it came as a surprise to the poverty war intelligentsia that big city mayors opposed CO development. O'Brien thinks that conflict should have been anticipated and institutionalized through a sort of bargaining apparatus in which the new COs would have shared power with the existing power structure of cities. But I would argue that it was precisely the recognition of their threat to the status quo that caused the framers of the war on poverty to insulate the activists from the local power hierarchies. In the end, of course, they failed. But it seems clear that if the COs had not had such initial insulation they would have been finished off long before they were. The mayors and welfare folk would simply have used a different, more subtle technique—but the end result would have been the same.

Especially in his last chapter, O'Brien gets off some important insights—particularly his observation that the increasing bureaucratization of all public service functions destroys the potential of effective community organization by depriving would-be indigenous providers of service with a service to provide. When the government provides all schoolchildren with free breakfasts, the Panthers must look for another way to serve their community and to sustain themselves as a legitimate, useful community organization. In the other chapters, however, the book is more than slightly flawed by a misplaced concreteness in its overdocumentation of banal observations. The following sentence is followed by parentheses containing reference to 16 documenting publications: "One of the most important political developments during the 1960s was the evolution of New Left student radicalism" (p. 98). Sometimes the assertions are merely banal and no docu-

mentation is thought necessary: "Because of the flexibility of democratic systems, movements for incremental change are more easily organized and accepted as legitimate than those for radical change" (p. 55). There are also antique conceptualizations of "social disorganization" in the slums, empirically suspect views of "white-flight" to the suburbs and "block-busting" in the grey areas, and glosses to an un-thought-out concern with "human dignity." Taken as a whole, the book pays serious attention to important matters, is sometimes right but is often wrong, and—in the main—lacks the intellectual bite which could sustain it as an important resource for those wishing to understand or change a society about which they have few false illusions.

A Comparative Analysis of Complex Organizations. By Amitai Etzioni. Rev. ed. New York: Free Press, 1975. Pp. xxiv+505. $12.95 (cloth); $6.95 (paper).

James L. Price
University of Iowa

The 1961 edition of Etzioni's *A Comparative Analysis of Complex Organizations* contained what turned out to be an influential statement of a compliance theory. This new revised and enlarged edition contains the 1961 statement of the theory plus an update.

The update contains five new chapters which codify research published since 1961 that is pertinent to the theory. A postscript to the 1961 edition has been replaced in the 1975 edition by a final chapter dealing with "new directions" for compliance theory.

What has been accomplished is impressive. Four aspects of the accomplishment are noteworthy. First, there is a careful building upon prior research. This is evident in the care with which the new research published since 1961 is summarized and evaluated. (The new research, it should be noted, is summarized *and* evaluated.) Nor are the founding fathers neglected. Many aspects of Weber's work, for example, are accepted, but valuable extensions are offered, especially in connection with charisma. Second, the range of data codified is very broad. Literally every type of organization is included within Etzioni's theoretical net. He seems to have read everything of significance. Especially impressive is the inclusion of sizable amounts of data about military and religious organizations, systems often neglected by contemporary organization scholars. The book is clearly a *comparative* analysis of complex organizations. Third, the conceptual analysis is of a high order of sophistication. Concepts are clearly defined and used consistently. New concepts are introduced where appropriate. The discussions of elites and cohesion are especially enhanced by the new concepts introduced. The combination of comparative analysis and conceptual sophistication results in a wealth of provocative insights into the operation of organizations. Fourth, and most critical, the value of focusing upon the compliance structure of organizations is clearly demonstrated. The compliance structure

is related to goals, effectiveness, elite structure, consensus, communication, socialization, recruitment, scope, and pervasiveness, to mention but a few of the many correlates documented. Etzioni views his compliance theory, not as a theory in the most rigorous sense of the term, but as an initial statement of a set of relationships. His most frequent term for these relationships is "correlate." Etzioni has a genuine capacity to perceive these relationships; there are enough correlates in this book to keep researchers busy testing them for years! Few theoretical efforts in the study of organizations equal what Etzioni has accomplished with his compliance theory.

Much work remains to be done, however. Three tasks require attention. First, further testing would have been facilitated had the many correlates been precisely stated in one place. The concepts are better formalized than the correlates. Second, the role of effectiveness requires clarification. In addition to the impressive set of correlates about compliance, the book also contains an implicit causal model of organizational effectiveness. This causal model should be explicitly stated in one place and systematically linked to the correlates. Third, the matter of causality must be squarely faced. Etzioni prefers to avoid causal statements in his compliance theory, yet he repeatedly uses the compliance structure of organizations as an independent variable. To attain a genuine theory, the correlates should be reformulated as causal statements.

This book is indispensable reading for serious organizational scholars. Let us hope that we do not have to wait 14 years for the next update.

Organizational Survival in the Performing Arts: The Making of the Seattle Opera. By Mahmoud Salem. New York: Praeger Publishers, 1976. Pp. v+210. $16.50.

Vera L. Zolberg

Purdue University

Mahmoud Salem's book on the Seattle Opera is mainly a manual on "how to succeed in business" based on the brief (10 years) history of a cultural organization whose rise to prominence in a provincial outpost is considered a marvel by foundations, governmental agencies, business columnists, and numerous critics. Despite Salem's assertion that he undertook the research because he "grew to love [the artists] and their world, to identify with their aspirations and dreams and to empathize with their problems" (p. vii), the artists—performers and composers—are given relatively little attention. Sharing the tacit assumption of organizational managers and philanthropists that the conventional operatic repertoire constitutes a form of "high" culture self-evidently worth preserving, Salem eschews any discussion of more general aesthetic and sociological significance concerning opera itself.

Although this is a microstudy with a strongly applied bent, it does suggest more general observations. To survive, the arts, and especially those de-

pendent for their existence upon public performance, must adopt the organizational and financial structures of their milieu. This means that arts organizations in contemporary America cannot depend solely on individual local philanthropy but must exploit the entire available current opportunity structure, including governmental and foundation funding, and function through a bureaucratically organized administrative staff, as well as "amateur" volunteers. Salem's study highlights the importance of a congruent type of leadership without which the successful launching of this company would not have been possible. By adopting the role of the classic entrepreneur, recombining elements from the business world with those of art, Glynn Ross, the general director, has gained a reputation for success which gives him (and through him, the Seattle Opera) a position of power locally and nationally beyond the expectations one might have for a company whose annual repertoire does not exceed five works (p. 156). His methods include using a trendy marketing style for his "product"; occasionally performing an unconventional work to attract a younger public (*Tommy*); featuring a new opera (*Of Mice and Men, The Crucible*); doing some foreign language operas in English; developing close ties with the business community, whose connections with other cultural organizations as well as personal skills make them useful volunteers and/or board members (at relatively low financial outlay to themselves); eliminating the threat of local competition from opera performances at the university; making concessions to attract highly paid "superstars" to fill out the complement of predominantly "unknown" and low-paid performers, sometimes at the expense of aesthetic or administrative requirements (p. 76); gaining support from local critics who act as boosters; and successfully tapping foundation and federal funds. He has thus gained legitimacy in terms of "fiscal responsibility" vis-à-vis national funding sources and local businessmen-patrons and aesthetic standing vis-à-vis national critics.

Salem's use of the analytic distinctions among official, overt, and action goals of the organization, whose content and relative importance shift from one phase of the organization's development to the next, is a useful one which clarifies much about organizational process. Apparently unlimited access to records permitted him to provide uniquely precise data on a variety of matters usually shrouded in secrecy or difficult to obtain: salaries of stars as opposed to lesser performers, magnitude of donations by board members as compared to regular contributors, administrative and other costs broken down by salary and other expenses. He concludes with recommendations for improving the organizational management of this company. Among these are a strong suggestion for increasing the proportion of "significant donors, astute fund raisers, active 'doers,' or individuals who are influential in various civic circles" at the expense of business-oriented members, and one for including a variety of useful professionals, including educators. He also urges that thought be given to geographical representation as a means for increasing city, state, and regional support (pp. 183–84). Danger spots he identifies are: "administrative schizophrenia" among staff members who feel pride in their connection with the company but are increasingly dis-

satisfied with lack of coordination, guidance, reward; the organization's lack of information "on long-range planning, measurement of results, and promotional activities"; the lack of cooperation between artistic production and public relations; as well as the increasingly long absences of Glynn Ross as he attempts to gain an international reputation for the company.

Salem's study is lucidly presented in the systematic manner of one trained in business administration. From the point of view of the sociology of culture, however, it would have been of greater interest if he had examined more critically aspects which neither he nor most others organizationally involved in the arts bother to question. With respect to an undertaking such as this, the following would be relevant: (1) Is opera an inherently valid art form, or is it, as some believe, outdated and stagnant work (Salem suggests a repertoire of not more than 40 operas suitable for production), much of which should be definitively interred? (2) Is conventional "high art" for the hinterland a worthwhile target for public support, or should greater attention be given to support of "indigenous" forms? (3) Is a "grand opera" company a suitable vehicle for artistic growth and innovation, or would it have been better for Seattle if the university had been encouraged to produce works in competition with the opera company?

Finally, in spite of Salem's strong attraction to artists, he never even suggests that composers, musicians, artists, or musicologists be included at the board level. Surely, this would constitute a major innovation which, though likely to create many problems, could change the character of arts organizations and modify the adversary relationship which obtains between producers and overseers.

The Over-educated American. By Richard B. Freeman. New York: Academic Press, 1976. Pp. xi+218. $12.00.

Howard P. Greenwald

University of Chicago

Based on several of his previously published articles in journals of economics, Freeman's book should enrich the thinking of any sociologist interested in stratification, occupations, or labor-market phenomena. The book contains extensive data and discussion of the experiences of Americans with college and professional degrees in today's labor market. As the book's title implies, these experiences include disappointment and frustration for many. Freeman presents a sophisticated yet readable survey of the complexities of the market for educated labor, including time-series analyses, comparisons among occupational specialties, and judgments about the future. The volume provides sociologists with sufficient economic background to understand the actual advantages of higher education, which students of stratification often consider uncritically to be both a cause and an indicator of favored status.

Freeman, a labor economist, presents data primarily concerning employment, unemployment, and starting salaries in various fields, assembled from sources including the U.S. Department of Labor, the National Science Foundation, and private research organizations. His figures confirm the impressions of many noneconomists that the economic advantages of advanced education have declined markedly since the late 1960s. He notes, for example, that the salary advantage of college-educated individuals over high school graduates has declined substantially since 1968. The economic return on the costs of a college degree, he writes, has declined from over 10% in the early 1960s to around 7.5% at present. In an especially disheartening passage for college and university faculty, he interprets a vast set of statistics to mean that we now face "the worst job market for Ph.D.'s in American academic history" (p. 91).

Many of Freeman's conclusions are more surprising. An enlightening example is that the economic advantages of higher education were consistently overestimated in the public mind and the mass media even before the late 1960s. Freeman writes that government reports of college graduates earning an average of $100,000 more during their lifetimes than high school graduates were deceptive. The high income of college graduates, he suggests, could reflect advantaged social origins or strong achievement drives as easily as the benefits of education. In addition, inflation reduces the real value of the $100,000 figure, because dollars spent early in life or foregone as "opportunity costs" in the college years are worth considerably less in the form of wages later on.

Freeman's most interesting findings, though, concern variations in the fortunes of graduates with different specialties. The economic advantages of college graduates over high school graduates, for example, have increased for those in fields related to business management since the late 1960s. Engineers, whose problems received wide publicity in the early 1970s, have improved their position but should experience recurring cycles of good times and bad in the years to come. Lawyers, whose numbers have increased markedly since the 1960s, should experience economic ups and downs similar to engineers, as should the burgeoning crop of new MBA holders. For economic reasons not applicable to engineers, lawyers, and business managers, though, teachers at all levels and researchers both inside and outside academia should experience a steady decline in economic position and employability through the 1980s.

The analytical core of Freeman's book consists of two widely used concepts in labor economics: the "recursive-adjustment model" and the "cobweb-feedback system." Both these concepts help explain variations in supply and demand for specialized labor. Under the assumption that the career decisions of young persons are highly sensitive to the state of the labor market, the recursive-adjustment model predicts "a dramatic fall in the fraction of young men choosing to attend college" (p. 53). Freeman's data appear to substantiate this prediction. The cobweb-feedback system, a model associated in economics with commodities whose production involves

fixed periods of delay, helps explain variations over time. Like corn and hogs, the number of college and university graduates in the market at any given time depends upon market conditions prevailing at an earlier period. Because students find it difficult to adjust to new market conditions before they graduate, academia produces periodic surplusses and shortages of educated labor. The student who selects a field because it offers good economic prospects may find these advantages considerably eroded by the time he completes his education. On the other hand, the few who choose relatively crowded fields may find comparatively good opportunities when they graduate.

Freeman highlights the homeostatic mechanisms in the economy which should adjust the supply of educated labor to demand in the long run. Again, the prospects differ from specialty to specialty. Academic disciplines, for example, have few prospects of enjoying periodic labor shortages, both the declining birthrate and economic value of the college degree undercutting demand for their services. Similar nonmarket factors should plague research and development specialists, whose numbers swelled in the post-Sputnik era under heavy government funding, only to find weak demand when public commitment diminished. Women and racial minorities, on the other hand, have fared reasonably well in the market for college graduates, aided in part by affirmative action and related antidiscriminatory activity (pp. 184–85).

While the Freeman volume provides the noneconomist with a detailed picture of the market for college degree holders, it seems superficial on the analytical side. The book contains little in the way of innovative economic formulations. Some important issues in labor economics appear to be glossed over. One important example is the part monopoly plays in determining the economic position of certain specialists. Freeman reports that physicians, for instance, earn a spectacular 18% on their educational investment, due primarily to the restrictive admissions policy of their profession. Many economists, though, would estimate this return at a much lower rate, considering the opportunity costs and time invested in medical training. It would have been valuable for Freeman to deal with the issues involved in this estimate, as well as the more general issue of restricted entry into various segments of the educated labor market.

Freeman's ventures into sociological analysis are equally shallow. He writes that the decline in employability of college graduates has changed a generation of socially critical students into deferential, sycophantic job seekers. He presents only fragmentary evidence to support this claim. He comments further that "economic depression has historically been the bane of radical and protest movements" (p. 43), although many historical events indicate otherwise. He suggests that the declining value of the college degree may lead to a more egalitarian society (p. 189), a hypothesis hardly substantiated by the experience of societies with relatively few college-educated individuals. But it is not entirely fair to fault Freeman on the basis of his social analysis. This, after all, is primarily the sociologist's responsibility, one that many will find easier to fulfill after reading Freeman's book.

Penelope's Web: Some Perceptions of Women in European and Canadian Society. By N. E. S. Griffiths. Toronto: Oxford University Press, 1976. Pp. 249. $4.95 (paper).

Gerda Lorenz

Hunter College

The image of Penelope's web, Naomi Griffiths says, served her well for preparing a TV series on contemporary Canadian feminism in historical perspective but is rather a confusing guide for a book on the topic. That is, the title of the book does not reflect the complex twofold task Griffiths, a historian, has set for herself: (1) to trace the current discontent of Canadian women due to the prejudice and discrimination that have roots extending back to 17th-century Europe and (2) to come to terms with her own ideas about the present status of Canadian women, sex roles, and the women's liberation movement.

In Griffiths's own words, "*Penelope's Web* was written not as a scholarly work but in an attempt to understand the present status and position of women" (p. 237). Since scholarship and understanding are not mutually exclusive enterprises, I take her remark as an admission that she does not pursue her inquiry in a rigorously scholarly way, presumably because of the wide-ranging interdisciplinary approach she recognizes as necessary but also as treacherous for one to undertake singlehandedly.

Griffiths's historical account is based for the most part on secondary sources. Women's lives are viewed in the context of social, economic, and demographic forces; of urbanization; the agricultural and industrial revolution; and changing patterns of communication. Griffiths outlines the pattern of increasing prejudice and discrimination against women during the past two centuries, the fight for women's suffrage, and the subsequent decline of feminism in Canada and in Western European countries which provided the bulk of the immigrants to North America. Surprisingly, nothing is said about the organization and activities of the women's liberation movement in Canada, except for references to some contemporary feminists' ideas and attitudes toward the movement, expressed in the Canadian press and in literature. The Royal Commission on the Status of Women and its influential *Report* of 1970 are discussed in some detail.

With the historical part of her book, Griffiths makes a useful contribution to the comparative study of women. The full story of women in Canada, as Griffiths herself points out, remains to be told (pp. 133, 144, 186).

Griffiths's way of dealing with the second theme, the current status of women and related issues, presents problems. In brief, she considers the social differentiation of sex roles as desirable (pp. 21–22, 220) but thinks it should not be rigid and should not involve the marking of any characteristics as inferior or superior (p. 95). Biology and environment interact to create important differences between the sexes (pp. 223–24). A major shortcoming of the Royal Commission's *Report* is the supposition that "except for the process of birth men and women can function interchange-

ably if necessary" (p. 220). No reference is made to the research of John Money and his colleagues on the differences between primary (biological) and secondary (socially programmed) sex characteristics. Her attitude toward the women's liberation movement, judging by some of her comments, appears to be ambivalent (pp. 18, 203–4, 235). She certainly is not fond of radical feminism (pp. 206–7, 237). Conceptually and theoretically, this part of the book could have profited from a closer reading of relevant sociological and anthropological literature.

The author's lack of conceptual clarity is at times puzzling, but occasionally it has more serious consequences. For instance, a feminist's argument that in order to preserve options everyone should be "a human being first and a man or woman second" (p. 18) is countered by Griffiths with the objection that in our society we cannot "all approach one another as *total* human beings in all circumstances" but, depending on the situation, one is "a customer, or a student, or a shoemaker," that is, "at present twentieth century life demands the playing of roles" (pp. 18–19, my emphasis). An argument (expressed in Mertonian terms) about which status in a status set should have priority is explained away by a conceptual shift to an argument about the predominance of functionally specific roles in *Gesellschaft*.

Griffiths's overall lack of theoretical perspective is disconcerting. Sociological, anthropological, and psychoanalytical theories of sexual stratification and the sexual division of labor, such as those developed by Randall Collins, Sherry B. Ortner, Alice Rossi, and Gayle Rubin, would have suggested to Griffiths different questions and different answers. For instance, she would not have unhesitatingly assumed, without questioning, that social differentiation on the basis of biological sex is desirable *and* compatible with social equality between the sexes (p. 204), but would have taken into account Rossi's argument against a pluralistic model. She might even have reconsidered her final conclusion that for her ambition—equal status for men and women—to be accomplished, "ideas about femininity and masculinity [have to be] discussed with some poetry, some vision, some feeling for human beings" (p. 237).

Judging the book as a whole, it seems to me that Griffiths has taken on too ambitious a task to deal with in one relatively short book. The annotated select bibliography, though limited, may be helpful to newcomers to the field. A series of photographs and pictures of Canadian women cited in the book and of some historical scenes are welcome additions.

Authoritarianism and Corporatism in Latin America. Edited by James Malloy. Pittsburgh: University of Pittsburgh Press, 1977. Pp. x+549. $5.95.

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Though Malloy's volume contains four sections, its basic content is divided into a collection of theoretical essays and a series of case studies. The case

studies are uneven—some of them informative and insightful, others rather pedestrian and superficial. Among the better case studies is Mericle's discussion of the Brazilian military regime's labor controls and that regime's efforts to dismantle the working-class movement. Unlike some of the other contributors, Mericle has a firm grasp of the class forces that are affected by the "corporatist" efforts at domination. Nevertheless, the author, like most of the others, fails to specify and analyze systematically the class nature of the state which is involved in labor control. Sharpe's critique of community development in the Dominican Republic and his critical analysis of church-promoted reform illustrate how liberal reformism can lead to new forms of dependency and manipulation. Both Sharpe and Mericle, however, fail to provide a detailed discussion of the repressive methods (purges, disappearances, torture, etc.) within the larger environment that make selective repression operative within the worker and peasant movements.

The descriptive accounts of Skidmore, Malloy, and Dietz of the relationship of inflation, social mobilization, and clientelism to authoritarianism are useful, if not terribly profound or novel.

The flaw in this collection arises from its central theoretical notion, "corporatism," and the efforts to rewrite history to accommodate that notion and give it substance. First, the theoretical discussion lacks any firm and comprehensive grasp of the theoretical and political sources of corporatism in Italian thought. The work of Schmitter, cited by most authors, reflects only the most superficial grasp of the work of Gentile, Rocco, and the writers grouped around the Revisti di Studi Corporativi. Lacking theoretical clarity, the notion of corporatism is variably used to refer to policies, institutions, structures, regimes, ideological pronouncement systems, etc., all involving n some fashion state structuring of associational relations. This particular confusion derives from a larger problem—an incapacity to conceptualize the arger universe in which the so-called corporate units function.

Perhaps with the exception of O'Donnell, there is little effort to locate 'corporatist classes" and the circumstances in which they embrace corporatist solutions. What is generally served up is an inventory of "crises" (of all sorts), together with a recitation of activities and movements preceding or contributing to corporatism—all of which tells us little about the kind of social forces that require "corporatism" to reproduce themselves.

The internal contradictions of corporatism that have, for example, led to he demise of corporatist pretensions in Peru are nowhere evident in Palmer's essay. Malloy hints at class conflict and "mobilization" among Bolivians but carries the discussion no deeper. The failures of authoritarians are suggested by Chalmers, but, in place of a historically specific analysis rooted in the egion's social structure, one finds a rather vapid relativism which suggests hat with time all will change. Finally, the discussion of the relative autonomy of the state, one of the centerpieces of the essay, is used in a multiplicity of senses, each ad hoc formulation serving to explain particular circumstances. It thus has little theoretical utility.

In general, the account of the political process accompanying capital ccumulation, class formation, and class conflict tends to be one-sided. It

focuses largely on successful efforts to impose (partially or wholly), through the state, associations encapsulating labor, capital, etc. There is insufficient discussion of the class struggles that preceded the current situation and the past history of revolutions which overturned "corporatist" posturing, leaving corporatist ideologues and analysts to reflect over organizational husks. All the writers fail to deal with the tradition of class conflict which parallels corporatist pretensions or that, operating within corporatist forms, attempts to turn them into class instruments, as was the case under Velasco in Peru. In this regard, the contributors fail to give sufficient importance to the use of force and its application in order to impose corporatism, and to the fact that in many cases such force is applied against the democratic aspirations of the workers and peasants. Too easily and simply, some of the contributors assume that democratic struggles and aspirations and class solidarity are artificially grafted by democratic and Marxist theorists when a multiplicity of cases from Argentina to Cuba contradict their assertions. Finally, the essays almost totally neglect to discuss the role of the United States (CIA, Pentagon, etc.), the multinationals, and the international lending agencies, their demands, policies, and activities, in shaping the repressive apparatuses. Massive U.S. loans have accompanied institutionalized torture and ritual disappearances, and yet these elements remain residual categories in the corporatist mind. What is needed is a return to the classic tradition of studying imperialism and class domination and not more obfuscations around classless states, corporatist or not.

Race and Ethnicity in Africa. Edited by Pierre L. van den Berghe. Nairobi: East African Publishing House, 1975. Pp. xxviii+357. Sh 54 [$6.50] (paper).

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This volume, the only one on racial and ethnic relations readily available to the East African market, has a number of deficiencies. Responsibility for the most serious, the obsolescence of the data, must be shared by the editor and the publishers. The introduction predates publication by five years. The contents include articles, many of them undated, written between 1959 and 1969. In such a subject area these studies quickly become outdated. The responsibility for a second shortcoming of the book must be shared by all the contributors, and that is that it lacks theoretical coherence. The analysis of race and ethnicity, which should form a part of the broader concerns not only of stratification but, in the case of Africa, of the political economy of decolonization and underdevelopment, is too often dealt with in isolation from these factors. Conventional generalizations about social stratification, whether of the bourgeois or Marxist variety, touch only peripherally on the issues which arise out of the interaction of race and ethnicity as criteria for differential access to or control over resources. In this volume, van den Berghe has made no attempt to provide theoretical writings in this area.

The book is divided into three parts, the first dealing with broad issues of methodology and theory and the second and third containing case studies of traditional and modern systems in North, West, East, and southern Africa. The introductory articles do not take the reader much beyond the now widely familiar generalizations about the artificial or socially defined nature of ethnic or tribal consciousness, the causal relation between ethnic differentiation and conflict and political and economic change under colonialism, and about varieties of social meaning given to the word "tribe" in colonial societies. None of these issues is explored in depth. Van den Berghe reprints Immanuel Wallerstein's now classic piece on West Africa which, while it contains valuable conceptual distinctions, now appears somewhat dated in its defense of ethnicity as a mechanism of national integration. A general article by Paul Mercier on tribalism has not been rendered more coherent by van den Berghe's translation, while Leo Kuper's discussion of Weber's problem of elective affinities and religious action in South Africa is more suggestive in relation to white consciousness than to the religious responses of black South Africans to oppression. The remaining article, by Paul LeVine, on ethnographic approaches to group stereotyping in East Africa, is more interesting for the partial findings he presents of the cross-cultural study of ethnocentrism in East Africa than for its methodological discussion. One cannot help wondering why van den Berghe did not try to include a larger excerpt from the findings.

The rest of the book consists of largely descriptive national studies. Those on traditional systems, dealing with Rwanda and Kgalagari-Bushmen relations, serve the useful purpose of reminding students that institutionalized subordination on "racial" grounds did not originate in Africa with European colonization. The other articles, on modern systems, are uneven in quality and have no thematic or theoretical elements in common. There are two on North Africa and three on West Africa, none of them particularly noteworthy, dealing with, among other things, racial attitudes, ethnic conflict in post-civil war Nigeria, and ethnically based inequalities in education.

The limited perspectives of the selections on East and southern Africa, which occupy almost half of the book, are most disappointing, given the existence of recent studies in these areas attempting to link race, ethnicity, and class through a Marxist analytical perspective, such as the work of Colin Leys on Kenya and Issa Shivji on Tanzania. The essay by John Okumu on tribalism in Kenya and the one by Lionel Cliffe on localism and ethnicity in the 1965 Tanzania elections only hint at the later developments which brought into sharp contrast the cynical manipulation of ethnic loyalties to undermine class bases for political action in postindependence Kenya and the deliberate deemphasis on "tribalism" in Tanzania during the same period. Inclusion of some part of Leys's analysis would have brought Okumu's otherwise perceptive discussion up to date. Roger Scott's observations on trade unions and ethnicity in Uganda have become largely irrelevant since the exodus of Kenyan workers from that country starting late in 1970.

The southern African section makes strange reading, with no mention either of the older African nationalisms, racial and nonracial, or of the more recent black-consciousness movement. There is little mention of the colored population, but two articles on Indians, one dating from 1960. A more up-to-date selection might have taken note of theoretical approaches to the study of South African society which explicitly relate ethnicity, race, and material factors. It has been argued, in one of the more fruitful theoretical perspectives, that the maintenance of "tribal" economic and social institutions in the reserves and Bantustans of South Africa (and their accompanying cultural particularisms) can be explained through the functionality of these precapitalist enclaves for the development of the larger capitalist economy. This model, based on the notion of internal colonialism, accounts for the paradox of persistent "tribalism" within an advanced industrial economy. Through the subsidy to capitalism provided by the noncapitalist ("tribal") sector in the form of support to the aged, dependent, and disabled, the subsistence economy makes possible the payment of wages below subsistence level in the urban sector, while guaranteeing a supply of labor.

Van den Berghe's introduction, presumably intended for students, provides definitions of a number of terms in the study of race and ethnicity and follows these with a general discussion of racism. Some of these definitions would appear to be idiosyncratic, particularly his attempt to unify terminology used in the analysis of European and African societies by the blanket application of the term "nation" to "politically conscious ethnic groups" (p. xi). This usage, while it appears to avoid the dangers of ethnocentrism which have plagued the study of African ethnicity, begs the question of the minimum conditions necessary for a cultural unit to be regarded as a nation. It thus obscures an issue of concern both to Marxist theorists of the "national question" and to practical strategists of ethnic separatism. One might also take issue with some of his generalizations, notably his lumping together of "indigenous" racism and European racism and his inclusion of Kenya among those African countries where racism is today "most prevalent."

If van den Berghe's intention, as his publishers suggest on the book jacket, was to make available to students and teachers articles of historical significance on race and ethnicity, then he would have done better to have reproduced some of the classic studies such as the early work of Mitchell, Epstein, Gluckman, and others on tribalism or excerpts of such discussions as Crawford Young's on "The Politics of Ethnicity" in the Congo (now Zaire), which remains one of the best analyses of the "creation" of ethnicity under colonialism. By trying to cover a wider field and to produce more timely articles, he has preempted a rather small local market with an expensive book of very limited usefulness either here or to students in Britain and North America.

Toward a Redefinition of Action Theory: Paying the Cognitive Element Its Due[1]

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By means of an examination and a revision of Parsons's "action framework," this paper offers to mediate the debate between Parsons and the critics of his Durkheim-Weber "convergence" thesis and to present a more satisfactory rendering of Durkheim-Weber continuities. First, Parsons's *Structure of Social Action* is scrutinized. It is shown that Parsons so construes Hobbes that the "problem of order" for social theory is answered by the concept of internalized normative motivation. As a consequence of this focus, Parsons derogates an analytically independent "cognitive" element in action and relegates cognitive matters either to the "scientific" or "normative" orientations. Second, the interrelated series of articles by Pope, Cohen, and Hazelrigg are shown to rest on framework assumptions similar to Parsons's own, while drawing nearly opposite conclusions, rendering Durkheim a "positivist" and Weber a "utilitarian" in Parsons's sense of these terms. Third, a "redefined theory of action" is proposed which acknowledges explicitly the analytically independent role of the cognitive element in action. Some course-of-action statements drawn from Durkheim and Weber and focusing on the cognitive structuring of the "situation" and of "ends" are presented and shown to involve compatible reasoning. Some implications are suggested.

> "When *I* use a word," Humpty Dumpty said, in rather a scornful tone, "it means just what I choose it to mean—neither more nor less."
> "The question is," said Alice, "whether you *can* make words mean so many different things."
> "The question is," said Humpty Dumpty, "which is to be master—that's all." [LEWIS CARROLL, *Through the Looking-Glass*]

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No doubt other sociologists have experienced dismay at the tone of acrimony that came to characterize the debate between Talcott Parsons and Whitney Pope, Jere Cohen, and Lawrence Hazelrigg over the alleged "convergence" of Durkheim and Weber on a "voluntaristic theory of action." On the one side, we have Parsons's long-standing, scholarly, and sustained argument that such a convergence is a fact (1937). On the other is a series of three papers by Pope (1973), Cohen, Hazelrigg, and Pope (1975*a*), and Pope, Cohen, and Hazelrigg (1975) (who together will be called "the critics") arguing, with massive documentation, not only that such a convergence is not a fact but that in the attempt to demonstrate it Parsons seriously misinterpreted both Durkheim's and Weber's theories. Parsons's "Comments" (1975*a*, 1975*b*, 1976) and the rejoinders by his three critics (Pope 1975; Cohen, Hazelrigg, and Pope 1975*b*) have brought more heat than light to the resolution of their differences; the major agreement that came out of the debate was that the disagreements were fundamental. Although a couple of points have been resolved—Parsons has acknowledged that his periodization of Durkheim's career was flawed and that his characterizations of Durkheim's and Weber's theories were partial—these resolutions have not brought the parties closer together. In two respects they remain at loggerheads: whether Durkheim can be characterized as an "action" theorist and whether Weber adduced norms as typical and crucial explanatory variables (Parsons affirming each position, and the critics denying each). It is to this confrontation that the present paper is addressed.

The thesis of this paper is that an area of agreement can be found, not only between Parsons and his critics, but also between Durkheim and Weber; but that this can be done only if the terms of the debate (and that means the terms of *The Structure of Social Action* [1937], where it all started) are scrutinized. Such a scrutiny shows that the "action framework" with which Parsons approached Durkheim and Weber was severely skewed in such a way as to bias fundamentally the conclusions that could be drawn and in such a way as to mislead the critics. To be specific, I show below that Parsons's overall framework, if not his detailed exegeses, (1) assumes individual interests to be inherently disintegrative and, as a corollary, assumes a dichotomy between (social) "values" and "norms" and (individual or partial) "interests," and (2) systematically derogates a "cognitive" element in action, with the corollary that an "action" orientation comes to be by definition a matter of "normative motivation." Accepting instead of challenging these terms of debate and failing to find in either Durkheim or Weber a strong conceptual emphasis on individual normative motivation within an action framework, the critics are perforce impelled to reject Parsons's argument part and parcel. Hence the impasse.

If, however, we recognize that an actor can be "self-interested" and also

at the same time "oriented to norms" and that an actor's orientation (to norms as well as to other aspects of the situation) is composed of variable interpretative as well as motivational elements, where what actors "want" and "see" are conditioned alike by socially structured normative and cognitive systems, we can begin to outline a rapprochement between the parties to the debate and between Durkheim and Weber. And we can go beyond the impasse to a tentative reformulation of an action orientation, an orientation that is already informing some current theoretical work (see, e.g., Wiley 1976).

To prevent misunderstanding, I must remark at the outset that Parsons's theoretical contributions (e.g., Parsons 1950) have attempted to transcend (in directions suggested by both Durkheim and Weber) the superficial "interests versus values" dualism as well as to enumerate the "cognitive" alongside the "cathectic" and "evaluative" modes of orientation. Notwithstanding these contributions, *The Structure of Social Action* does have a tendentious framework, and the "values versus interests" dichotomy has appeared prominently in the work of at least one of Parsons's students (Neal 1965), while it has taken a years-long struggle by another of Parsons's students (Garfinkel 1967) to reassert the problematicity of the cognitive mode.

And lest the present paper (which does, after all, have a thesis and a theoretical agenda) be thought to represent merely a partisan statement, I will here enumerate a few matters that I take to be axiomatic (not to be defended here, but to be stated as informing the analysis). (1) Durkheim and Weber are very different as social theorists, as Parsons acknowledged (1937, p. 722) and his critics emphasize (Pope et al. 1975). Their differences are manifold, and among them are that Durkheim was a positivistically inclined theorist of harmony with a peculiarly sociocentric model of the actor, whereas Weber was a *verstehende* theorist of tragedy, whose actor was more oriented to problems of cosmic meaning (Warner 1976, pp. 108–10; see also Bendix 1971). (2) Notwithstanding these differences, contextual analysis may show that there are significant areas of agreement between Durkheim and Weber, significant not only for the light that is thereby shed on each of their theories but also for the help that can thereby be given to contemporary sociological theorizing. (3) *The Structure of Social Action* is a superb work of contextual-critical analysis. (Another fine work in the same genre is *The Coming Crisis of Western Sociology* [Gouldner 1970], which is less distinguished only because it is less ambitious.) (4) The "voluntaristic" aspect of Parsons's message has been scrutinized before (Scott 1962, 1963, 1974; Turner and Beeghley 1974; Parsons 1974, 1975*a;* Cohen 1975; Gerstein 1975; Johnson 1975) and is either a philosophic argument

or, strictly speaking, a redundancy in the attempt to establish a theory of action; as such, it is outside the scope of the present discussion.[2]

In addition, two of the methodological issues of the debate must be mentioned for orientation. Like the critical debate, this paper will concentrate on *The Structure of Social Action* rather than on the whole of Parsons's exegetical corpus (cf. Cohen et al. 1975*b*).[3] It is the value of the interpretation presented in that work, rather than Parsons's general scholarly capacities, that is at issue. However, textual "strict constructionism" (cf. Parsons 1975*a*, 1975*b*, 1976) is not appropriate for an effort of this sort, especially as the original Durkheim and Weber texts so constructed were often programmatic remarks written for polemical, even scholarly political, purposes. For purposes of theoretical analysis, we are no more obligated to take as definitive a theorist's protestations that he abjures "subjective" constructs than we are a politician's insistence that he is "not a crook." Evidence of actual practice alone can be definitive for such an inquiry; in the theorist's case, this means an analysis of his empirical explanations.[4]

I shall now proceed to the detailed demonstration. First, I shall examine the terms of Parsons's original argument. Then, having done so, I will show that a large part of his critics' argument and their disagreement with him rests on a similar framework and that this very framework is such as to prevent their agreement with him. Finally, I shall suggest a redefinition of the action framework and give examples of Durkheim's and Weber's action analyses that are compatible with the interpretations of both Parsons and his critics.

2 Gouldner (1970, pp. 189–95) suggests and Johnson (1975) demonstrates at some length that the concepts of free will and the moral responsibility of the actor were important ingredients of the hidden agenda of *The Structure of Social Action*. Yet Parsons himself has recently (1974; see also Turner and Beeghley 1974 and Scott 1974) disclaimed such a metaphysical intention. Taking Parsons's disclaimer at face value, "voluntarism" in his 1937 action scheme is reduced to a matter of the transcendence of the merely "natural." In other words, that which cannot be explained by heredity or environment is, ipso facto, voluntaristic. But this is simply to repeat what Parsons means by action (see Scott 1962, 1974). Given this prima facie redundant status of the "voluntaristic" argument, there can be little doubt that the reasons for its inclusion in *The Structure of Social Action* are those that Gouldner and Johnson have indicated, pace Parsons's disclaimers. Yet, since the purpose of the present paper is to examine the scientific value of Parsons's action scheme, such philosophical issues will not occupy our time, in spite of their undoubted importance in other contexts.

3 Hereafter references to *The Structure of Social Action* (1937) will be by page number in text.

4 A contextual and practical approach, rather than a "strict constructionist" one, is necessary to explicate Parsons's own theoretical efforts; see Cartwright and Warner 1976.

THE STRUCTURE OF *THE STRUCTURE OF SOCIAL ACTION*

Parsons claims to be making an empirical statement about empirically oriented theory, albeit a statement at an extremely abstract level. He says that it is a matter of fact that Durkheim and Weber (as well as Pareto and Marshall, with whom we shall not be concerned here) converged on a "voluntaristic theory of action" (v.t.a.) and that this convergence is evidence for the factually correct status of the theory. He insists on speaking of theory in the singular, whereas others had spoken of theories. "Convergence" means both a movement between thinkers, away from one tradition (e.g., positivism or idealism) toward the v.t.a. (e.g., Kant to Weber), and a movement within the work of one thinker (especially Durkheim). The v.t.a. represents an attempt to account for the fact of social order on the basis of the assumption of socialized, individual, rational actors. The v.t.a. combines the notions of the unit act and of normative consensus. The action framework includes the following analytic elements: an actor (who expends *effort* and makes *choices*); the *end* of his action (which in the v.t.a. turns out to be in part normatively constituted); and a *situation*, combined of (*a*) *conditions*, those aspects of the situation over which the actor has no control, and (*b*) *means*, those aspects subject to the actor's manipulation; —all this occurring within a framework of *normative regulation*, which partly determines the *ends* and sets normative constraints on the choice of *means*. The word "normative" in this scheme refers to elements of action insofar as they manifest "a sentiment attributable to one or more actors that something is an end in itself" (p. 75). "An instance of a norm is the statement: 'Soldiers should obey the orders of their commanding officers'" (p. 75).[5] Parsons does not insist that each and every act must manifest such a sentiment on the part of the actor, but he holds that actors do orient themselves to norms, and that a chain of upward reasoning to justify normative injunctions will eventually reach a statement of an ultimate

[5] The text recapitulates Parsons's explicit definition of the concept of the "normative." Yet, further on in his argument, it becomes clear that the concept requires more than that something be conceived of as an "end in itself." What else becomes included in the definition of the normative is the sentiment of "moral obligation" (pp. 382–88), or the taking as an end in itself of socially oriented (as well as socially imposed) definitions of the desirable. Thus, "interests," which may be defined as individually oriented conceptions of states desirable in themselves (e.g., material well-being, respect, salvation, etc.), are not conceptualized by Parsons as "normative." In this implicit definition, Parsons, Pope et al., and the present author concur. The disagreement concerns whether the categories of the normative (including norms and values) and interests are held to be logically opposed as well as analytically distinct. As will become clear below, these categories are conceived of as logically opposed (by the critics if not consistently by Parsons) only on the basis of the notion that norms can only be a factor in action if they are elements of motivation.

end.[6] In this sense, "in terms of the given conceptual scheme there is no such thing as action except as an effort to conform with norms" (pp. 76–77). The action framework, that is to say, defines action as normatively oriented, and Parsons disclaims any intention of deciding at the outset whether "human behavior is 'really' normatively oriented" (p. 77).

The theory of action comes out of the utilitarian tradition, which posited means-ends reasoning on the part of monadic individual units. But utilitarianism, says Parsons, could not explain the facts of social order without either making philosophically naive and objectionable assumptions (a "natural harmony of interests," whether implicit or explicit) or degenerating into a form of empiricist positivism (social order is analogous to astronomical order) which involves the abandonment of the idea of action, the idea that the actor is a variable in the process. To avoid these traps, Parsons argues, it is necessary to conceive of the normative element in action, where conceptions of the desirable are internalized in common among a plurality of actors, which conceptions serve partially to constitute the ends of action and to delimit the means employed. And this, Parsons says, is what Durkheim and Weber came to see. In a detailed exegesis of their works, Parsons claims to show this. It is Parsons's conclusion that the critics rightly reject. But assuming that his exegeses are not wholly in error, the question must be, How and why did Parsons go wrong? To answer this question, it is necessary to examine in detail two of Parsons's analytical moves.

The Hobbesian problem.—The utilitarian tradition begins with Hobbes's concept of men as creatures of reason in the service of passion. Under these premises, Hobbes posed the problem of order "with a clarity which has never been surpassed, and his statement of it remains valid today" (p. 93). Hobbes's state of nature is a state of war of all against all, and Hobbes's solution is the Leviathan, or rationally accepted coercive authority.

Parsons presents, without acknowledging the fact, two versions of the Hobbesian state of war. The first occurs when he interprets Hobbes as saying that desires are random because, in Hobbes's words, there is "no common rule of good and evil to be taken from the nature of the objects themselves" and hence, that the pursuit of such diverse ends produces conflict (p. 89). This interpretation of Hobbes is the fount of Parsons's refrain that the theoretical problem of utilitarianism is the assumption of randomness of ends (see pp. 59–60). But here Parsons's interpretation is incorrect: the cited statement of Hobbes comes in the midst of a discussion asserting that the natural inclination of man is to assume that what is good for him

[6] Thus, for Parsons, what is a norm, in the full sense, for one actor may be a means for another (p. 73). Nonetheless, to view norms as merely conditions, rather than ends, of action, violates the v.t.a.; and his scheme requires an "end-in-itself" sentiment somewhere among a plurality of actors.

is what is good in general (Hobbes 1651, pp. 48–49). Contrary to Parsons, the problem as so far stated is amoral individualism, not random wants.[7] The problem, in other words, is a lack of restraint on the means individuals use in the pursuit of their ends.

That becomes clearer if we look at Parsons's second, and correct, statement of the Hobbesian problem, which occupies most of five pages of *The Structure of Social Action* (pp. 89–94). In this reading, the Hobbesian problem is that men are creatures of desire, who want things and, what is more, security in the enjoyment of things. Their natural insecurity—they know that everyone else is as amorally inclined as they—impels them to seek power to secure the enjoyment of things. But power (including, says Parsons, "the ability to command the recognition and services of other men") is a means of action that is inherently limited. Power becomes a "proximate end" which "is inherently a source of division between men" (pp. 89–90; see also pp. 93–94). Thus, by clear implication, it would seem that not randomness of ends but scarcity of means is the basis of the problem of order. Hobbes's solution was to change the nature of the game by depriving each individual of the means for subduing each other individual, and this solution is characterized by Parsons as theoretically right but factually wrong (pp. 97n., 362).[8] By extension it is easy enough to see that this solution does not involve changing any but "proximate" ends. The solution is that all individuals, being rational, would agree to the monopolization of force by Leviathan, for such monopolization would give them the security to go about seeking their otherwise unchanged ends without the necessity of being obsessed with the acquisition of power (see Wolin 1960, pp. 265–85).

Following Parsons through these two interpretations of Hobbes is extremely instructive. Parsons does give (pp. 89–94) a very good statement of the Hobbesian problem, but out of it he extracts the (in the context)[9] fallacious argument that randomness of ends is the essence of the Hobbesian, and then the utilitarian, dilemma. In fact, most contemporary soci-

[7] That Parsons misrepresented Hobbes in this way has been earlier noted by Scott (1962). However, Scott's primary purpose in this regard seems to have been to criticize Parsons's characterization of Hobbes as a utilitarian (in Parsons's sense). That is not at issue here.

[8] Hobbes is "factually wrong" for Parsons because he does not see that men regulate themselves, but that he is "theoretically right" is indicative of Parsons's own framework.

[9] "In the context" because Parsons may have been suggesting the critique of welfare economics and (by implication) social contract theory later to be presented by Arrow (1963). That critique demonstrated that utilitarian theories cannot be made the basis for social order without implicit or explicit restrictions on the range within which individual utilities are allowed to vary. The same implicit qualifications operate in the work of recent contract theorists (e.g., Buchanan and Tullock 1962, pp. 80, 115–16).

ologists would undoubtedly follow Rousseau (1755, pp. 126–30, 201, 219–20) in faulting Hobbes for assuming that human nature is basically insecure, nasty, and aggressive.[10] Hobbes, in fact, assumed consensus on the desirability of security and comfort and of the proximate end of power, and, since power is inherently scarce, such consensus produces disorder. That this is obvious makes it all the more strange that Parsons does not say it. Rather, (1) he assumes that Hobbes's basic problematic—How do we get from amoral individualistic chaos to social order?—is a correct statement of the fundamental sociological problem; and (2) he misconstrues that problem so that it becomes one of randomness of ends. Already we begin to see that he is moving in the direction of saying that individual interests are inherently disintegrative because they are unstructured and that the solution to the problem of order is to be found in the structuring of ends and means. He will imply, without justification, that norms and values are inherently integrative and that consensus on them is the solution to the problem of order (see Schwanenberg 1971). Such a view ignores the fact that such normative injunctions as "honor thy father" and "revere the emperor" are by no means automatically conducive to social order.

Parsons indicates his approval of the paradigmatic correctness of Hobbes's starting point in many places (pp. 91, 97n., 105, 109, 113–14, 120, 313–14, 327–28n., 381, 435). He assumes here, following Hobbes, that individualism is naturally disintegrative. What Parsons adds to Hobbes is that this problem is solved by the internalization of social, structured normative elements. That there is an equally compelling problem of social order, which begins from the assumption that men are disposed to cooperate and which focuses on the way they come to construct a world in common (Simmel 1908), is disregarded by Parsons in his concentration on the Hobbesian problem.[11]

Following from this orientation, then, Parsons has set up the following choices. (1) Order is either "natural" or it is not. He says, following Hobbes, that it is not and that those (following Locke) who think that it is natural are simply wishing the problem away (p. 97). (2) Given that order has to be explained, it can be on the basis either of coercion (which

[10] Similar criticisms have been directed to the recent neo-Hobbesian approach of van den Berghe (1974); see Fischer (1975) and Mazur (1975).

[11] The problem of social order as a problem of coordination is central to the work of, among others, Park (1927), Goffman (1967, 1972), Garfinkel (1967), and Schutz (1963). Those who concentrate on the "Simmelian problem of order" may belong to a philosophic tradition in which basic harmony, rather than Hobbesian conflict, is seen to characterize social life (e.g., Lippmann 1922), or to one in which the problem of structuring an inherently meaningless world is every bit as urgent as that of overcoming individual interests (e.g., Berger 1971). These traditions converge in the work of Goffman.

is both empirically unrealistic and philosophically objectionable in its positivistic implications) or of the normative element.

Parsons's forcing of this choice can be seen in his distinction (pp. 91–93; see also Schwanenberg 1971) between a "normative order" and a "factual order." Science presumes factual order in the sense of scientific predictability and amenability to logical treatment, but this is not the order Hobbes wants to guarantee. "Thus the 'struggle for existence' is chaotic from the point of view of Christian ethics [i.e., is not a normative order], but that does not in the least mean that it is not subject to law in the scientific sense, that is to uniformities of process in the phenomena" (p. 92). Parsons never gives a neat definition of a normative order, but he says that a normative order is at issue in the Hobbesian problem. In other words, Hobbes wants to institute an order in which the normatively desirable criterion of "the attainment of human ends" is realized. So far, Parsons is simply stating that we can scientifically conceive of and analyze (if not expect to see) social conditions that are unlivable, and he may be allowed the term "normative order" to designate livable conditions. But Parsons engages in sleight of hand when he implies that such a normative order can only be normatively determined (p. 113), or that we can only enjoy a normative order under conditions of "normative regulation," in the sense explicated above. Hence, says Parsons, "a social order is always a factual order . . . but, as will be later maintained, it is one which cannot have stability without the effective functioning of certain normative elements" (p. 92).

So, without saying so quite explicitly, Parsons identifies social order with normative order and disregards the possibility that the road out of social chaos can be anything other than normatively determined (cf. pp. 385, 670). This normative determination will turn out to involve normative structuring of ends and means. This perception Parsons sees especially in Durkheim, who, according to Parsons, was concerned with the Hobbesian problem.[12]

Parsons's tendentious use of the Hobbesian problem, then, establishes a series of oppositions on the basis of a sustained problematic. The problematic is, How do we get individuals to overcome their naturally antisocial inclinations? and the oppositions thereby assumed are between interests, dissension, and disorder, on the one hand, and norms, consensus, and order, on the other. Insofar as a theorist has a correct (that is, action-oriented) approach and insofar as he explains the prevalence of social order, then, Parsons is almost forced to view him as working with the terms of the

[12] That Durkheim was concerned with the Hobbesian problem is a matter of dispute (see Giddens 1976, pp. 705–9). That Durkheim's model of man stressed his malleability rather than (with Hobbes) his fixity should be a warning against the facile identification of Durkheim's concern for order with Hobbes's.

voluntaristic theory of action, and his conclusions regarding Durkheim and Weber have to be understood in this light. Yet Parsons has not proved his point. He has merely suggested a series of forced analytic choices and by implication a number of empirical propositions to the effect that social order must be normatively determined.

Action theory and disregard for the cognitive element.—This examination of Parsons's framework began with the Hobbesian problem because that was undoubtedly Parsons's key concern. Yet he reaches his analysis of it only a goodly way into his book (p. 91), and he recognizes that it is a different issue from that of the "action theory" with which the substantive part of the book begins (chap. 2, p. 43). That is, the explanation of social order is one thing and the proper understanding of human action is another. Parsons is not idiosyncratic (see Berger and Luckmann [1967], for a different approach) in wishing to answer both questions in one theory (p. 56), but they are different questions. We will now turn to the action side.

As stated above, Parsons begins with a utilitarian theory of action and modifies it to become the v.t.a. What he says, in effect, is that for a theory of action to be viable it must be a voluntaristic theory involving a normative component.[13] Why should this be so? Parsons would answer in this way: An action theory is an attempt to account for human conduct with the recognition that people assign subjective meaning to their acts (pp. 11, 26, 46) and (this is what Parsons's whole book claims to show) the emphasis that this subjective element matters empirically. It is a fact, Parsons claims, that people account for their acts in terms of subjective motives; it is not only a fact but an empirically consequential one. Hence, the emphasis on action (which in contrast to "behavior," by definition, involves a subjective component) is intended both philosophically and scientifically to comprehend the facts of our existence as human beings. An action orientation has its intellectual roots in the distant past and has now become a matter of common sense (p. 51). Thus, Parsons says, in effect, that taking the actor's subjective orientation into account analytically and treating him as a choosing creature is no news to the man in the street. But when looked at closely, this requires that the analysis of action include the normative element of action. (It requires, that is, that we scientists impute norms to the actor, not that we take a normative orientation toward the actor. What is subjective from the point of view of the actor is an objective fact from the point of view of the scientist [pp. 46, 49n.].) The normative element is crucial, for without it the actor is reduced to a scientist, an objective observer of reality (pp. 58–59). An objective observer of reality, Parsons concludes, is not a variable element.

[13] Scott (1962) in part agrees with Parsons here, calling "positivism" (as Parsons defines it) an "impossible theory of action."

Hence, to see the actor only as a scientist is to vitiate the action orientation and to devolve to a positivistic theory where objective facts predict other objective facts. But taking the point of view of the actor makes sense, Parsons says, only if we include the normative element; and postulating choice is possible only if we include in our analysis the criteria on which choices are made, and those criteria are bound up with the normative element, as defined above. This is much of the burden of chapter 2 of *The Structure of Social Action,* and it involves a key non sequitur.

The non sequitur is simply this: Parsons nowhere proves that the normative element is the unique analytical device that can rescue an action orientation. Without the normative element, Parsons seems to say, we are left with only biologically derived "random" ends and the objective, "scientific" assessment of means and conditions (pp. 44–45, 75). But at least one other analytic device suggests itself to rescue action from positivism, and that is a socially structured cognitive element. It is a fact that we selectively perceive (and misperceive) means and conditions (among other elements of action), and a reasonable prima facie supposition is that ends are experienced by the actor less as desirable and more as given. If, with Parsons, we wish to maintain an action orientation, cannot the cognitive element serve?

By the "cognitive element," I mean to designate an aspect of action parallel to, but independent of, Parsons's normative element, as I have explicated the latter term above. The following definition will suffice: "The cognitive element is that element in action consisting of an orientation toward the belief in the actual or potential existence of things (events, relationships, etc.), whether experiential or supernatural." As will be seen below, this definition is compatible with Parsons's use of the term. The cognitive and the normative overlap empirically but are analytically distinct, and, just as the statement "Soldiers should obey" is in the normative mode, so the statement "The norm is that soldiers should obey" is in the cognitive mode. Thus, the cognitive element includes the sorts of statements attributable to scientists like Parsons, but use of the analytic designation "cognitive" implies no presumption as to the factual accuracy or empirical verifiability of such statements. Therefore, statements presuming to be matters of knowledge are cognitive statements: "The sun revolves about the earth"; "The world is flat"; "Lee Harvey Oswald acted alone"; "The United States is suffering a shortage of natural gas"; "Violence on television is harmful to young children"; and "Marijuana is not harmful." (I deliberately have chosen controversial or demonstrably false statements here, but statements that have been affirmed by masses of ordinary people. The reasons for this will become clear soon enough.) Thus, also, statements about ultimate reality, or about the existence or nonexistence of supernatural powers, are beyond experience and empirical refutation, but they

too are in the cognitive mode: "There is but one God"; "The Bible is literally true"; "History has a meaning"; "I am saved"; and "The world is as it seems to be."

Parsons certainly recognizes the cognitive nature of such statements, and he does take account of a cognitive orientation both in *The Structure of Social Action* and in the later *Social System*. But he does not, in the former work, include the cognitive element within the action frame of reference, and, as already implied, he systematically overlooks it in favor of the normative element. What happens is that Parsons makes two analytic moves: one is to assimilate the cognitive to the scientific (in effect to deny that the cognitive element is a variable, particularly with regard to matters of actors' "knowledge"); the other is to assimilate the cognitive to the normative (in effect to recognize its variable status but to reduce it to his prime category, particularly with regard to actors' "beliefs").[14]

1. The cognitive element as a merely scientific orientation. It should be clear that a theory claiming to view action conceptually from the point of view of the actor must take the cognitive element into account. Parsons knows this, and, for example, in his discussion of Durkheim he says that religious ideas belong to a "category of cognitive elements" which are neither norms nor ends but "existential references to what are to the actor contemporaneous things, persons, entities" (p. 419; see also p. 424). And very early in his book, he criticizes positivism for assuming "explicitly or implicitly (more often the latter) the view that positive science constitutes man's sole possible significant cognitive relation to external (nonego) reality . . ." (p. 61). Insofar as positivism does this, the actor is conceived of purely as passive, and action theory (as we have seen) is vitiated. The scientific orientation is only a subset of the cognitive, Parsons points out; the former does not exhaust the latter.

Notwithstanding Parsons's sophistication about these matters, he regularly falls into the trap of assuming (usually implicitly) that when a cognitive orientation is adduced, one is dealing with a conception of an interpretatively passive actor (what Garfinkel [1967, pp. 66–68] will call the "cultural dope" concept). Parsons makes both this error himself and the error of attributing it to others, most especially to Durkheim. In his discussion of behaviorism and Darwinism, for example, he once again criticizes positivism, but with an interesting nuance: Behaviorism approaches "the

[14] Parsons's technical Note B, "Schematic Outline of System Types in the Theory of Action," (pp. 77–82), subsumes "subjective" (including cognitive) orientations under the rubrics of *T* (scientifically valid knowledge), *t* (unscientific elements, due to error, fallacy, or ignorance), *r* (random elements), and *i* (normative or ideal elements). This section of the present paper can be read as an extended interpretation of the exegetical use to which Parsons puts this abstract framework. Note that this framework assumes that there *is* a situation, of which actors may have more or less accurate knowledge and to which they may bring one or another normative orientation.

subjective aspect of action in terms of the role of scientific knowledge, that is, the standard has been the *cognitive* aspect of the subjective" (p. 116); thus, Parsons connects the scientific and the cognitive. And later (p. 431) he expresses agreement with A. D. Nock that our relations to religious symbols "are not merely cognitive, but also involve active attitudes," thus identifying the cognitive as merely passive (see also p. 467). In other parts of his exposition, Parsons's critique of positivism's overemphasis on neutral knowledge seems to bias him toward neglecting the cognitive element altogether (pp. 57, 67, 120, 123).

The problem becomes most serious when Parsons discusses Durkheim. He recognizes that Durkheim had a "cognitive bias" (pp. 360, 387, 445), but he attributes this tendency to residual positivism and hence assimilates Durkheim's emphasis on the cognitive to an emphasis on the scientific (p. 468; cf. Pope 1973, pp. 400–401). In trying to place Durkheim's discussions of social facts into an action framework, Parsons recognizes that Durkheim adduced rules of conduct as elements of action (e.g., contractual norms in *The Division of Labor* [1893]; see below, "Action cognitively oriented to norms"); but since Parsons fails to find in Durkheim's early work the notion that these rules are conceived by the actor as ends in themselves (i.e., the notion that they are normative in Parsons's sense), he has to characterize Durkheim's approach at this stage as "positivistic." In this view, social facts become mere "conditions" of which the actor has passive, quasi-scientific knowledge (pp. 365–66); and Parsons's Durkheim only begins fitfully to overcome the view of ego as "a photographic plate, a registry of facts pertaining to the external world" (p. 388) when he (according to Parsons) begins to appreciate the sentiment of moral obligation. Hence it is that the sentiment of moral obligation is the sine qua non of Parsons's action framework. The possibility of a third position between positivism and the (normative) v.t.a., where the actor's orientation is seen as something other than epistemologically passive, is regularly ignored by Parsons (see pp. 384–85n., 396–97). It is difficult to see how cognitive sociologists such as Goffman (1972), a celebrant of the interpretative difficulties of social interaction and hence of the cognitive activity and interpretative competence of the actor, could draw on the Durkheim that Parsons depicts (cf. Stone and Farberman 1967).

Here another of Parsons's conceptual conflations contributes to his sleight of hand. He emphasizes that action is active, that it involves an expenditure of individual effort to maintain or change a state of affairs. He tends, however, to conflate this notion of activity of conduct with activity of choice and (conversely) to conflate concepts of actors' behavioral and epistemological passivity. The notion that the cognitive element may represent a socially structured variable, which the actor does not "choose" to take but which is nonetheless not given by the purely objective aspects

of the situation (i.e., that the actor's point of view is still analytically necessary), does not seem to occur to him. Hence, when Durkheim, in *The Elementary Forms* (1912), brings together his seminal observations on the social structuring of religious ideas and interpretative ideas (e.g., causality, classification, etc.), Parsons must say that he has overshot the v.t.a. and "gone clean over to idealism" (p. 445). He correctly recognizes that Durkheim still has a cognitive bias, but he cannot see this as compatible with an action orientation because the elements of choice and the normative are not sufficiently prominent. He characterizes Durkheim's positivistic and idealistic phases (a conception of Durkheim's theoretical trajectory that is refuted by Pope [1973]) as alike involving a "passive" actor, as if there were something less passive about Parsons's own theory of normative internalization (pp. 385–86).

Clearly, what Parsons sees but cannot accept into his scheme is precisely Durkheim's "cognitive bias," and Parsons is able to say that Durkheim converges on the v.t.a. only insofar as he (correctly) sees Durkheim emphasizing the causal role of norms (which is not equivalent to normative orientation in Parsons's definition) and as he emphasizes those passages where Durkheim speaks of the actor's choice as based on a sentiment of moral obligation (which *is* normative orientation). Hence, Parsons passes up numerous opportunities (later to be picked up by, among others, Collins [1975]) to incorporate Durkheim's conceptions of the role of the cognitive in social action into an action frame of reference. He does this because he commits the same mistake of which he accuses the positivists, that of assimilating the cognitive to the neutral, passive, objective, and scientific.[15]

2. The cognitive element as subordinate to the normative. Parsons's procrustean treatment of Durkheim is the more surprising when we consider not only his recognition in Durkheim of positions that are not compatible with his framework but also his astute statement of the role of ideas in Weber's theory. By way of concluding his chapter on Weber's religion-and-capitalism thesis, Parsons appended a "Note on the Role of Ideas" (pp. 533–38), which clearly and nicely states that religious ideas serve to structure the situation (a cognitive function) in which religious interests (e.g., the interest in "salvation") are pursued. Parsons later correctly states that "the religious ideas Weber is primarily concerned with are not as such exclusively value ideas. . . . They do not themselves constitute ends of action but rather a framework of ideal conditions under which ends may

[15] That Parsons's conceptual scheme is heavily weighted toward motivational questions with normative answers rather than toward orientational questions with cognitive answers is seen again in his concluding chapter where illustrations are given of actors with objective knowledge (about, for example, the laws of gravity [pp. 734–36, 742]) and where "culture" is declared to be not a system of action and where ideas are taken as constants for the theory of action (pp. 758, 764). So recently as his last entry into the debate with his critics, Parsons has reaffirmed this emphasis (1976, p. 361).

be pursued" (p. 668). (Parsons here draws on Weber's paradigmatic statement of the role of ideas [1915, p. 280], which statement is also emphasized by Parsons's critics. I shall return to this issue below.) Given Parsons's clear recognition here of the cognitive element in Weber, how does he escape the obvious conclusion that the cognitive is a key analytically independent element in action and (by extension) an additional way out of the positivist trap? The answer is that that part of the cognitive element which is not assimilable to the scientific, especially the nonempirical aspect of the cognitive, is assimilated by Parsons to the normative element. He does this mostly in his discussion of Weber, but this move also takes place in his discussion of Durkheim.

Given Parsons's view that Weber derives more from the idealistic tradition (which stems from Kant's epistemologically activist theory), Parsons would be hard put to say that any "cognitive bias" (not something he attributes to Weber) in Weber stems from a residual positivism. Moreover, Parsons recognizes Weber as the fount of his own definition of action, so he cannot accuse Weber of wanting to skirt or overshoot an action theory. So Parsons must take a different tack here in his conceptualization of the cognitive element in Weber.

His full-blown answer comes in his discussion of Weber's "charisma" in chapter 17 (pp. 640–94) where ultimate beliefs seem to be traced to a value attitude. But adumbrations of this interpretative tack are evident earlier in his discussion of Durkheim. From time to time, Parsons seems to treat the cognitive and the normative as formally parallel. Thus, "religious ideas [cognitive] are to be regarded as partly determinant of, partly determined by, men's ultimate-value attitudes" [normative] (p. 424). Then again, Parsons appears to subordinate the cognitive to the normative: "Analytically regarded the reference of religious ideas cannot be to any empirical reality at all, if they are to be held to represent the principal existential *cognitive element of the ultimate value complex*" (p. 421, emphasis added. Parsons appends a footnote disclaiming metaphysical implications; his point here is an analytical one).

The keystone to Parsons's thinking here comes in a radical separation between the cognitive aspect of nonempirical beliefs and that of empirical knowledge about the prosaic "situation," and this separation is combined with a view that the nonempirical cognitive element is essentially bound up with the (normative) ultimate-value complex. Thus (from his discussion of Durkheim):

> Already it has been necessary to conclude that the collective representations, in so far as they constitute the cognitive element in a common value system exercising moral authority, do not stand in the same relation to the "external world" as do those elements in the subjective which constitute scientifically verifiable knowledge of the means and conditions of action. [Note that here Parsons excludes the possibility of a cognitive

> orientation to the empirical situation that is not merely "scientific." Parsons continues:] The underlying reason for this has been found to lie in the normative character of the value elements in relation to action. *That which is of normative significance must, in the nature of the case, be analytically distinguished from any elements which play a role in the situation of action.* [P. 418, emphasis added]

What Parsons does in this passage is to confuse the levels of contrasting normative significance to situational elements. It is reasonable, in contrast to this bit of Parsons's phrasing, to suppose that many aspects of the "scientifically ascertainable" situation are perforce of normative significance, but Parsons evidently wants to convey the impression that, in this case, collective representations are inherently analytically normative elements. He does not quite make this identification explicitly, but he evidently holds that normatively significant cognitive ideas are inherently bound up with the normative orientation. But about as close as he comes to saying as much (in his discussion of Durkheim) is this: "Since the nucleus of the social element in a normative sense lies in the existence of a common system of value attitudes, it is to be expected that these will, in turn, *be associated with* a common system of religious ideas" (pp. 424–25, emphasis added). The implication to be drawn is evidently that, even if we may distinguish analytically between the normative and cognitive orientations, those aspects of the cognitive element that are significant for the normative system are empirically so bound up with the common value system as to form a subset of it. Following Parsons's logic on this score, we would have to conclude that cognitive revolutionaries are also value revolutionaries. Yet it is not at all obvious that such cognitive innovators as Plato, Copernicus, Columbus, Descartes, Luther, and Calvin, to mention a few, were anything other than normative conservatives (cf. Warner 1970*a;* Wolin 1969, pp. 1065–69).

Parsons's theoretical tendency in this regard becomes clearer when we turn to his discussion of Weber. Though his treatment is careful and for the most part accurate, his "Note on the Role of Ideas" concludes with a characteristically tendentious but circumspect observation:

> The question of the role of religious ideas, which is involved in so acute a form in Weber's work, is in part a phase of the broader question, which is of central interest to this study, of that of value elements. The clear distinction made by both Pareto and Weber between scientific and non-scientific ideas has cleared the way considerably. Part, at least, of the latter form a cognitive element in the value complex. The status of such ideas has been clarified by the above discussion of Durkheim's treatment of religion. Weber goes still further in clarifying it. But it seems quite apparent that the cognitive constitute only one group of elements in the value complex. Knowing, or believing, is not, as such, doing. In addition an element of effort of some sort is needed. The actor does not take to-

> ward his ideas the emotionally neutral attitude of the scientist which has played such an important part in the discussion of Durkheim. [Pp. 537–38]

Here Parsons implies two propositions, which he neither states baldly nor demonstrates. One is that values provide the element of effort, whereas for Weber he has already acknowledged that interests are the driving force of action. The other is that the actor's attitude toward empirical cognitions is affect free. Such implicit propositions are the necessary steps in his forcing of the nonempirical aspect of the cognitive element into the value complex.

Having stated Weber's case clearly, albeit with a biased conclusion, Parsons does not return to the question of the relation between the normative and the cognitive until he concludes his discussion of Weber with his chapter on Weber's "Systematic Theory" (pp. 640–94). There it becomes evident that Weber's concept of "charisma" is broadened to become parallel to Durkheim's "sacred" and, like it, is seen to be the fount not only of ultimate values but also of what we might call ultimate beliefs: "That is, men's ultimate-value interests are in the nature of the case inseparably linked to their conceptions of the supernatural. . . . It is hence through this religious reference that charisma may serve as the source of legitimacy. That is to say, there is an *inherent solidarity* between the things we respect (whether they be persons, or abstractions) and the moral rules governing intrinsic relations and actions" (p. 669, emphasis added).

With respect to cognitive elements of a nonempirical reference, then, Parsons has established a framework which (as soon as we recognize that both Durkheim and Weber devoted substantial attention to religious ideas) requires that their theories be subsumed under the (normatively deterministic) voluntaristic theory of action. To overstate the case only slightly, in this respect Parsons's convergence thesis is "proved," almost by definition, as soon as one acknowledges that Durkheim and Weber wrote about religious ideas, whether in a normative or a cognitive sense being immaterial. Although Parsons's exegeses of both Durkheim and Weber acknowledge their analyses of cognitive processes in both empirical and nonempirical respects, his framework requires that he systematically derogate these analyses, or, to put it more strongly, his framework denies that the cognitive element is an analytically independent factor in action, being absorbed into either the "situation" or the "value complex."

Summary.—Both Parsons's treatment of the Hobbesian problem and his treatment of the cognitive structure his argument so that the sole way conceptually to include in his interpretations of Durkheim and Weber their analyses of the operation of cultural variables is to assimilate these analyses to the voluntaristic theory of action. The basic problematic of the v.t.a., in turn, stresses motivation to accept social control by normative considerations and neglects a potentially independent, empirically consequential role for cognitive factors. In this way, Parsons's intricate and provocative

interpretations of Durkheim and Weber lead to the truncated conclusions that his critics rightly reject (Pope et al. 1975). But in rejecting his conclusions, the critics do not challenge his framework, and their corrections of those conclusions still do not exhume many of the insights into Durkheim and Weber that Parsons's framework required him to bury.

AGREEMENT WITHIN DISAGREEMENT: THE CRITICS' VIEW

With the foregoing exercise in mind, we now return to the issues of the debate between Parsons and Pope, Cohen, and Hazelrigg. Having shown that Parsons implies an analytical choice between interests and norms and that he searches for an action orientation primarily in terms of a socially inculcated sense of moral obligation rather than also in terms of a socially inculcated set of cogitive orientations, I will now maintain that if the critics demonstrate that the category of interests plays a large role in Weber's theory (as the critics do) and if they acknowledge that socially defined ideas play a role in Durkheim's theory (as they do), then they are not so much disagreeing with Parsons's detailed renderings of the two theorists but rather with his conclusions; and that they overstate their disagreement with him because they do not transcend his framework and in effect interpret it more rigidly than he did. That Parsons's exegeses of Durkheim and Weber in *The Structure of Social Action* are as complex as they are and that they have been as influential as they have been are no doubt due to the fact that Parsons himself does not apply this framework rigidly in them. It is this character that likely makes *The Structure of Social Action* "frustrating" to the critics (Cohen et al. 1975*a*, p. 230n.). But they end by taking Parsons more at his word than he himself does; and, while accepting his framework, they dismiss as palpably absurd its conclusions as applied to Durkheim and Weber.

The explication of frameworks resolves the debate in a limited way. I do not claim that all the issues (especially those regarding the overall tenor of Weber's work—whether he is properly regarded as a "political realist"—and the interpretation of Durkheim—whether he was in any sense a "voluntarist" [see Cohen 1975; Parsons 1975*a*]) can be resolved in this way. But resolution of what are perhaps the two major confrontations of the debate can be provided. In particular, the foregoing analysis allows us to say that Parsons and his critics are both right on two counts: Weber could view individual interest as the fundamental dynamic of social action and view actors as oriented to norms (because he did not accept the interests versus norms dichotomy); and Durkheim could view individual states of consciousness as crucial and be a social realist (because he did not accept the individual versus society dualism as it is posed by Parsons's Hobbes).

Parsons's *Structure of Social Action*, as I have shown, addresses two

questions, that of the problem of order and that of the construction of an action theory. Through some intricate (and not always airtight) reasoning, Parsons arrives at the same conclusion to both questions: that order and action are possible only on the basis of the assumption of individual normative orientation. The "convergence thesis" is that Durkheim and Weber arrived at the same conclusion. Parsons's critics do not themselves address the questions that motivated him, but they do address and take issue with his conclusion as it regards Durkheim and Weber. Above all, they deny that Durkheim was an action theorist and that Weber emphasized the role of norms.

The critics' denial of Parsons's conclusions, however, is stated in terms of their own framework, which may itself be derived from Parsons's or from extrinsic sources. Whatever its source, the critics' framework accepts a version of the individual versus society dualism and the interests versus norms dualism. Because of this framework, the critics do not challenge Parsons's framework but rather draw conclusions nearly opposite to his, interpreting Durkheim as what in Parsons's framework would be called a "positivist" (the actor is not a variable) and Weber as a "utilitarian" (ends are random). Failing to break out of Parsons's framework, the critics introduce distortions of their own.

Individual versus society.—The critics' case for the rejection of Parsons's interpretation of Durkheim and their consequent rejection of the Durkheim-Weber convergence thesis rests heavily on the claim that "Durkheim emphatically rejected the use of the action frame of reference for explaining social phenomena" (Pope et al. 1975, p. 426). Aside from adducing in support of this claim the patently sociologistic and antipsychologistic remarks that Durkheim routinely made, the critics provide a number of specific explications of Durkheim, many of which presuppose an illegitimate individual-social dualism.

First, and clearly most important, is Pope's citation of the concept of the "radical heterogeneity of the individual and the social" which appears "throughout [Durkheim's] work" (1973, p. 413). This concept, according to Pope, is such as to vitiate any claim that Durkheim could be construed as an action theorist, for it presupposes that the sources of social order (and social organization more generally) are not to be sought in individuals but in society itself. Pope's claim, however, is overstated. (1) As a substantive contrast, the dualism that he cites appears primarily in Durkheim's later work, particularly in *The Elementary Forms of the Religious Life* (1912) and "The Dualism of Human Nature and Its Social Conditions" (1914), which was the stage Parsons characterized as "idealistic."[16] In

[16] Pope's paraphrase (1973, p. 413) of the "radical heterogeneity" concept does not cite specific works, but it reads like a summary of the philosophical framework of Durkheim's *Elementary Forms* (1912, pp. 259–60, 297–99, 345, 355–56, et passim).

earlier works, especially *The Division of Labor* (1893) and *Suicide* (1897), Durkheim presents the individual and society as in at least potential harmony (Warner 1976, pp. 72–77). (2) Although Durkheim's rhetorical style was thoroughly dualistic (see Lukes 1972, pp. 16–30), his dualisms were inconsistent. In particular, he located dualism within the individual psyche (1914); he also attributed needs for affiliation and regulation to the individual (1897) and maintained that the very process of individual thought was social in derivation (1912).[17] It is as much a reasonable interpretation that Durkheim's dualisms were part of a normative and polemical strategy of his own as that they were intended to deny that individuals and society are mutually interpenetrating. (3) For polemical methodological purposes (as Pope [1973, p. 413] recognizes), Durkheim regularly and throughout his work contrasted psychological (or individual-based) and sociological modes of analysis, but Pope's claim that such contrasts represent a social realism incompatible with an action frame of reference is unsupported.

Several of the critics' further points rest on an implicit acceptance of an individual-social dualism. Second is the claim that, although Durkheim recognized and appealed to subjective states in many of his analyses (Pope 1973, pp. 406, 408; Pope et al. 1975, p. 419), he viewed them as "effects but never as causes." Failing to provide other than Durkheim's programmatic remarks in support of this contention, the critics cite only his numerous and uncontested statements and analyses to the effect that psychic states relevant for sociological purposes are "prolongations . . . inside of individuals . . . of the social causes on which they depend" (Pope et al. 1975, p. 419). Nowhere do they demonstrate that such a view requires that what is an effect for one purpose cannot be a cause for another. Recognizing Durkheim's functionalism (Pope 1973, pp. 411–12: Pope et al. 1975, p. 418), they fail to take the further step of seeing the individual and society as mutually interpenetrating, even though Durkheim could see society as existing in the minds of its members (Pope 1973, pp. 409–10). The rhetorical force of the critics' claim that Durkheim was not and cannot be interpreted as an action theorist rests heavily on their apparent view that seeing psychic states as effects precludes their being seen subsequently or simultaneously as causes. The viewpoint represented by Berger and Luckmann (1967), itself influenced strongly by Durkheim (and not the Durkheim of Parsons [see Berger and Luckmann 1967, pp. 17–18]), that

17 Another way of saying this is that Pope apparently interprets Durkheim's individual-social dualism as a concrete one, whereas Parsons sees Durkheim moving toward an analytical individual-social dualism (pp. 327–28n., 337, 350–59, and 377n.). Part of Parsons's case that Durkheim moved in the direction of action theory, in fact, is drawn from his interpretation of Durkheim's dualism as increasingly analytical. What is "individual," then, is more a characterization of tendency than a concrete location. For Parsons, then, Durkheim's dualism does not preclude his being an action theorist.

"society is a human product, man is a social product, and society is an objective reality" (1967, p. 61) seems not to have occurred to the critics as a heuristic device for the interpretation of Durkheim.

Third, and by extension, the critics' claim that, since Durkheim viewed internalization of norms and religious ideas as socially derived, Durkheim cannot be viewed as an action theorist (Pope 1975, p. 113) itself rests upon the individual-social dualism. It is either a thoroughgoing individual-social dualism or a peculiar reading of action theory that would require us to take the view that internalization denies action.

Fourth, and perhaps most significant from the point of view of the interpretation of Durkheim, the critics' individual-social dualism and their opting to see Durkheim's contributions as limited to half of the dualism flatten his social psychology. Maintaining that Durkheim is throughout a social realist and that consequently (a non sequitur) psychic events are always effects and never causes, Pope's critique of Parsons fails to explicate the social-psychological basis of Durkheim's distinction between egoism and anomie. Though the interpretation of these concepts is a vexed matter (see Johnson 1965), Pope has effectively shown that the distinction is not to be found by way of Parsons's claim that egoism is a matter of the content of values. Although Pope (1973, pp. 406, 408) does present brief and compelling statements of Durkheim's view of man, his framework requires him to slice off one side of Durkheim's analysis; and he is left with only the (correct) statement that each concept is a matter of weak social integration, and the two concepts become "basically similar" (Pope 1973, p. 405). That one form of social malintegration fails to satisfy Durkheim's individual's need for social bonds and the other his need for limiting rules, an interpretation strongly grounded in Durkheim's corpus (Warner 1976, pp. 72–77), would require looking at the individual as a factor in sociological explanation. But this is what Pope's truncated Durkheim refuses to do because of Pope's insistence that such needs are socially generated. But it is only a dualism that requires socially generated needs to be incompatible with an action orientation.

To this point, then, it seems likely that Parsons's original overstatement of his case has led his critics to overstate theirs and consequently to abandon those insights regarding Durkheim's use of subjective categories of explanation that Parsons (his framework notwithstanding) generated. The baby has gone out with the bath water.

Their dualistic orientation toward the individual and society further leads the critics to distort Weber and, consequently, to exaggerate his differences with Durkheim regarding action theory. The critics rightly attribute the label of "methodological individualist" to Weber, but, in contrasting him to Durkheim, they make the implicit statement that Weber was for purposes of sociological explanation more interested in concrete indi-

viduals than was Durkheim. They juxtapose (Pope et al. 1975, pp. 418–19) Weber's individualistic methodological pronouncements to Durkheim's insistence that subjective states are too inaccessible, too capricious, and too various to be useful for scientific purposes. There are important (especially philosophical and ethical) differences between Weber and Durkheim on this score, and immense differences in emphasis (Warner 1976, pp. 96–99), but Weber's theory depended no more than Durkheim's on evidence of the psychic states of concrete individuals. Weber's "individual" was a construct, a "homunculus" (Schutz 1963), an extension of, but as abstracted as, the economists' "rational man." Surely Weber and Durkheim differed mightily on the role assigned to *Verstehen* in the theoretical construction of courses of action; nonetheless, each employed models of the actor in their attempts to render social phenomena understandable.

Finally, though the critics rightly emphasize the predominance of conflict in Weber's view of the world in contrast to Durkheim's emphasis on consensus, their opting for Weber as methodological partisan of the individual and Durkheim as partisan of society impels them to deny that shared cognitive ideas are often adduced in Weber's analysis of religion (Pope et al. 1975, pp. 423–24). The critics apparently feel that because Weber stressed that the carriers of particular religious conceptions are (socially partial) groups, he must also stress cognitive dissension in the "typical" situation. Yet Weber spent years of his life showing (whether he was right or wrong is not the issue) that the Brahmins' concepts of transmigration and ethical compensation (1917, pp 118–23, 326–28), the small-stock breeders' idea of a war god with whom the people made a covenant (1919, pp. 75–81, 118–46, 295–96), and the Calvinists' idea of predestination (1905, pp. 232–33n.) had captured the imaginations of whole continents (Warner 1972, pp. 79, 115–20, 175–201). Surely Weber differed with Durkheim here too, because the latter (as both Parsons and his critics state) never saw religious ideas as the results of social struggle. But there is no point in overplaying their differences to deny that each perceived high levels of cognitive consensus in the social worlds they analyzed.

The foregoing remarks are offered tentatively to suggest one explanation of the impasse at which Parsons and his critics have arrived. The polarization of positions may be due, in part, to a polarization in frameworks, as Parsons's tendentious action theory confronts his opponents' implicit dualism. Each side has made notable contributions to our understanding of Durkheim and Weber, but each also enters distortions, and neither side can appreciate the contributions of the other.

Norms versus interests.—A second dualism in the critics' framework is perhaps more consequential, if only because it is identical with one in Parsons's framework. As explicated above, it involves the doubtfully logical

views that, because "the problem of social order" is construed to mean "How do unsocial individuals come to form a society?" and the answer to be "Because they are constrained by norms," norms and individual interests as categories of action are logically opposed and, by extension, the sole socially conditioned intrapsychic variables relevant to the sociological analysis of action are normative ones.

The problem is most serious when the interpretation of Weber's theory is at issue. The critics rightly note (Cohen et al. 1975*a*, p. 236) that Weber's notions of human motivation "emphasize habit and self-interest," but in thus correcting for Parsons's overstatement of the importance of normative motivation in Weber they themselves systematically understate the importance for Weber of (cognitive) orientation to norms. In responding to them, Parsons implies (1976*b*, p. 668) that they exhibit a tendency to think primarily in terms of "material interests," an implication that they reject (Cohen et al. 1975*b*, p. 673), maintaining that "the Weberian concept of interest is inclusive and subsumes both material and ideal interests." When they go on, however, to contrast the "importance" of "norms" versus "interests" (1975*b*, p. 673), the interpretative problem becomes clear. In fact, for Weber, norms serve as points of orientation, conformity to which is motivated by various factors, among them being expediency, habit, and obligation. As Parsons notes (1975*b*, pp. 667, 669), questions of structure and questions of motivation ought to be kept separate, and yet he further compounds the problem (1975*b*, pp. 667, 668) by adducing "moral obligation" in Weber and by implying that the "motivational implementation problem" requires reference to the normative element. Both Parsons and his critics, then, are caught within a framework in which norms and interests are opposed elements in action. In the words of the critics, "a category-by-category analysis [of Weber's action concepts] shows that traditional behavior, usages and customs are primarily habitual, while instrumentally rational behavior and complexes of interests are largely oriented to expediency *rather than* to norms" (Cohen et al. 1975*a*, p. 231, emphasis added).

The same dualism becomes apparent when the critics refer to the "characteristically (though not exclusively) Weberian" view that "ideas and interests are interdependent" and operate in "mutual interaction and persistent tension" (Cohen et al. 1975*a*, p. 236). This summary of Weber's position requires a dualistic conception, a conception that Weber did not subscribe to (and in fact labored to overcome) but which both Parsons and his critics implicity assume, though each maintains the opposite side of the duality. The notion that ideas and interests are in tension implies that each operates on the level of motivation. However, Weber most often speaks of ideas as conditions under which interests (which is the motivating force) are pursued; ideas and interests are not opposed categories.

There is room in the interpretation of phenomena and of Weber for many differences over the role of interests (though I tend to agree with the critics that the category of interests was of immense significance for Weber [Warner 1972, pp. 87–201]); but, no matter how empirically powerful interests may be, emphasis on them should not obscure Weber's recognition of such social structural matters as norms and ideas.[18]

In fact, Weber's position on this score was not far from Durkheim's: the structural significance of acts cannot be inferred from their motivation. Durkheim's rejection (1897, p. 43) of the idea that an act can be defined by the end sought by the actor[19] is quite parallel to Weber's insistence (1922, pp. 314–15) that the "psychological facts" of "predominantly utilitarian or ethical or subjectively conventional" motivations for adherence to law are "irrelevant" to the use of the sociological concept of law.

Finally, the critics do not acknowledge that Parsons's dualistic framework leads him to see the Durkheim of *The Division of Labor* as a "positivist," because in that work Durkheim regards norms as conditions rather than as ends of action. This depiction of Durkheim, as we have seen above, depends on Parsons's view that Durkheim at this stage regarded the actor as a "scientist" whose orientation was not therefore a variable. Pope rejects (1973, pp. 400–401) Parsons's interpretation of Durkheim as a positivist, but he fails to scrutinize the grounds for Parsons's claim and so himself fails to explicate the cognitive orientation to norms that Durkheim emphasized at this stage of his career. Caught within Parsons's framework while attempting to deny his conclusions, Pope also obscures Durkheim.

Summary.—The interpretation and use of Durkheim's and Weber's theories will continue to be inhibited unless we step outside of the framework that has generated so much misunderstanding. If the interrelated sets of dualisms of Parsons's and his critics' frameworks were to be abandoned, it could be seen that much of the work of Durkheim partakes of both social realism and an action orientation and that of Weber stresses the operation of both norms and interests.

[18] In my own reading of Weber, it is clear that, at the level of motivation, interests (as defined in n. 5, above) are more powerful than norms. That is not, however, the same as saying that interests are more important than norms. In fact, in Weber's world view, "interests" comes close to being an ever-present and multifaceted concept whose theoretical invocation is nearly constant. The variables of normative and cognitive structures are adduced to explain divergent comparative/historical outcomes.

[19] Pope (1973, p. 408) cites this passage as representing Durkheim's rejection of the means-ends action schema. Yet the passage in fact speaks to the issue of how we go about defining phenomena for sociological analysis (e.g., "What is a suicide?") and is thus formally parallel to Weber's methodological point to be mentioned presently. Durkheim's remark occurs, in fact, in precisely that work (*Suicide*) in which, as Pope acknowledges, Durkheim employs a means-ends schema.

DURKHEIM AND WEBER IN LIGHT OF THE REDEFINED THEORY OF ACTION

I have attempted to show that, although both Parsons and his critics present scholarly and well-documented analyses of aspects of Durkheim's and Weber's theories, they are forced into polarized positions because of untenable frameworks. Despite these polarized conclusions, however, each presents discrete theoretical elements that are compatible with one another. In this final section, I will present several course-of-action statements drawn from Durkheim and Weber in order to demonstrate that when the cognitive element is restored to its rightful analytical place (which is possible only when the individual-society dualism, in its various guises, is abandoned), aspects of Durkheim's and Weber's analyses are, in certain respects, very similar. I will not claim that their theories "converge," both because any such argument would require greater space than is available here and because there remain significant Durkheim-Weber differences that ought not to be obscured. Complementarity and continuity, not identity, is my agenda.

A redefinition of action theory.—To that end, an explication of the framework implicit in the preceding discussion is required. Like Parsons's framework, this revision includes norms and a normative orientation but decouples them so that "norms" are defined as behavioral prescriptions and proscriptions that are endowed by the social environment with an "oughtness" and are enforceable by informal and formal sanctions, but where the extent to which that oughtness is experienced and affirmed by the actor is an empirical issue, not to be decided a priori. The actor's normative orientation, then, will be that part of the actor's orientation that is governed by the sense of an ought. Among the actor's cognitive orientations will be those matters that are experienced by the actor as "is," whether these be prosaic assessments of the situation, metaempirical conceptions of a divinity, or even what to others (but not necessarily to the actor) are matters of ought, including norms. Paraphrasing some significant cognitive orientations imputed by Durkheim and Weber to various social actors, we have the following two "situational" and two metaempirical statements: "Divorce is impossible" (Durkheim 1897, pp. 270–76); "Dueling is illegal" (Weber 1922, p. 32); "The totem is extraordinary" (Durkheim 1912, pp. 150–56); and "All men are predestined to eternal salvation or damnation" (Weber 1905, pp. 100–105). Matters of "knowledge" and "belief" are alike socially structured and socially variable.

Under the "redefined theory of action" (r.t.a.), then, the action framework has the following elements: an *actor* (who expends effort); the *end* of his action (which is constituted in part by his normative orientation, in part by his cognitive orientation, and in part by exogenous elements); and a *situation*, combined of (*a*) *conditions*, those aspects of the situation over

which the actor experiences no sense of control (including, in some cases, norms), and (*b*) *means*, those aspects of the situation subject to the actor's manipulation;—all of this occurring within the framework of *normative* and *cognitive* structuring, which, in varying ways, partly determines the *ends* of action, sets constraints on the *means* of action, and structures a picture of the *situation* of action. Just as, for Parsons, the possibility of social order rested upon the sharing of normative orientations in common (the nucleus of the "value consensus" idea), so also in the redefinition, to the extent that we are still concerned with a problem of order, we shall be interested in the degree to which cognitive as well as normative orientations are held in common (cf. Scheff 1967). The r.t.a. incorporates all of the elements that Parsons's framework and his exegeses of Durkheim and Weber took to be operative, but it has the advantage of not prejudging either empirical reality or the theorists who have addressed that reality, for the r.t.a. explicitly rejects the notion that "action" can only be an effort to conform to norms (cf. pp. 76–77).

Action cognitively oriented to norms.—Whether individuals are "naturally" disposed to be cooperative or antisocial, rules have a function in facilitating interaction. In the words of Lon Fuller (1971, p. 196): ". . . consider the law [of] . . . contract, agency, marriage and divorce, property . . . and the rules of court procedure. All of these vital branches of the law . . . facilitate human interaction as traffic is facilitated by the laying out of roads and the installation of direction signs. To say that these branches of law would be unnecessary if men were more disposed to act morally is like saying that language could be dispensed with if only men were intelligent enough to communicate without it." The point here is that Parsons's concentration on the theoretical correctness of the Hobbesian problem (in either of its renderings) has obscured for both him and his critics the role that norms play in facilitating interaction. Durkheim's and Weber's perceptions of the role of law (and other norms) are informed, however, in part by such a cognitive perspective.

In *The Division of Labor*, Durkheim derives a statement of the necessity of law in a differentiated society. With the advent of the division of labor, individuals engage in multiple exchanges with each other, interchanges that, because of their very variety and multiplicity, would demand an enormous amount of decision-making time were it not for the consensually accepted institutions of contract law. "If, then, it were necessary each time to begin the struggles anew, to again go through the conferences necessary to establish firmly all the conditions of agreement for the present and the future, we would be put to rout" (1893, pp. 213–14). Earlier in the same passage, Durkheim refers to travelers who must make arrangements for lodging. A rule that states "Pay in advance for a night's lodging" may be observed for any variety of motivations, yet knowledge of such a

rule is socially distributed (not a factor Durkheim emphasized [Cartwright and Schwartz 1973]) and its appearance is socially determined (which he did emphasize). Contract law is then a variable which is to be taken into account in the understanding of action. That Durkheim did not, at this stage of his career, adduce moral obligation as a motivation for compliance is the basis on which Parsons calls him a positivist; yet Parsons's action framework does not demand that moral commitment be socially distributed, only that it be socially structured. That is, the sole ground Parsons has for characterizing this type of analysis as positivistic is the cognitive rather than normative orientation that Durkheim imputes to the actor.

Weber's approach to law, though more elaborate, is similar. He distinguishes custom, convention, and law on structural (not motivational) grounds. Customs are "factual regularities" that are not enforced by sanctions (1922, pp. 332, 319). Conventions are social norms that are supported by the "spontaneous response" of the actor's environment (1922, p. 320), what we would call today informal sanctions. Conventions are no less powerful for their relative informality, and Weber gives the striking example of the "British constitution" as an example of a "conventional order." Law, finally, is distinguished by the fact that a potentially coercive apparatus is specifically held ready for enforcement whether that apparatus be governmental or not. Motivations for conformity are irrelevant to these distinctions, and Weber holds that the three types of rule are on a continuum "with imperceptible transitions leading from one to the other" (1922, p. 319). In particular, "from the sociological point of view, legal order and conventional order do not thus constitute any basic contrast, since, quite apart from obvious cases of transition, convention, too, is sustained by psychological as well as (at least indirectly) physical coercion. It is only with regard to the sociological structure of such coercion that they differ" (1922, p. 326). What Weber has to say about the actor's orientation to laws, and their consequent efficacy, thus applies largely to informal norms (conventions) as well. And his analysis of law centers on the factors of consensus and predictability: "In the case of certain events occurring there is general agreement that certain organs of the community can be expected to go into official action, and the very *expectation* of such action is apt to induce conformity with the commands derived from the *generally accepted interpretation* of that legal norm . . ." (1922, p. 314, emphasis added). Although, by definition, "law" requires an official enforcing staff, its implications for action depend upon the predictability of its enforcement and its consequent empirical validity. For these purposes, conventions (norms) are often as effective, since the operation of their nonviolent sanctions (e.g., threats of boycott, ostracism, or spiritual sanction) are often more predictable than those of the law (1922, p. 316). The empirical validity of a norm affects the interests of the individual as, among

other things, a determinant of the conditions of action: "In particular, it may convey to an individual certain *calculable chances* of having economic goods available or of acquiring them under certain conditions in the future. [These chances are] guaranteed to him by the *consensually accepted interpretation* of a legal norm" [or, by extension, of a conventional norm] (1922, p. 315, emphasis added).

In contrast to Parsons's interpretation, then, Weber's view does not highlight, though it certainly acknowledges cases of, the actor's sentiment of moral obligation. As the critics (Cohen et al. 1975*a*) rightly point out, Weber's actor is motivated by expediency as much as morality. But in contrast to the critics' interpretation, Weber does not thereby demean norms as a factor in sociological explanation. Not only is the empirical validity of a norm much enhanced by there being some actor or actors supporting it on moral grounds, but also (regardless of the actors' motivation) Weber stressed the cognitive orientation to norms and their cognitively consensual character. Parsons was therefore quite right to draw attention to the importance of norms for Weber. Moreover, in this respect, Weber is similar to the Durkheim of *The Division of Labor.*

The cognitive construction of the ends of action.—The foregoing examples of rule-oriented action schemas may appear (especially to those who accept Parsons's critique of positivism and utilitarianism) as merely positivistic or utilitarian. However, in the case of law and other social norms, the incidence of and knowledge of their existence are structured variables, both intra- and intersocietally. Nonetheless, those who wish to avoid the "positivist trap" may wish to see more thorough, less apparently calculative, examples of structured action. Contrary to Parsons's implication that (from the sociological point of view) ends are structured only either randomly or normatively, there is to be found in both Durkheim and Weber the view that the cognitive element structures not only conditions but also ends. Weber's paradigmatic statement to this effect occurs at the end of a passage that both Parsons (1975*b*, p. 668; and see paraphrases in 1937, pp. 533–38, 668) and his critics (Cohen et al. 1975*a*, p. 236) cite as strengthening their respective positions. The passage (with some comments interlarded) reads as follows:

> Not ideas, but material and ideal interests, directly govern men's conduct. [This observation is emphasized by the critics.] Yet very frequently the "world images" that have been created by "ideas" have, like switchmen, determined the tracks along which action has been pushed by the dynamic of interest. [The sentence is cited by the critics in support of their contention that Weber saw ideas and interests in "tension," and Parsons cites it as a statement of the "canalizing" role of ideas. But Weber goes on:] "From what" and "for what" one wished to be redeemed and, let us not forget, could be "redeemed" depended upon one's image of the world. [1915, p. 280] [Here, in addition to interests and their vectoring, we see Weber speaking of the cognitive construction of ends.]

As I have demonstrated above, Parsons clearly recognized the cognitive aspect in Weber's thinking in this respect, but his framework necessitated that he construe it as part of the normative complex. His critics reject this conclusion, but with it they (Cohen et al. 1975*a*, pp. 234–35) also reject the clear statement that for Weber this process occurred frequently.

Weber's paradigm for the role of ideas informs the entire range of his studies on the economic ethics of the world religions (Warner 1970*b*, pp. 81–87), but here the example of *The Protestant Ethic and the Spirit of Capitalism* will suffice. Taking the content of Calvin's doctrine seriously but not as the last word, Weber showed that its predestinarianism was psychologically intolerable. Calvin's view that good works could not influence salvation chances was "turned on its head" to read that the "saved" will be the doers of good works. Weber's Calvinist believer was interested in his salvation. He wanted the feeling "here and now" that he was saved. Through an understandable twist of Calvin's doctrine, the believer's salvation was cognitively indexed by his worldly success. "In practice, this means that God helps those who help themselves" (1905, p. 115). Thus, ideas and interests constructed a powerful motivation for systematic work. The motivation for worldly success is thus, for Weber, constituted from an interpretative, cognitive process.

A very similar kind of reasoning is seen in Durkheim's analysis of anomie. Durkheim used statistics on marriage and the family in his treatments of both egoistic and anomic suicide. Marriage (for men) and children (for women) have an inhibiting effect on egoistic suicide because they form a conjugal or domestic society that provides individuals with mutual moral support. But Durkheim uses statistics on the rate of divorce in his analysis of anomic suicide. Finding that (for men) a high rate of divorce was conducive to suicide, Durkheim did not simply maintain that the voluntary ending of a marriage was disruptive or even that marriage provides a restraint on primitive passions. In fact, he constructed an argument (1897, pp. 246–54, 270–76) based on the assumption that men (human males, that is) and animals were in principle different. They differ, moreover, not only in basic intelligence or reasoning power, in the manner of Hobbes. Rather, Durkheim held that men's desires could not only be restrained by social norms but could also be stimulated by the weakness of those norms. Human males, he said, are endowed with imaginations (a cognitive factor), which could, in fact, increase the level of desire. The institution of marriage, then, serves not only to delineate appropriate avenues for the gratification of given desires (parallel to Weber's "interests") but also can reduce those desires themselves through its control over the mind. Long-endured poverty, Durkheim held, also has the same effect of structuring the individual's ends. Parsons correctly sees in this analysis the structuring of ends (pp. 334–38), but he downplays (pp. 387–

90) its cognitive aspect so that he can see it as an adumbration of Durkheim's alleged later, more fully developed, voluntaristic theory of action, with its emphasis on an attitude of "moral respect" for the rules. Yet the action frame of reference, as redefined, is already there, in full form.

The foregoing examples of action schemes drawn from Durkheim and Weber represent the operation of the analytically independent element of cognition. They are reducible neither to scientific truth or falsehood nor to normative conceptions of an ideal state of affairs. Any such reduction would have to ignore two general cognitive factors that both Durkheim and Weber took into account. First is that the cognitive element structures the situation. Rather than assuming that there is a preexistent situation about which actors can have knowledge that is simply either true or false, the explanatory work of Durkheim and Weber takes the view that actors' orientations, especially when held in common, bring cognitive order into a setting which itself is chaotic. This, of course, is a dominant theme in Durkheim's *Elementary Forms* and in Weber's "Science as a Vocation" (1918). Second, for both Durkheim and Weber, the cognitive element could itself be a motivation. The desire, that is, for a cognitively secure world is one of the wellsprings of human action. For Durkheim's man, the security so desired concerns largely the realm of social encounters, whereas Weber is more likely to attribute anxiety in the face of meaninglessness to the actor facing the cosmos. These differences between the philosophic anthropologies of the two theorists—the difference between the agonies of "anomie" and "senselessness," for example—are important and should not be submerged in a general thesis of the two theorists' "convergence." Yet, the cognitive element in the two is formally parallel.

CONCLUSION

There may be no necessary logical link between theories of order, on the one hand, and theories of action, on the other. Yet from the foregoing analysis, it seems clear that Parsons's concentration on the "Hobbesian problem of order," especially in the manner in which he initially misconstrued it, has led him to neglect the cognitive element in action. His critics do not challenge his focus, and thus they merely reject his conclusions on his own terms and themselves obscure the action element in Durkheim's theory and the powerful role of norms in Weber's. There is, however, a second "problem of order" that does not assume the initial structuring of antisocial individuals who must be constrained by morality. That second problem of order is the Simmelian, and it is to those who (implicitly or explicitly) follow in Simmel's tradition that we owe an understanding of the operation of social norms that extends the cognitive insights provided by Durkheim and Weber. The cognitivists have shown that the assumption of norma-

ive consensus (on such desirable states as "peace" and "propriety") by
no means solves the problem of social order, because there remains the
question of how it is that actors can come to concert their action in common. Schelling (1963, pp. 53–118) has shown that norms and other socially
defined symbols can serve as points of common orientation for actors in
states of uncertainty, whether or not such norms recommend themselves
by "logic" or "fairness." Goffman (1967, pp. 27–28, 44) has argued that
actors attempt to resolve incipient threats to a commonly accepted social
order without regard for sentiments of justice. Suttles (1968, p. 233) interprets the subculture of the slum as "more nearly a means of gradually
discovering a moral order than a set of rules which one mechanically
obeys." "Thus, cognitive rather than moral considerations dominate the
residents' interests" (Suttles 1968, p. 231). Contrary to Parsons's framework (see pp. 380–81, 385–86), then, it is possible for a sociological analyst to take account of the role of norms in action without assuming that
they must be either experienced as morally obligatory or merely calculated
against as aspects of a given, scientifically knowable, situation. The redefined theory of action (which, it should be clear, has long been in use)
thus can help rescue from the oblivion into which Parsons's overinterpretation has threatened to throw them Durkheim's and Weber's insights into
the operation of norms and ideas in social life.

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Comment on R. Stephen Warner's "Toward a Redefinition of Action Theory: Paying the Cognitive Element Its Due"

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The editor has asked me to comment on Professor Warner's very interesting article. This request has come at a moment of rather stringent time bind so that, in order to meet his deadline, I must restrict myself to a few rather specific issues instead of attempting to survey a broader range.

I very much appreciate the seriousness and competence with which Warner attacks the issues in the controversy between myself and the group he calls the "critics" of my work (Cohen, Hazelrigg, and Pope in their recent series in the *American Sociological Review*, which is referred to by him so frequently that further references in this comment would be "bibliographical supererogation"). Warner has, in my opinion, elevated the discussion to a much more general level, transcending the earlier issue of which party was "right" in the interpretation of the works of Durkheim and Weber. To a degree which I prefer not to try to explicate in detail here and would qualify carefully if I did, I concur with Warner's position that "both were right."

I should like to center this comment on one major issue: Warner's allegation that I have seriously neglected the "cognitive element" and that, as his subtitle suggests, he proposes to contribute to restoring it to its rightful place. Even on this issue, it will not be possible to enter into the many complexities of the problems, but only to sketch an outline of connected considerations.

Warner suggests that there is a great deal of common ground between myself and my critics. In one respect, however, I would like to put Warner and the critics together in contrast to my own present self-interpretation. This lies in the extent to which all, in discussing "Parsons's theory," focus their attention on one book, *The Structure of Social Action*, which was my first book, published 41 years ago and not revised in the meantime. It may well be construed to be "unfair to organized interpreters" for the author of such a work to be alive and "in circulation" so long after its publication.

However that may be, my complaint at the present juncture is that both the "critics" and Warner concentrate so heavily on this one work that they tend to neglect almost everything I have published in the intervening period. Hence I propose to present a brief outline of developments, in my

own theoretical concerns, of considerations relevant to this theme. In evaluating my account it is essential to keep in mind that I—like Warner—was a "child of my age" and, in working on *The Structure of Social Action* in the 1930s, thought in terms of the intellectual environment of the time, when, for example, Durkheim's and Weber's work had not yet permeated the American cultural milieu.

As I tried to make clear in my controversy with Pope et al., however, despite the ignorance of social scientists in the English-speaking world at that time concerning the work of Durkheim and Weber, my intention in writing that book was not to introduce those thinkers to the Anglo-American professional public. It was, rather, to analyze their work, as well as that of Marshall and Pareto, in terms of its bearing on a particular theoretical problem complex. This concerned the relations between economic theory and what could at the time be called sociological theory—about the nature of which I was not very clear at the start of the enterprise. The importance of this problem complex for my enterprise, including the treatment of Durkheim and Weber, was totally ignored by Pope et al., and is certainly not stressed by Warner.

My intention meant that my concern with cognitive problems—apart from those involved in my own analytical methods, which were formidable—focused on the problem of the involvement of cognitive functions and processes in economic action, especially as formulated in the conception of economic rationality. Consideration of this in turn led me to an intensive study of utilitarianism because of the importance of that movement as the intellectual matrix, as it were, within which the classical economics, from Adam Smith on, grew in England.[1] This path, greatly illuminated for me by Halévy, led to the Hobbes-Locke dilemma and the Hobbesian version of the problem of order in society. Among my subject-authors, if one may call them such, the one most directly concerned with this problem was clearly Durkheim, who was also the one to make explicit reference to Hobbes in *The Division of Labor* (Durkheim [1893] 1933, bk. 1, chap. 7).

I was particularly concerned with the tendency of many economists of that time—the problem has not yet evaporated—to hold that a system of competitive markets could be "nearly" self-regulating and that such a market system could be considered to be a prototype of a social action system. I used the word "nearly" in the preceding sentence because the "laissez-faire" economists regularly paid a somewhat grudging respect to

[1] In this I was immensely helped by Élie Halévy's *La Formation du radicalisme philosophique*, which at that time had not yet been translated into English. Very recently I have become indebted, in the same broad area, to another French source, the book of Louis Dumont the English title of which is *From Mandeville to Marx: The Genesis and Triumph of Economic Ideology*, which in a new way has helped greatly to clarify various problems Warner raises.

the necessity of a legal system involving property and contract. It was, however, Durkheim's treatment of this problem which first strongly attracted me to his work.

To me the paradigmatic example of the laissez-faire economists' position was the book by Lionel Robbins (now Lord Robbins) entitled *The Nature and Significance of Economics.*[2] My view came to be that the kind of "economic individualism" which Robbins represented far transcended the limits of economics as a theoretical discipline and ran straight into the Hobbesian problem of order. To account for such order as indeed had existed it was necessary to invoke factors quite other than the play of interests in competitive markets as treated by this order of economic theorists.

Whether, as Warner asserts, I wrongly confused the problem of the "randomness of ends" with the factors in the genesis of Hobbes's war of all against all seems to me a moot point which could be resolved only by an investigation of intellectual history which cannot be undertaken here. In the development of *The Structure of Social Action*, however, the relevance of the randomness problem clearly grew out of the economic theories of the Robbins type. This is to say that such economists drew the limits of their discipline in one direction by insisting that the "wants" of economically acting individuals should be treated as *given* and thus were not a proper subject for economists' investigation. I think I am correct that, in the logic of theory, the assumption of givenness, in the absence of the introduction of specifically structured parameters, is tantamount to the assumption of randomness, as the most parsimonious assumption. The famous assertion of Bentham that "push-pin is as good as poetry," if someone thinks so, is the classical statement of this position. It seemed to me then, and still does, that this is a reasonable interpretation of the utilitarian position, including Hobbes as a utilitarian. Of course I was and am fully aware that at another level Hobbes took account of certain nonrandom ends of individuals in his state of nature, notably the need for security and that for power—again in Hobbes's specific sense.

One more point needs to be made in this broad context. Warner is quite correct that, in the context we have been discussing, I insisted that some sort of normative consensus must be taken into account as one essential factor in the explanation of such social order as exists and that therefore Hobbes's invocation of the power of the sovereign is an insufficient explanation. I would like, however, sharply to repudiate Warner's imputation to me of the view that normative, especially moral, consensus is the *only* important factor in social order and therefore under no circumstances do "interests" contribute. Particularly in recent years I have made a good

[2] I wrote a review article (Parsons 1934) about this book well before the publication of *The Structure of Social Action* (1938).

many statements about the process of institutionalization as, in the nature of the case, a complex process, statements invoking a wide variety of combinations of normative and nonnormative factors. His imputation to me of a radical dissociation of the factors of normative, especially moral, commitments and of interests, and treating them as always and necessarily in conflict is, from my own point of view, simply incorrect. Having been socialized with one foot in economics—of a non-Marxian variety—I have always presumed that important social phenomena have multifactor, not single-factor explanations.

The above background leads to the relevance of what Warner calls the "cognitive element" in action. As I have said, at the time in question, my substantive concern was primarily with its relation to economic action. Weber certainly included it in his conception of *Zweckrationalität*, though this was broader than economic. The same was true of Pareto's category of "logical action." Pareto in particular emphasized the standards of science as the criterion of whether action could be treated as logical in his sense, and this seemed quite compatible with Weber's usage, at least in this context. Then there was a similar "definition of the situation" in Durkheim's early treatment of the category of social facts, especially in the *Rules*. These views had in common either explicitly treating the actor as if he were a "scientist" as Pareto and Durkheim did, or treating his action in the requisite respects as compatible with this image, as Weber did.

I find it difficult to understand how Warner came to impute to me the view that such an actor was "purely passive" in relation to the facts of his situation. Surely I never suggested that the producer, as treated by economic theory, was purely passive, even though he could be considered to be rational in a sense about which there was a good deal of theoretical consensus. This indeed is a conception which has become even more dominant among economic theorists than it was at the time when I wrote. Such a claim about economic action would indeed be grossly incompatible with the importance of the means-end schema which I made a centerpiece of my study. This interpretation, it seems to me, would make me into just the kind of positivist—mainly of the behavioristic type—against which I was vigorously polemicizing.

Warner of course is right that in epistemological contexts I was already then more Kantian than anything else, and definitely not a Humean empiricist—my use of Whitehead's work should make that quite clear. I have in recent years been strongly reinforced in my Kantian perspective, as forthcoming publications mentioned below should make clear.[3]

[3] My Kantian "epistemological activism," to use Warner's phrase, applies not only to the objective side of the subject-object relationship. This can be seen in my recently published interchanges with Alfred Schütz (Schütz and Parsons 1977), which took place in 1940–41. In his previously unpublished review article about *The Structure of Social Action*, the

It is at this point that I become particularly frustrated by Warner's concentration on *The Structure of Social Action* and his neglect of subsequent developments which my attempt to clarify the issues forces me to outline. First, immediately following completion of *The Structure of Social Action* I embarked on a study of medical practice as an example of the professions which did not fit the main economic paradigm but were still definitely concerned with rationality. This led immediately and deeply into the problems of the role of scientific knowledge in rational action. It was in this connection that the beginnings of the "pattern-variable" scheme, which has attracted a good deal of attention, were worked out. Surely, like the businessman, the physician was not to be, nor was by me, regarded as a "purely passive" consumer of facts about the world in which he acted, namely, the pathologies presented by his patients. I will not take space to elaborate on the many complications of the relations between knowledge and practical action which are involved in medicine.

The pattern-variable scheme led, by way of the "four-function paradigm," which by the way Warner also does not mention, to a very general theoretical reorganization, which certainly involved the cognitive element. This development in turn led to a far-reaching reorganization of my theoretical treatment of the relations between the economic sector of social action and the rest, which was published in 1956 in *Economy and Society*, coauthored with Neil J. Smelser. (In choosing our title we wished to avoid disrespect to Weber—see the preface and dedication page.)

This theoretical reorganization of the treatment of the economic theory problem, by focusing on the economy as a primary functional subsystem of a society, and its interchanges with the other subsystems, greatly clarified the original formulations and certainly did not neglect or play down the role of rationality and thus the cognitive element. Indeed I should claim that it greatly strengthened and broadened the theoretical base on which at least the concept of economically rational action rested in the more general system of action. This involved above all the treatment of money as a generalized symbolic medium of interchange which could then be systematically related to three others, namely, political power, influence, and value commitments, all of which operated at the level of the society as an analytically defined system of action. (See the three papers on these media, chaps. 14–16 of my volume of essays, *Politics and Social Structure* [1969].)

Indeed, all four of these societal media can be said to constitute the institutional embodiment of modes of patterning of the rationality of action.

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first point Schütz takes up is his objection to my definition of fact as a "statement . . . in terms of a conceptual scheme." I had adapted this from L. J. Henderson, but it was basically Kantian. Schütz objected to both the word "statement" and the reference to the conceptual scheme. For him "facts" were the *direct* experience of the subject. On this point I have not changed my position.

This concept of course included economic rationality but went well beyond it. To me *any* concept of rationality involves the cognitive element in a central place. But this way of posing the problem of defining the precise nature of its centrality was much more general than that permitted by the approach through economic rationality alone. Once posed at this level the problem could not be stated in a form which ignored the relations among—to put it somewhat loosely—economic, political, legal, and moral rationality. The last category clearly brings us to Weber's second category, *Wertrationalität*, which Warner brings up at the end of his paper, but of which I shall defer discussion just now.[4]

I have noted that the main path which has led me from the problems of the status of economic theory and along with it economic rationality into broader fields has been the study of the professions, in the first instance that of medical practice. This, by way of the prominence of "scientific medicine," naturally prominently involved the cognitive element. To make a long story short, it was not a very profound observation that in the modern world not only medical practice but essentially all the other occupational groups which are called professions have become intimately connected with the universities. The essential point is that there has been a rapidly crystallizing institutionalization involving first the requirement that professional status should depend on training in university professional schools and second the somewhat later development that university professional schools, teaching hospitals, etc. should constitute the main centers of research in the fields of professional action. Of course the university's concerns have by no means been confined to the applied professions, but have come to center on what I have called the "pure" intellectual disciplines, which, organizationally, have been located primarily in faculties of arts and sciences. Here there has been a very important "symbiosis" involving complex articulations among the graduate schools (which have come to be the training centers in the first instance for future members of the academic profession itself and the primary centers for the research activities of members of this profession), the professional schools, and the undergraduate colleges.

This is the context in which, as a sociologist, not a philosopher, I followed up my interest in what Warner calls the cognitive element to a level of generality well beyond that of the status of economic rationality as such. A rather long program of studies in the field of higher education has culmi-

[4] The pattern variable scheme was, as noted, the formal generator of the four-function paradigm (Parsons and Platt 1953, chaps. 2 and 3). It also underwent a major analytical development, documented in the 1960 article "Pattern Variables Revisited," which was reprinted as chap. 7 of *Sociological Theory and Modern Society* in 1967. This smoldering "bed of coals" has very recently burst into flame. The limitations of space of this comment, however, preclude entering into these problems here.

nated so far in *The American University*, coauthored with Gerald M. Platt (1973) which, curiously in view of his stress on the cognitive element, Warner does not mention.

The theoretical core of *The American University*—along with the technical appendix, written last—was chapter 2, entitled "the Cognitive Complex: Knowledge, Rationality, Learning, Competence, Intelligence." This constitutes, I think it is fair to say, one of the most theoretically comprehensive attempts in the social science literature to deal with the cognitive element. As the author (Platt served as an exceedingly important critic, as did Smelser, but I took responsibility for the chapter) I found it a very seriously difficult chapter to write. Indeed it went through three complete drafts. In no sense does it claim to be a "last word." I do think, however, that it can serve to refute an allegation of neglect of its subject and just possibly to have advanced the discussion a bit.

The most significant point to stress at this juncture is that this broadening and deepening of the approach to the role of the cognitive element in human action did not come about by a single jump from the level discussed in *The Structure of Social Action*—with its concentration on economic rationality and the rest of the analytical paraphernalia, especially the concern with utilitarianism—to a philosophical solution of how to "render it its due." It followed a much more laborious path, through analysis of the role of the professions and of political and other aspects of societal level rationality, to an attempt to tackle the analysis of the cognitive complex itself.

This approach, however, started at the societal level, concentrating on a social phenomenon, namely, the modern university as the center—in Shils's sense—of the modern system of higher education (and research). In the course of that quest, however, it became increasingly clear that the university could not be understood only or even primarily as, in the analytical sense, a social phenomenon. In the first instance it was as much cultural as social. On the more general theoretical level, however, this meant that its problems had to be seen in the perspective of what I and others have been calling the "general system of action," which includes cultural, social, personality, and behavioral primary subsystems. Thus, though economic action is in the first instance a social system category, a general clarification of the nature of the cognitive element involved in it could not bypass the cultural level.

In very oversimplified terms, this is where the published record stands. It is not, however, so far as I am concerned, the end of the story. On the basis of considerations which had been in active ferment since completion of *The American University*, and in active communication with a small group of colleagues—especially at the University of Pennsylvania—it has seemed both necessary and highly advantageous to carry the theoretical development outlined in this comment one major step farther.

This step consists in treating not only the social system, but, beyond that, the general system of action, as just referred to, as a subsystem of a still more comprehensive, humanly meaningful system, for which we have adopted the term "the human condition." In addition to the system of action we include in this condition the physicochemical world and the organic world, as these have come to be conceived in the modern scientific tradition, namely, that of the physical and the biological sciences. There is then a fourth subsystem of the human condition as system, which we have ventured to call the *telic* system. It will be evident that we order the relations of these four subsystems to each other in terms of the four-function paradigm.[5]

It seems to me that carrying the analytical enterprise to the level of the human condition—the reader will have to judge its usefulness for himself—has helped to clarify a problem which Warner poses in the latter part of his paper. This concerns the relation between the two principal categories of rationality in Weber's work, which he called *Zweckrationalität* and *Wertrationalität*.

In the frame of reference just sketched, the social system seems to have built into itself a kind of "watershed" in terms of directionality in the cybernetic hierarchy. This hierarchy, I am convinced, permeates at least all living systems. Within the social system the "downward" direction leads from the institutional framework, which is primarily normative, in a sense of which Warner is suspicious, and concerns rationality first in the political context then in the economic. Beyond that it leads to organic health areas, technological areas, etc. In the other, the cybernetically "upward," direction it leads toward the definition of and commitment to values—the morality aspect of the normative complex. Beyond that it leads into the problems of "meaning" in the special sense of Weber's discussions of the world religions. Indeed one of our principal reasons for distinguishing a telic system in the paradigm of the human condition is to give formal recognition to the necessity—theoretically defined—of making clear that there is a scope of rationalization in this area of human concern, as well as in the economic-technological area. Above all we insist that the two are not mutually reducible to each other. My suggestion is that, with respect to Weber, Warner's insight is sound but needs further theoretical clarification.

Finally, however, no matter how important the cognitive element may be, in any usable theory of action or of the human condition it cannot stand alone or in any simple sense "predominate." At the most "elementary" levels it must be complemented by what is variously called a "motivational"

[5] A first attempted statement of what we call a "paradigm of the human condition" is at this writing in press as the final chapter of a new volume of my collected essays (Parsons, in press). Although I am the author of this chapter, I am deeply indebted to the several others who have contributed to its development.

or an "affective" component. Moreover, various kinds of combination of the two are to be found at many levels of action organization. I think that what we call the "moral" level is one main type of such combination. I have stated above that I am resolutely opposed to single-factor explanations of phenomena in the world of human action. I suggest that one of the main sources of Weber's difficulties—theoretical even more than personal—lay in his attempts to operate overwhelmingly in terms of the rational aspects of human action, with the result of treating the nonrational residually.

Let us by all means "pay the cognitive element its due." The question I leave the reader with is, What is its due and how does one mobilize the means of payment? I confess I am not wholly satisfied with Warner's answers to these questions.

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On R. Stephen Warner's "Toward a Redefinition of Action Theory: Paying the Cognitive Element Its Due"[1]

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Stephen Warner's hope for a rapprochement between Professor Parsons's interpretation and ours of Weber and Durkheim rests on the ironic observation that abandonment of Parsons's framework would permit us to embrace many of Parsons's conclusions.

CLEARING DECKS: THE TWO DUALISMS

Having correctly noted that we assess the individual-versus-society dualism in Durkheim and having located both this dualism and a norms-versus-interests dualism in Parsons's writings on Weber and Durkheim, Warner asserts that our analysis is based on the use of both dualisms. However, our *analysis* of Durkheim should not be mistaken for reliance on his analytic views. In like manner, although our reinterpretations of Weber and Durkheim entailed, in part, meeting Parsons on his own interpretive grounds to confront his views directly, we did not subscribe to his framework. Warner structures his discussion of our work in terms of these two dualisms; however, since they are not ours (nowhere does Warner cite any passage in which we endorse either), the distortion thereby introduced originates with Warner, not with us. Recognition of this fact destroys Warner's own framework for explaining how and why we err.

DURKHEIM AND ACTION THEORY

Evidence that Durkheim is not an action theorist.—Contrary to Warner, there is massive evidence that Durkheim was not an action theorist. We wish that Warner had identified Durkheim's action theory, but his only attempt is the same approach earlier used by Parsons; that is, he cites the appearance of subjective categories in Durkheim as decisive evidence that Durkheim

[1] We thank Carolyn J. Mullins for her editorial assistance.

was an action theorist and thereby loses the opportunity to move the discussion forward. Since Warner has provided us with no specific examples of Durkheim's action theory to assess, we state the following general argument. Surely an explanation of social behavior from the actor's standpoint requires that subjective states be treated not just as effects but also as causes; not just as dependent but also as independent variables. In contrast, Durkheim treated these subjective categories only as effects.

Warner correctly states that effects in one context can be treated as causes in another. However, when Durkheim permitted subjective categories to enter his sociological explanation, he carefully avoided treating them as causes. For instance, in his longest discussion of the characteristic subjective states associated with the different types of suicide, Durkheim ([1897] 1951, pp. 277–94) did not specify additional subjective causes to explain variation in social suicide rates not explained by the social causes of suicide (integration and regulation); rather, he was interested only in individual types of suicide insofar as they denoted and reflected the impact of social, etiological forces.

The two foremost examples of categories that are both social forces and subjective types are egoism and anomie. Since in both cases society's presence in the individual is at a minimum, one might expect individual variables to be important in causing suicide. However, in egoism "new moralities," collective in nature, elevate individualism to a principle and, as social currents, drive on the egoistic suicide so that "at the very moment that, with excessive zeal, he frees himself from the social environment, he still submits to its influence" (Durkheim [1897] 1951, p. 214). In anomie variations in individual aspirations are determined not by individual passions, which are "unlimited so far as they depend on the individual alone" (p. 247), but by variation in (1) the limits set by a person's social position and (2) the strength of social regulation. High aspirations derive not from the physical nature of the organism (pp. 246–47) but from the excitement of extended economic markets (pp. 255–56), feelings of power inspired by a state of wealth (p. 254), and the elevation of industry (p. 255) and liberated desires to marks of moral distinction (p. 257). Thus the suicidal impulse, which follows from an imbalance between needs and means, is social.

Durkheim's question is not why this or that individual kills himself; rather, he seeks to explain the suicide *rate* in a group (or category) of individuals. Although his theory of egoism uses meaninglessness to link social cause with social suicide rate, variation in suicide rates is explained not by the fact that one individual experiences a higher or lower level of meaninglessness than another but by the level of meaninglessness that characterizes the group or category in question. A parallel argument applies to the means-needs balance Durkheim uses to link another cause (anomie) with social effect (suicide rate).

Durkheim could have held that, while social causes account for some variation in suicide rates, individual differences in subjective states also account for some variation. Or, with Warner, he might have treated social variables as causes with respect to individual, subjective states but also treated subjective states as causes in relation to social effects. Finally, he might have tried to explain differences in suicidal impulses (due to differences in subjective states, individual temperament, personality, intelligence, etc.) among individuals subjected to identical social causes. However, he made none of these arguments.

Warner's arguments.—Attempting to reject the contention that an individual-social dualism is central to Durkheim's perspective, Warner (pp. 1335–36) (1) argues that "the dualism that [Pope] cites appears primarily in Durkheim's later work," (2) observes that "Durkheim presents the individual and society as in at least potential harmony," and (3) notes that Durkheim "located dualism within the individual psyche."

In response, (1) we note that the individual-social dualism occurs in both early and late Durkheim ([1897] 1951, pp. 37–38, 209, 309–10, 318–19, 335; [1893] 1960, pp. 80, 92, 100–101, 105–6, 129–30, 152, 195, 197–98, 227, 305, 349–50; [1912] 1961, pp. 237–38, 271, 297–98, 307, 351, 356–57; [1914] 1964*a*; [1895] 1964*b*, pp. 1–13). More importantly, both his early and late theories assume the validity of this dualism. (2) We agree that Durkheim saw potential harmony between the individual and society. Precisely because this harmony is caused by society's ability to control the unsocialized aspect of individuals, for Durkheim the degree of individual-social harmony varies directly with the strength of the social factor (integration, regulation, solidarity, moral rules, the collective conscience, collective representations). (3) Durkheim viewed the individual psyche as composed of both a social and a nonsocial component; consequently, there is no contradiction between his individual-social dualism and location of this dualism within the individual psyche. In short, none of Warner's arguments undermines the centrality to Durkheim's theory of his individual-social dualism.[2]

[2] Warner (pp. 1336–37) further argues that (1) we fail to see "the individual and society as mutually interpenetrating"; (2) we may feel that a theorist who recognizes internalization cannot also be an action theorist; (3) our interpretive stance "flatten[s]" and "slice[s] off" Durkheim's social psychology, one instance of which is our failure "to explicate the social-psychological basis of Durkheim's distinction between egoism and anomie." However, (1) our discussions of Durkheim's views on internalization and the relations between the individual and society analyze Durkheim's conception of the interpenetration between the individual and society (Pope 1973, pp. 405–6, 409, 413; 1975, p. 113; 1976, pp. 187–97; Pope, Cohen, and Hazelrigg 1975, p. 419). Of course, Durkheim did not assert parity between the two halves of his dualism. Rather, he emphasized that society is the product of *social* man and that the individual is far more the product of society than vice versa (see, e.g., Durkheim [1897] 1951, pp. 38–39, 323; [1893] 1960, p. 350; [1887–88] 1974, p. 196). (2) Nowhere do we subscribe to the indeed peculiar "view that internalization denies action" (Warner p. 1337). Rather, Pope rejected Parsons's claim that Durkheim's discovery

IDEAS, NORMS, AND INTERESTS IN WEBER

Warner's assertions versus our interpretation.—In rescuing Weber from Parsons we certainly did not "demean" (Warner, p. 1344) the importance of norms in Weber. Indeed, we asserted that "Weber had a focus of interest on the normative aspects of action" (Cohen, Hazelrigg, and Pope 1975, p. 231). We noted (ibid.) that norms have varying importance in Weber's numerous analyses. We have shown that in many instances Weber emphasizes non-normative elements. On the other hand, his sociology of law, his analysis of legitimate domination, his discussions of value-rationality, and his treatment of discipline in religious sects all identify normative factors as important.[8]

Ideas, norms, and interests.—Weber felt that both ideas and interests are important, as are their mutual relations; and each influences and is partially autonomous of the other. Contrary to Warner's assertion (p. 1339), we did not interpret Weber as viewing ideas and interests as inherently or necessarily opposed. Ideas channel, structure, and define interests. However, Weber rejected attempts to reduce either ideas or interests to the other. It follows, then, that ideas and interests can conflict. For instance, religious doctrines which made salvation uncertain (Calvinism) or difficult to obtain (early Buddhism) came into conflict with believers' interests in the immediate subjective certainty of being saved.

Because norms are one type of idea, parallel observations may be made about the relationship between norms and interests. Contrary to Warner's assertion (pp. 1338–39), we did not argue that Weber viewed norms and interests as inherently and necessarily opposed. However, we did point out that norms and interests *may* be opposed.

In his attempt to deny any implication that Weber subscribed to a norms-versus-interests dualism, Warner incorrectly asserts that Weber did not compare and contrast the effects of interests with the impact of both ideas and norms. Weber (1946, p. 280) stated that "not ideas, but material and ideal interests, directly govern men's conduct," and ([1922] 1968, 1:30) that "orientation to the situation in terms of the pure self-interest of the individual and of the others to whom he is related can bring about results comparable to those which imposed norms prescribe, very often in vain." Clearly, in Weber's scheme interests are distinct from both ideas and norms; also norms and (other) ideas and interests may be compatible or mutually reinforcing, but they also *may* be opposed.

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and use of internalization made the latter an action theorist (Pope 1973, pp. 405–6; 1975, p. 113). (3) Pope has analyzed Durkheim's social psychology of suicide and identified the social-psychological distinction between egoism and anomie (Pope 1976, pp. 14–33).

[8] We did not label Weber a utilitarian in Parsons's sense; this is only one more of Warner's misattributions without citation. Clearly, Weber believed that self-interest depended on such factors as a person's status group and class situation; thus, in his scheme, ends were far from random.

Paying cognitive and noncognitive elements their due.—We agree with Warner (p. 1340) that an emphasis on interests "should not obscure Weber's recognition of such social structural matters as norms and ideas." It should be clear, though, that norms and ideas have different roles. The cognitive element, which Warner rightly emphasizes, pervades Weber. Weber's *verstehen* approach emphasized subjective meanings,[4] and his comparative macrosociology emphasized religious beliefs and the process of rationalization. Here one might detect an "overweening emphasis on the cognitive," if so inclined. But this emphasis should be contrasted with Weber's ([1922] 1968, 1:30) assertion that much of social action is "not determined by orientation to any sort of norm"—a denial that *any* aspect of norms, motivational or cognitive, decisively affected individual orientations. Indeed, Weber's religious analyses repeatedly described inattention to norms; for example, attempting to relieve anxiety, the ideal-typical Calvinist ignored tenets that denied the possibility of *certitudo salutis* (Weber [1905] 1958*a*, p. 110), and most Taoists ignored orthodox theology to concentrate on "naked magic" (Weber [1920] 1951, p. 204). We accept Warner's distinction between motivational conformity to norms and simple orientation to them; Weber ([1922] 1968, 1:32) himself stated that people often orient to norms while disobeying them. Nonetheless, Weber portrayed actors not simply as disobedient but often as not oriented to norms (see Cohen et al. 1975, pp. 231–36).

Warner's (pp. 1343–44) single example to illustrate cognitive orientation to norms was, appropriately enough, from Weber's sociology of law. According to Weber's theory, mutual agreement about what the law says and the calculability of legal sanctions often affect the structure of social life. However, Warner's example needs to be balanced against other evidence, such as Weber's ([1922] 1968, 3:942) assertion that economic power and its application often exercise "a determining influence on the structure of domination." In short, legal norms are just one of the factors that Weber felt structure society.

WEBER AND DURKHEIM

Parallels drawn by Warner.—Warner proposed the cognitive dimension itself as a Weber-Durkheim parallelism, but the examples he cites do not support the degree of parallelism he asserts. For Weber, the cognitive construction of ends through a doctrine of salvation reflects an orientation to a

[4] However, not all Weberian actors were, as Warner (p. 1319) puts it, "oriented to problems of cosmic meaning." We (Cohen et al. 1975, pp. 234–35) have shown that not every religion was, in Weber's view, concerned with the meaning of life. Also, not every status group was typically concerned with problems of meaning: intellectuals and religious leaders often were, but peasants, bureaucrats, warriors, the middle class, and the lower classes typically were not.

system of ideas; in contrast, for Durkheim the dependence of aspirations on restraints is an orientation to structured social opportunities. The Durkheimian need for contract law, presented by Warner as an efficiency measure, is not parallel to the desire for meaning or certainty of salvation in Weber's work. Likewise, the parallel between the needs of calculability (Weber) and contract law (Durkheim) breaks down because the former is a material interest of businessmen while the latter is a function that serves modern society. In sum, far from showing parallels, Warner's examples show only that the category of the cognitive is broad enough to accommodate very diverse elements.

Approach to cognitive phenomena.—First, Weber and Durkheim diverged radically in their very conception of the cognitive phenomena of primary concern to the sociologist. For Durkheim these phenomena are sui generis, emergent phenomena, that is, the cognitive dimension of moral rules, the collective conscience, or collective representations. Weber would reject Durkheim's very conception of such cognitive phenomena as hopelessly reified. For Weber, cognitive phenomena are the ideas of individuals. Regardless of what proportion of individuals in a group or category Weber might feel subscribe to given ideas, the underlying difference remains between Durkheim's conception of collective ideas as an emergent social phenomenon and Weber's conception of ideas as the (more or less widely shared) ideas of individuals.

Second, Weber and Durkheim had different ideas about the interests that cognitive phenomena serve. Durkheim emphasized that cognitive phenomena serve society's interests. Collective representations control the nonsocial, disruptive force of the unsocialized individual. Beyond that, the specifically cognitive element in collective representations serves definite societal interests. For instance, *The Elementary Forms* argues that religious beliefs are conducive to the enactment of religious rites, which are themselves the sine qua non of society's very existence. Again, both as imposed upon the individual and as the individual develops his own individualized, individual representations of collective representations, the latter constitute the source of the common ideas and understandings without which social life (society) would be impossible.

In contrast, Weber believed that ideas are differentially compatible with the interests of individuals and groups differentially located in the social structure (1946, pp. 282–84). Rejecting reified conceptions of society, he analyzed the way in which ideas enter into the struggle for advantage between individuals and groups in the competitive arena we call society (see Cohen et al. 1975, p. 238; Weber [1920] 1951, p. 217; [1917] 1958*b*, pp. 130, 259–60, 270–71, 283–85).

Third, Durkheim treated the strength of ideas as primary and their content as a secondary, derived phenomenon in the following specific sense:

equally powerful collective ideas are essentially equal in the decisive variable aspect of ideas, namely, the degree to which they reflect and serve social interests. In contrast, Weber was greatly concerned with the content of ideas (e.g., Weber [1920] 1951, pp. 142–249; [1905] 1958*a*, pp. 95–154) and did not treat content as a secondary, derived characteristic.

Fourth, Weber and Durkheim also differed about the processes that (potentially) produce cognitive consensus. Characteristically, Durkheim viewed society itself as the source and cause of the cognitive consensus in society. He believed that many of our basic ideas, including the fundamental categories of thought, both reflect social realities and interests, and are imposed by society upon individuals. In contrast, Weber analyzed cognitive consensus in terms of the complex interplay of variables such as the strictly cognitive appeal of the ideas themselves, the compatibility between ideas and interests, the degree to which ideas are adopted by given groups, and the concern and ability of the groups to achieve consensus by imposing ideas on others.

Cognitive consensus.—Warner's reference to commonly held orientations as a Weber-Durkheim parallel overlooks great differences in the degree to which Weber and Durkheim saw ideas as shared. Whereas Durkheim ([1912] 1961, pp. 18, 474–75) viewed ideas as shared thoughout society and identified "universal consensus" as "the distinctive feature of societies" ([1887–88] 1974, p. 196), Weber ([1922] 1968, 1:48–53) saw ideas as typically shared by corporate groups or (1968, 2:468–518) within status groups, but usually not by whole societies (for a fuller discussion, see Pope et al. [1975], pp. 423–24).

Warner asserts that our position impels us "to deny that shared cognitive ideas are often adduced in Weber's analysis of religion" (p. 1338). Nothing in our position impels us toward a statement we did not make and do not believe. However, we judge Warner's reference (p. 1338) to Weber's analysis of common religious ideas which "captured the imaginations of whole continents" to be as potentially misleading as it is enlightening. If Weber felt that a whole continent might be captured by one religion, still he did not treat the eventuality as typical. Indeed, he ([1905] 1958*a*) portrayed Europe as split among the Catholics, Lutherans, Calvinists, Pietists, Methodists, Baptists, Mennonites, Quakers, Jews, and so on, each with a somewhat different salvation doctrine and economic ethic (see, e.g., Weber [1905] 1958*a*, pp. 95–154). Even the "spirit of capitalism" could not be regarded as the ethos of a whole continent. It met with different degrees of acceptance by Catholics and Protestants (pp. 35–46). Moreover, Weber's ([1921] 1977, p. 10) uncomplimentary analysis of Polish farm workers portrayed a primitive, precapitalist mentality; the Junkers, too, were holdovers from a previous era: though becoming capitalists, they were economically oriented to monopolistic privileges (Weber 1946, pp. 373, 381–82; [1921] 1977, p. 13). *The*

Religion of China and *The Religion of India* also describe ideological splits. For example, orthodox Confucianism struggled for centuries with various heterodoxies, including Taoism (Weber [1920] 1951, pp. 171–80) and Buddhism (pp. 195–96). Moreover, Weber did not view corporate groups and status groups as the idea-sharing unit simply because they were his focus of analysis; on the contrary, he analyzed whole societies and found them split. In contrast, Durkheim viewed common moral rules, or collective representations and a common religion as the very essence of society.

CONCLUSIONS

Taken in their original context, Durkheim's consideration of individual actors' subjective states does not make him an action theorist. He did not subscribe to a theory of action—voluntaristic (Parsons), revised (Warner), or otherwise. Normative elements occupy vastly different places in Weber's and Durkheim's theories. Furthermore, though Weber and Durkheim both were concerned with the cognitive factor, their conceptions of both this variable and its place in sociological theory differ substantially. Assessment of the cognitive factor only expands the grounds for rejecting the assertion that they developed parallel (Warner) or convergent (Parsons) theories. In sum, we remain convinced of fundamental differences between the sociological theories of Weber and Durkheim.

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Race, Class, and Income Inequality[1]

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The basic thesis of this paper is that class, defined within the Marxist tradition as common position within the social relations of production, mediates racial differences in income returns to education. That is, class position is viewed as a determinant of the extent to which education can be transformed into income, and thus it is hypothesized that much of the commonly observed racial difference in returns to education is a consequence of the distribution of racial groups into class categories. The results of the study strongly confirm this perspective: the differences in returns to education between black and white males largely disappear when the regression equations are run within class positions.

One of the most consistent findings of research on racial inequality is that black males receive considerably lower income returns to education than white males. Weiss (1970, p. 154) found that, within specific age groups, black males received significantly lower returns to education than white males, whether education was measured as years of schooling or as achievement level. Siegel (1965) found that, net of occupation and region of the country, the difference in expected incomes of black and white males increased monotonically with education: at less than elementary education, blacks earned $700 less than whites (net of occupation and region) in 1960; at the high school level this increased to $1,400, and at the college level, to $3,800. Duncan (1969) has shown that even after controlling for family background, number of siblings, and occupational status black males still receive lower returns to education than white males.[2]

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[2] To my knowledge, the only study which claims to present findings different from these results is the research of Ross Stolzenberg (1973, 1975). Stolzenberg estimates a rather complicated income-determination equation within 67 detailed occupational categories for both black and white males. He then compares the partial derivatives of income with respect to education for the equations and finds that in nearly one-half of the occupational

None of these studies, however, has controlled for class position as understood within the Marxist tradition, that is, the position of individuals within the social relations of production. The underlying premise of a Marxist class analysis is that, while the diverse dimensions of social inequality cannot be reduced to class inequality, class relations nevertheless play a decisive role in shaping other forms of inequality. In the study of income inequality, this implies that class relations organize the structure of income inequality in the sense that class position shapes the ways in which other causes influence income. If this notion is correct, then an analysis of racial differences in income that ignores positions within the social relations of production is incomplete. More concretely, if it is true that the returns to education vary substantially across class positions, and if it is true that black and white males are distributed quite differently across class positions, then much of the racial difference in returns to education could in fact be a consequence of the class distribution of races. This paper will explore such a possibility.

OPERATIONALIZING CLASS

Before developing a series of explicit hypotheses about the interactions of race and class in the income-determination process, it is necessary to discuss briefly how the Marxist notion of class will be operationalized in this study.[3] When non-Marxists use the term "class," it generally designates a group of people who share common "life chances" or market positions (Weber 1968, p. 927; Giddens 1973; Parkin 1971, pp. 18–23), common positions within status hierarchies (Warner 1960; Parsons 1970, p. 24), or common positions within authority or power structures (Dahrendorf 1959, p. 138; Lenski

categories the partial derivative is larger for blacks than for whites. Thus, he concludes, "Earlier findings suggesting high within-occupation racial differences in wage returns to schooling . . . were probably artifacts of the gross occupational classifications used. These past findings appear to have been produced by the tendency of black men to be concentrated in the lowest-paying *detailed* occupation categories within the major occupational group in which they are employed" (1975, p. 314). The problem with this conclusion is that Stolzenberg uses a natural logarithmic transformation of income, whereas Siegel (1965) uses raw dollars. This means that Stolzenberg is estimating rates of returns to education rather than absolute returns. The absolute returns to education within the detailed occupational categories may still not have differed significantly between blacks and whites, but Stolzenberg's results do not demonstrate this. I ran Stolzenberg's equation using the data in the present study, calculated the partial derivatives for all blacks and all whites, and discovered that the rates of return for all blacks were significantly greater than for all whites (Stolzenberg does not report the results for all blacks and all whites). Stolzenberg's results thus indicate that these higher rates of return to education for black men as a whole can also be found within about one-half of the detailed occupations held by black men. His results do not indicate that the absolute returns for black and white men are the same within detailed occupations.

[3] For a more detailed discussion of this operationalization of class, see Wright (1976*b*, pp. 137–39). For an extended discussion of the concept of class within the Marxist tradition, see Wright (1976*a*; 1976*b*, pp. 20–90) and Poulantzas (1975).

1966, p. 95). In contrast to these usages, Marxists define classes primarily in terms of common structural positions within the social organization of production. In contemporary American society, this means defining classes in terms of positions within capitalist social relations of production.

For the purposes of the present analysis, position within the social relations of production will be defined according to two basic criteria: whether or not the individual owns his or her own means of production, and whether or not the individual controls the labor power of others (i.e., supervises people on the job). These two criteria generate four basic class positions, as illustrated in figure 1. "Ownership of the means of production" is operationalized by the question, "Do you work for yourself or someone else?" For self-employed individuals, "control of the labor power of others" is operationalized in terms of having employees; for individuals who are not self-employed, this criterion is operationalized by the question, "Do you supervise the work of others, or tell other employees what work to do?"

One further distinction will be made within this basic class schema. Clearly, some of the people who are placed in the manager/supervisor category are nominal supervisors. This would be the case, for example, for the head of a work team who serves as the conduit for orders from above but who lacks any capacity whatsoever to invoke sanctions on his or her fellow workers. Proper managers are thus distinguished from nominal supervisors by the question, "Do you have any say in the pay and promotions of your subordinates?"

It is important to be clear about the logic underlying these class categories. They are not simply proxies for occupations. "Occupation" designates the technical function performed by individuals within the labor process; "class" designates the social relationship within which those technical functions are performed. While, of course, different class positions include different mixes of occupations, every broad category of occupations is

		Ownership of the Means of Production	
		YES	NO
Control over the labor power of others	YES	Employers	Managers/Supervisors
	NO	Petty Bourgeoisie	Workers

FIG. 1.—Criteria for class position. In the upper left hand quadrant, the term "Employers" is used rather than "Capitalists," since in the present study most of the individuals in this category employ fewer than 10 workers. See Wright (1976*a*, pp. 35–36) for a discussion of small employers as a contradictory class location between the capitalist class and the petty bourgeoisie.

represented within each class category (Wright 1976b, pp. 168–73; Wright and Perrone 1977).

HYPOTHESES

The empirical investigation will revolve around six hypotheses.[4]

1. *Managers as a whole will receive much higher returns to education than workers.*—This basic result has already been established in an earlier study (Wright and Perrone 1975, 1977). The rationale underlying this hypothesis is based on an analysis of the specific position of managers within capitalist social relations of production. Specifically, this analysis suggests that within the managerial category there will be a strong link between income and hierarchical position on the one hand and hierarchical position and education on the other. The high returns to education within the managerial category are a result of this double link.

First, examine the link between hierarchy and income. The behavior of all employees within a capitalist organization is controlled by a combination of repressive sanctions and positive inducements. As one moves up the managerial hierarchy, however, the balance shifts between these two modes of control. While repressive controls may be effective in creating conformity to explicit rules, they are not terribly well suited to generating responsible and enthusiastic job performance. Because the managerial hierarchy is one of increasing responsibility (and, in a limited way, increasing power as well), there will be a tendency for the behavior of higher managers to be controlled more exclusively through a structure of inducements. The result is that managerial hierarchies will be characterized by a steep income gradient attached to authority position (Tannenbaum et al. 1974, p. 107), even when the education of managers is held constant (Wright 1976b, pp. 235–38).

Second, examine the relationship between educational credentials and hierarchical position. In both the working class and the managerial category, education is in part a determinant of the value of the labor power of the individual (or what non-Marxist economists typically call "human capital"). It is therefore to be expected that both workers and managers would receive a positive income return to their education. However, among managers, educational credentials serve a second function. In addition to creating genuine skills, education also serves as an institutional mechanism for legitimating inequalities of power within capitalist organizations. In practice,

[4] All of the hypotheses that follow center on the relationship of the working class and the managerial category to racial differences in returns to education. Since such a small percentage of black males are either capitalists or petty bourgeois it is impossible, using the sample available for this study, to examine systematically the interactions of these class positions with race.

this means that there will be a general tendency for people with lower credentials not to be promoted above people with higher credentials, and thus there will be a tendency for managerial hierarchies to be characterized by rather steep educational credential gradients (Tannenbaum et al. 1974, p. 112).

The combination of this steep education gradient and steep income gradient associated with hierarchy means that the managerial category as a whole will be characterized by an especially high return to education. That is, in addition to the direct return to education resulting from increases in the market value of labor power (skills), which both managers and workers receive, managers receive an additional increment of income for education, stemming from the link between the legitimation function of education within hierarchies and the use of income as a control mechanism within authority hierarchies. (For a more detailed discussion of this interpretation, see Wright and Perrone 1977; Wright 1976*b*, pp. 105–10.)

2. *Black males will be more concentrated in the working class than white males.*—While we will not explore the actual mechanisms by which individuals are sorted into class positions, it is nevertheless predicted that one outcome of this sorting process is that blacks will be more heavily concentrated in the working class than whites.

3. *When class position is ignored, black males will receive lower returns to education than white males.*—This is the standard finding in sociological studies of racial differences in returns to education. It will be formally tested in order to show that the usual results hold for the data used in the present study.

4. *Within the working class, the returns to education for black and white males will be much more similar than for all blacks and all whites.*—If at least part of the overall difference in returns to education for blacks and whites is a consequence of the class distributions of the two racial categories, then it would be expected that within the working class itself the returns should be much more similar. While white workers may be in relatively privileged working situations as compared with black workers, neither white nor black workers occupy positions of authority (by definition) and, thus, neither receive the legitimation increment of returns to education discussed in hypothesis 1.

5. *Within the supervisor category, the returns to education for black and white males will be more similar than for all blacks and all whites.*—The argument here is essentially the same as that presented in hypothesis 4. To the extent that the overall racial differences in returns to education are a consequence of class distribution, within a single class position—in this case, the very bottom level of the managerial hierarchy—the returns for blacks and whites should be much more similar than for all blacks and whites.

6. *Within the managerial category, black males will have lower returns to*

education than white males.—The argument in hypothesis 1 concerning the high returns to education of managers hinges on the dual link between education and hierarchical position and between income and hierarchical position. If a particular category of managers is highly concentrated at the bottom of the authority structure, then this education-hierarchy legitimation mechanism will tend to be attenuated. Although no data are available in the present study concerning the hierarchical distribution of race within the manager category, white males can be expected to be much more evenly distributed throughout the hierarchy than black males. If this is the case, then within the managerial category the returns to education for black males should be considerably smaller than returns for white males.

DATA

The data for this study come from the eighth wave of the *Panel Study of Income Dynamics* (Institute for Social Research 1975) conducted by the Institute for Social Research (ISR) at the University of Michigan. While in the original year of the panel study the sample was a random sample of 5,000 households, by the eighth year of the study, because of successive nonresponses, the sample was no longer genuinely random.[5] While this probably will not seriously affect the regression results, it certainly may affect hypothesis 2, concerning class distribution of races. Two other data sets, the 1969 Survey of Working Conditions and the 1973 Quality of Employment Survey (both from ISR), will thus be used for hypothesis 2.[6] Throughout the analysis, the samples will be restricted to active participants in the labor force.

EQUATIONS

In order to test the hypotheses about class and race interactions with returns to education, two regression equations will be estimated for each of the race-class groups being compared:[7]

[5] Two things need to be noted concerning nonrandomness of the sample in the *Panel Study of Income Dynamics*. First, whenever an individual left the original household in the study (because of divorce, high school graduation, etc.), the "split-off" was also included in the subsequent years of the panel. Thus, the sample is not particularly skewed on age distribution. Second, a fairly complex system of weights has been devised to correct, at least partially, for nonrandomness in nonresponse. Thus, the regression results in the present study are probably reasonably reliable in spite of the nonrandomness of the sample.

[6] A discussion of these data sets can be found in Wright (1976*b*, pp. 132–35).

[7] The significance of the slope differences between groups will be assessed using the conventional dummy-variable interaction model (Kmenta 1971, pp. 419–23). This means that, if we are comparing the education slopes for two groups, the *t*-test would be:

$$t = (B_{11} - B_{12})/\sqrt{(\nu_1 s^2_{B_{11}} + \nu_2 s^2_{B_{12}})/(\nu_1 + \nu_2)}\,,$$

$$\text{Income} = a + b_1\text{Education}\,, \tag{1}$$

and

$$\begin{aligned}\text{Income} = a &+ b_1\text{Education} + b_2\text{Occupational Status} + b_3\text{Age} \\ &+ b_4\text{Seniority} + b_5\text{Father's Status} + b_6\text{Father's Education} \\ &+ b_7\text{Parental Economic Condition} + b_8\text{Annual Hours Worked}\,.\end{aligned} \tag{2}$$

While the present study will not investigate the more complex structural equation model which underlies the second equation, this equation is nevertheless important in assessing the extent to which the class interactions observed in equation (1) may be consequences of the class distributions of the various control variables in equation (2). Thus, for example, if in hypothesis 1 the greater returns to education for managers were entirely due to the occupational status distribution across class categories, then, when occupational status is included in the equation, the differential returns to education between classes should be substantially reduced. All of the control variables in equation (2) are to a greater or lesser extent either causes or consequences of education, and all of them can also be plausibly thought to vary with class position. By controlling for these variables in equation (2), we will be able to see if the differences in education coefficients between the various groups being compared can be considered direct consequences of class and race.[8]

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where B_{11} is the education coefficient for group 1, B_{12} is the education coefficient for group 2, $s^2_{B_{11}}$ and $s^2_{B_{12}}$ are the standard errors of the coefficients for groups 1 and 2, respectively, and ν_1 and ν_2 are the degrees of freedom for groups 1 and 2, respectively. The denominator in this t-test is the standard error of the education × class-dummy interaction term in the usual dummy-variable interaction model. It can be computed equally well from the separate regression equations for the two groups using the above formula.

[8] A brief comment about the kind of information contained in these regression equations might clarify the logic of eq. (2). These equations describe characteristics of positions in a social structure. While the data are tagged onto individuals, the equations themselves do not adequately describe individual income-attainment processes, but only the income-determination process for certain structural locations. Since we have no data on how individuals are sorted into structural positions, and since education obviously may play an important role in this sorting process, the education coefficients in the various equations may say very little about individual returns to education. What they describe is the return to education which characterizes the position "worker," "manager," "black worker," etc. Now, positions within a social structure can be characterized along many dimensions. Marxists assert that the critical dimension of position within social structures is class relations. Other social scientists have argued explicitly or implicitly that occupational status or the social backgrounds selected into positions are the key dimensions. Equation (2) in effect holds constant a variety of positional dimensions which have some claim to being important determinants of the relationship between education and income. To the extent that class and race positions differ on education coefficients even when all of these factors are held constant, these coefficient differences can be considered direct, unmediated consequences of the class-race positions themselves. (See Wright and Perrone 1977, p. 38, for a further discussion of this issue.)

VARIABLES

The variables to be included in the analysis are measured as follows.

1. *Income* is measured by total annual taxable income received by the individual. In addition to wage and salary income, this variable includes income from assets, interest, and other sources of unearned income. Both regression equations were also estimated for three other income variables: annual earnings, an estimate of "permanent income" (an average of income over the previous seven years), and imputed hourly wage (wage and salary income divided by total annual hours worked). In none of the comparisons of racial differences in returns to education did the results differ significantly for these alternative income variables (see Wright 1976*b*, pp. 328–39).

2. *Education* is operationalized by a quasi-credential scale in which:

0 = no schooling or illiterate,
1 = less than elementary school,
2 = elementary school,
3 = some high school,
4 = completed high school,
5 = high school plus some nonacademic training,
6 = some college,
7 = college degree, and
8 = graduate training.

3. *Occupational Status* is measured by the standard Duncan SEI scores. These scores were available only for the seventh wave of the panel study (1974), while the class position questions were asked only in the eighth wave. This means, in effect, that we have the individual's occupational status score at the beginning of the year in which income was earned and his class position at the end of the year (i.e., the beginning of the following year).[9]

4. *Age* is included in the regression both as a rough control for cohort effects and as a rough measure of years of experience in the labor market.

5. *Seniority* designates the number of years the individual has worked for the same employer, or the number of years an individual has been self-employed in the same business.

6. *Father's Status* is measured by the average SEI score for the father's gross-census occupation category. While this is clearly a much weaker variable than a status score based on the three-digit occupation classification, it is the best available from the panel study data.

[9] One other minor point concerning the status variable needs mentioning: about 6% of the sample represents "split-offs" in the 1975 year of the survey, that is, those who left a household after the 1974 survey and set up a new family unit. For these people a three-digit occupation classification was not available, and thus in these cases the Duncan score is based on the average SEI value for the gross-census occupation categories.

7. *Father's Education* is measured by the same scale as respondent's education.

8. *Parental Economic Condition* is a scale reflecting the respondent's subjective perception of parents' economic status in which:

1 = parents generally poor,
2 = parents generally about average, and
3 = parents generally well-off.

9. *Annual Hours Worked* is a product of the number of weeks worked in the previous year and the average number of hours worked per week.

The means and standard deviations for each of these variables for each of the race and class groups included in this study are given in table 1.

RESULTS

Hypothesis 1: Managers as a whole will receive much higher returns to education than workers.—Table 2 indicates that, in the simple regression of income on education, workers receive $851 for each increment in education while managers/supervisors receive $1,689. When the various control variables in equation (2) are added, the returns for workers are $655, while the returns for managers/supervisors are $1,169. In both cases, the difference in returns is significant at the .001 level. This hypothesis is thus strongly confirmed.

The theoretical rationale for this hypothesis centers on the relationships among hierarchical position, education, and income. If this rationale is correct, then it would be expected that the high returns to education among managers/supervisors would disappear if we examined a single hierarchical level within the managerial hierarchy. In a limited way, we can test this proposition by examining separately the returns to education among mere supervisors (positions which involve no say over pay and promotions) and proper managers, since mere supervisors can be considered the bottommost level of the hierarchy. As can be seen in table 2, mere supervisors differ hardly at all from workers in returns to education (although they receive more income at every level of education), whereas proper managers receive much higher returns.

One obvious objection to these results is that the true relationship between educational credentials and income might be curvilinear. Thus, what appear to be slope differences between workers and managers might in fact simply be a consequence of the two linear regressions reflecting different parts of a single, nonlinear credential-income function. Figure 2 presents the relationship between the linear regressions in table 2 and the mean incomes for each educational level for workers, managers, and supervisors. Table 3 indicates the R^2 for equation (1) using the credential scale and using a series of dummy variables to represent the individual levels of the scale. Both the visual

TABLE 1

MEANS AND STANDARD DEVIATIONS OF VARIABLES USED IN REGRESSION EQUATIONS

	Annual Taxable Income ($)	Education	Occupational Status	Age (Years)	Seniority (Years)	Father's Education	Father's Status	Parents' Economic Condition	Annual Hours Worked
Workers	10,976	4.4	34.0	36.5	6.7	2.9	29.5	2.5	2,047
	(5,929)	(1.8)	(21.8)	(13.0)	(7.5)	(1.7)	(18.4)	(1.5)	(591)
Managers/supervisors	15,257	5.3	49.5	39.1	7.7	3.2	34.3	2.6	2,300
	(8,111)	(1.8)	(23.4)	(12.3)	(8.0)	(1.8)	(20.3)	(1.5)	(581)
Supervisors	12,266	4.9	42.2	38.3	7.4	3.1	32.4	2.6	2,189
	(6,081)	(1.8)	(23.5)	(13.2)	(7.9)	(1.7)	(19.7)	(1.5)	(578)
Managers	18,090	5.6	56.6	40.0	8.0	3.4	36.2	2.7	2,412
	(8,770)	(1.7)	(20.7)	(11.2)	(8.1)	(1.8)	(20.7)	(1.5)	(559)
Whites	14,615	5.0	43.8	38.7	8.0	3.1	33.1	2.7	2,231
	(11,133)	(1.8)	(24.0)	(13.0)	(8.8)	(1.7)	(19.7)	(1.5)	(657)
Blacks	9,307	3.8	28.0	37.4	7.3	2.4	22.4	2.1	2,050
	(5,408)	(1.8)	(20.5)	(13.1)	(7.6)	(1.4)	(16.3)	(1.5)	(630)
White workers	11,339	4.6	36.0	36.6	6.8	3.0	31.0	2.6	2,055
	(6,020)	(1.8)	(22.0)	(13.0)	(7.6)	(1.7)	(18.5)	(1.5)	(589)
Black workers	8,469	3.6	24.1	36.1	6.8	2.3	20.2	2.2	1,964
	(4,411)	(1.6)	(16.8)	(13.3)	(7.3)	(1.2)	(13.5)	(1.6)	(555)
White supervisors	12,519	5.0	43.1	38.4	7.5	3.2	32.8	2.7	2,189
	(6,111)	(1.8)	(23.4)	(13.5)	(8.0)	(1.7)	(19.6)	(1.5)	(584)
Black supervisors	8,646	3.6	28.2	36.7	7.1	2.4	25.2	2.0	2,191
	(4,071)	(1.8)	(20.3)	(10.2)	(7.2)	(1.7)	(19.2)	(1.4)	(596)
White managers	18,301	5.6	57.3	40.0	7.8	3.4	36.6	2.7	2,412
	(8,708)	(1.6)	(20.0)	(11.3)	(8.0)	(1.8)	(20.6)	(1.5)	(539)
Black managers	13,250	4.8	46.2	39.9	11.4	2.7	31.1	1.6	2,321
	(6,090)	(1.9)	(28.8)	(11.5)	(9.4)	(1.8)	(23.6)	(1.1)	(841)

NOTE.—Standard deviations are numbers in parentheses.

TABLE 2

COMPARISON OF RETURNS TO EDUCATION FOR WORKERS, MANAGERS/SUPERVISORS, MANAGERS, AND SUPERVISORS

A. REGRESSION EQUATIONS

Dependent Variable (Total Annual Taxable Income)	Unadjusted Constant	Education	Occupational Status	Age (Years)	Seniority (Years)	Father's Education	Father's Occupational Status	Parents' Economic Condition	Annual Hours Worked	R^2
Workers (N=1,715):[a]										
Eq. (1):										
B	7,193	851.4	...	...	...	...	...	...	...	.066
(SE)	...	(77.3)	...	...	...	...	...	...	...	...
Beta	...	.26	...	...	..	...	...	...	...	...
Eq. (2):										
B	−6,627	655.1	67.9	122	124	249	−30	263	3.2	.369
(SE)	...	(92.8)	(7.0)	(11)	(18)	(89)	(7.6)	(87)	(.19)	...
Beta	...	.20	.25	.27	.16	.07	−.09	.07	.32	...
Managers/ supervisors (N=1,014):										
Eq. (1):										
B	6,382	1,689.1	...	...	...	...	...	...	...	.135
(SE)	...	(133.5)	...	...	...	...	...	...	...	...
Beta	...	.37	...	...	...	...	...	...	...	...
Eq. (2):										
B	−7,145	1,168.7	98.6	140	127	−251	29	−422	2.6	.373
(SE)	...	(153.4)	(11.0)	(21)	(29)	(143)	(12)	(152)	(.35)	...
Beta	...	.25	.28	.21	.13	−.05	.07	−.08	.18	...
Supervisors (N=535):										
Eq. (1):										
B	8,065	854.6	...	...	...	...	...	...	...	.065
(SE)	...	(140.4)	...	...	...	...	...	...	...	...
Beta	...	.25	...	...	...	...	...	...	...	...
Eq. (2):										
B	−3,468	855.8	54.5	78	176	−157	28	−512	2.7	.349
(SE)	...	(161)	(11.4)	(21)	(31)	(170)	(14)	(166)	(.37)	...
Beta	...	.26	.21	.17	.23	−.05	.09	−.13	.25	...

TABLE 2 (*Continued*)

Dependent Variable (Total Annual Taxable Income)	Unadjusted Constant	Education	Occupational Status	Age (Years)	Seniority (Years)	Father's Education	Father's Occupational Status	Parents' Economic Condition	Annual Hours Worked	R^2
Managers (N=479):										
Eq. (1):										
B	6,481	2,081.6	...	...	...	...	...	...	...	.155
(SE)	...	(222)	...	...	...	...	...	...	...	...
Beta	...	.39	...	...	...	...	...	...	...	...
Eq. (2):										
B	−6,903	1,402.7	115.8	184	80	−379	29	−291	1.5	.339
(SE)	...	(258)	(20)	(37)	(48)	(216)	(18)	(246)	(.60)	...
Beta	...	.27	.27	.24	.07	−.08	.07	−.05	.10	...

B. COMPARISON OF RETURNS TO EDUCATION

	Workers Compared to Managers and Supervisors		Workers Compared to Managers Only		Workers Compared to Supervisors Only	
	Eq. (1)	Eq. (2)	Eq. (1)	Eq. (2)	Eq. (1)	Eq. (2)
Difference in education coefficients	838	514	1,231	748	4	201
t-value of difference	8.2*	4.3*	9.9*	5.1*	<1	1.7
Workers slope as % of other	50	56	41	47	99	77

[a] The Ns are the number of actual cases used in the regression (unweighted N). The regression coefficients are estimated using the weighted sample; the standard errors are calculated on the basis of the unweighted N.

* Significant at the .001 level (one-tailed test).

inspection of figure 2 and the closeness of the R^2 using the dummy variables and a single scale in table 3 indicate that the relationship of credentials to income is reasonably linear within each class category. The differences in slopes thus cannot be interpreted as artifacts of a single curvilinear relationship between income and credentials for both classes.

Hypothesis 2: Black males will be more concentrated in the working class than white males.—Table 4 gives the class distribution of black and white males based on an average of the 1969 Survey of Working Conditions and the 1973
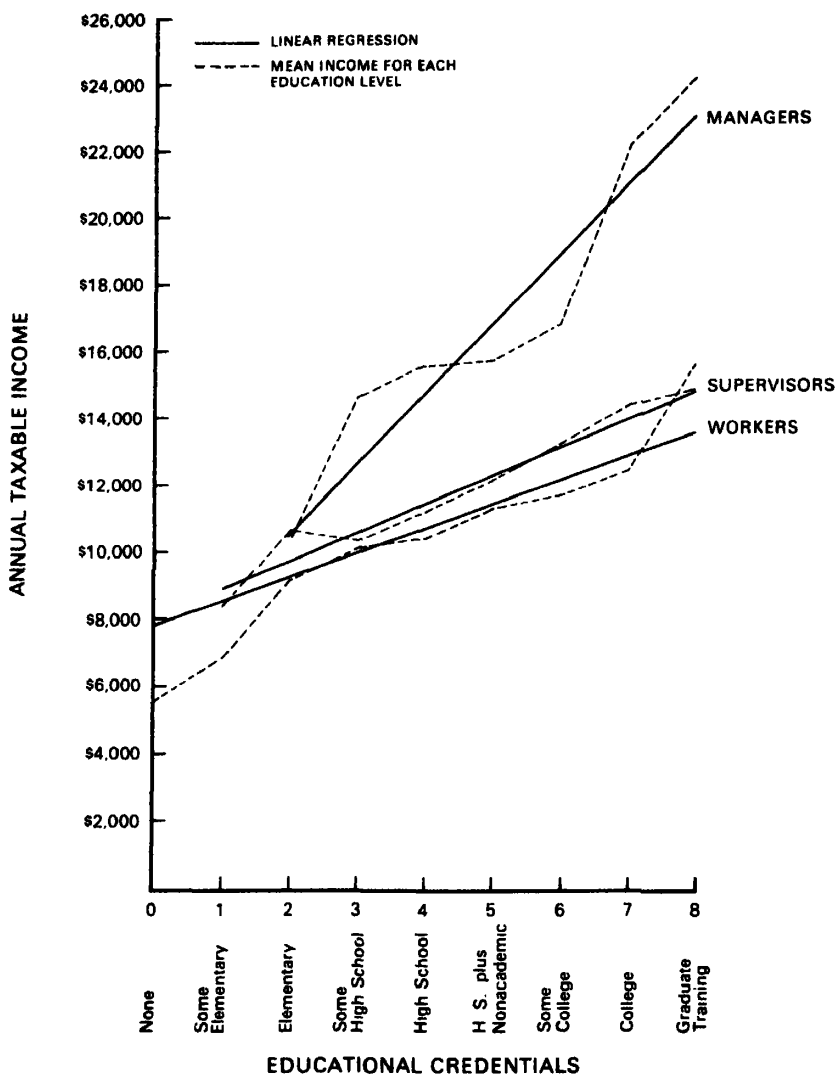
FIG. 2.—Relationship of income to education for workers, supervisors, and managers

TABLE 3

COMPARISON OF EXPLAINED VARIANCE USING EDUCATION SCALE AND EDUCATION DUMMIES

(Annual Income = Dependent Variable)

	R^2 Using Education Credential Scale	R^2 Using Education Dummies
Equation for:		
Workers	.066	.069
Managers/supervisors	.135	.154
Supervisors	.065	.065
Managers	.155	.180

Quality of Employment Survey and the distribution for the panel study. Because of the problems of nonrandomness in the panel study, the average of the two earlier surveys is undoubtedly a more accurate estimate of the actual class distribution of races. It is clear from these results that black males are indeed more concentrated in the working class than white males: 61% of all black males compared with only 40% of white males fall within the working class.[10]

Hypothesis 3: When class position is ignored, black males will receive lower returns to education than white males.—Table 5 presents the regression equations for all blacks and all whites, for black and white workers, for black and white supervisors, and for black and white managers. Figure 3 graphically presents the results for the simple regression of income on education. Table 6 presents the statistical tests for significance of the differences in returns to education for various race-class comparisons. In the simple regression of income on education, white males receive $1,419 for each increment in education; black males receive only $860. When the controls in equation (2) are added, the returns among white males decrease to $1,147 and among black males to $614. Both of these differences are significant at the .001 level. Thus, as in most studies of black-white differences in returns to education, black males as a whole do receive lower returns than white males as a whole.

Hypothesis 4: Within the working class, the returns to education for black and white males will be much more similar than for all blacks and all whites.—In the simple regression of income on education, the returns for all black males are less than one-half of the returns for all white males; within the working

[10] If anything, these figures underestimate the proportion of the black male population in the working class, since unemployed persons are excluded from both the Survey of Working Conditions and the Quality of Employment Survey. If one assumes that most unemployed black males belong in the working class, then the actual proportion of black males who are workers would probably be closer to 70% or 75%, and of white males closer to 45% or 50%.

TABLE 4

Class Distribution within Race Categories

	Average of Survey of Working Conditions, 1969, and Quality of Employment Survey, 1973, Distribution		Panel Study of Income Dynamics, 1975[a]					
			White Males			Black Males		
				Weighted	Unweighted		Weighted	Unweighted
	White Males	Black Males	%	*N*	*N*	%	*N*	*N*
Employers	11.5	4.9	10.9	302	217	4.1	11	31
Supervisors and managers	40.2	32.5	39.3	1,090	812	24.7	66	198
Supervisors	...	...	18.9	524	401	13.1	35	126
Managers	...	...	20.4	566	411	11.6	31	72
Workers	43.5	61.4	44.2	1,225	984	69.7	186	657
Petty bourgeoisie	4.9	1.2	5.5	153	117	1.5	4	15
Total	100	100	100	...	...	100	...	...
N	2,100	168	...	2,770	2,130	...	267	901

[a] Percentages are calculated on the basis of the weighted *N*. The weights are designed to correct for oversampling in the original sample design and differential attrition rates during the first four waves of the panel.

TABLE 5

REGRESSION EQUATIONS WITHIN RACE-CLASS CATEGORIES WITH ANNUAL TAXABLE INCOME AS DEPENDENT VARIABLE

	Unadjusted Constant	Education	Occupational Status	Age	Seniority	Father's Education	Father's Occupational Status	Parents' Economic Condition	Annual Hours Worked	R^2
Whites (N = 2,145):[a]										
Eq. (1):										
B	5,583	1,818.6	...	...	...	...	...	...	...	.087
(SE)	...	(127.1)	...	...	...	...	...	...	...	...
Beta	...	.30	...	...	..	...	...	...	...	...
Eq. (2):										
B	−10,519	1,147.2	120.4	146	159	−232	6	329	3.1	.268
(SE)	...	(158.0)	(11.2)	(21)	(29)	(149)	(13)	(158)	(.31)	...
Beta	...	.19	.26	.17	.13	−.04	.01	.04	.18	...
Blacks (N = 912):										
Eq. (1):										
B	6,069	860 2	..	...	...	...	...	...	...	.080
(SE)	...	(96.8)	...	...	..	...	...	...	...	...
Beta	...	.28	...	...	...	...	...	...	...	...
Eq. (2):										
B	−5,273	64.1	78.4	100	30	410	−26	−135	2.9	.376
(SE)	...	(119)	(8.9)	(14)	(22)	(125)	(10)	(103)	(.24)	...
Beta	...	.21	.30	.24	.04	.11	−.08	−.04	.33	...
White workers (N = 984):										
Eq. (1):										
B	7,657	802.6	...	...	...	...	...	...	...	.055
(SE)	...	(105.9)	...	...	...	...	...	...	...	...
Beta	...	.235	...	...	...	...	...	...	...	...
Eq. (2):										
B	−6,639	656.4	64.8	128	136	238	−39	340	3.2	.359
(SE)	...	(125.7)	(9.3)	(15)	(25)	(118)	(10)	(120)	(.26)	...
Beta	...	.19	.24	.28	.17	.07	−.12	.08	.19	...

TABLE 5 (*Continued*)

	Unadjusted Constant	Education	Occupational Status	Age	Seniority	Father's Education	Father's Occupational Status	Parents' Economic Condition	Annual Hours Worked	R^2
Black workers (N = 657):										
Eq. (1):										
B	6,246	610.4	...	...	...	...	...	...	...	.052
(SE)	...	(101.7)	...	...	...	...	...	...	...	...
Beta	...	.23	...	...	...	...	...	...	...	...
Eq. (2):										
B	−4,048	649.0	76.9	83	64	122	−62	113	2.8	.359
(SE)	...	(136.7)	(11.0)	(15)	(24)	(139)	(12)	(103)	(.26)	...
Beta	...	.24	.29	.25	.11	.03	−.19	.04	.36	...
White supervisors (N = 397):										
Eq. (1):										
B	8,827	734.3	...	...	...	...	...	...	...	.045
(SE)	...	(170.3)	...	...	...	...	...	...	...	...
Beta	...	.21	...	...	...	...	...	...	...	...
Eq. (2):										
B	−2,163	751.2	56.4	73	174	−189	−30	−570	2.7	.343
(SE)	...	(192.3)	(13.5)	(24)	(37)	(200)	(16)	(197)	(.44)	...
Beta	...	.22	.22	.16	.23	−.05	.10	−.14	.26	...
Black supervisors (N = 123):										
Eq. (1):										
B	6,334	641.5	...	...	...	...	...	...	...	.083
(SE)	...	(194.1)	...	...	...	...	...	...	...	...
Beta	...	.29	...	...	...	...	...	...	...	...
Eq. (2):										
B	−2,782	966.9	10.0	84	95	279	16	−405	1.7	.230
(SE)	...	(309.6)	(21.5)	(49)	(57)	(275)	(25)	(267)	(.63)	...
Beta	...	.43	.05	.21	.17	.12	.07	−.14	.24	...

TABLE 5 (*Continued*)

	Unadjusted Constant	Education	Occupational Status	Age	Seniority	Father's Education	Father's Occupational Status	Parents' Economic Condition	Annual Hours Worked	R^2
White managers (N = 405):										
Eq. (1):										
B.........	6,429	2,107.4	...	...	...	...	...	...	...	.153
(SE).......	...	(247.0)	...	...	...	...	...	...	...	...
Beta.......	...	.39	...	...	...	...	...	...	...	...
Eq. (2):										
B.........	−7,794	1,480.7	118.1	189	109	−400	28.9	−285	1.5	.350
(SE).......	...	(281.0)	(21.5)	(39)	(52)	(231.5)	(19.5)	(264.5)	(.67)	...
Beta.......	...	.27	.27	.24	.10	−.08	.07	−.05	.09	...
Black managers (N = 72):										
Eq. (1):										
B.........	7,628	1,168.5	...	...	...	...	...	...	...	.134
(SE).......	...	(354.2)	...	...	...	...	...	...	...	...
Beta.......	...	.37	...	...	...	...	...	...	...	...
Eq. (2):										
B.........	−1,005	−510.5	57.4	135.4	−235.8	550.1	104.4	−167	2.96	.517
(SE).......	...	(475)	(27.6)	(75.6)	(89.9)	(436.5)	(35.5)	(591.3)	(.89)	...
Beta.......	...	−.16	.27	.26	−.36	.16	.40	−.03	.41	...

[a] The Ns are the number of actual cases used in the regression (unweighted N). The regression coefficients are estimated using the weighted sample; the standard errors are calculated on the basis of the unweighted N. The discrepancies between the Ns in this table and those in table 4 are due to missing data.
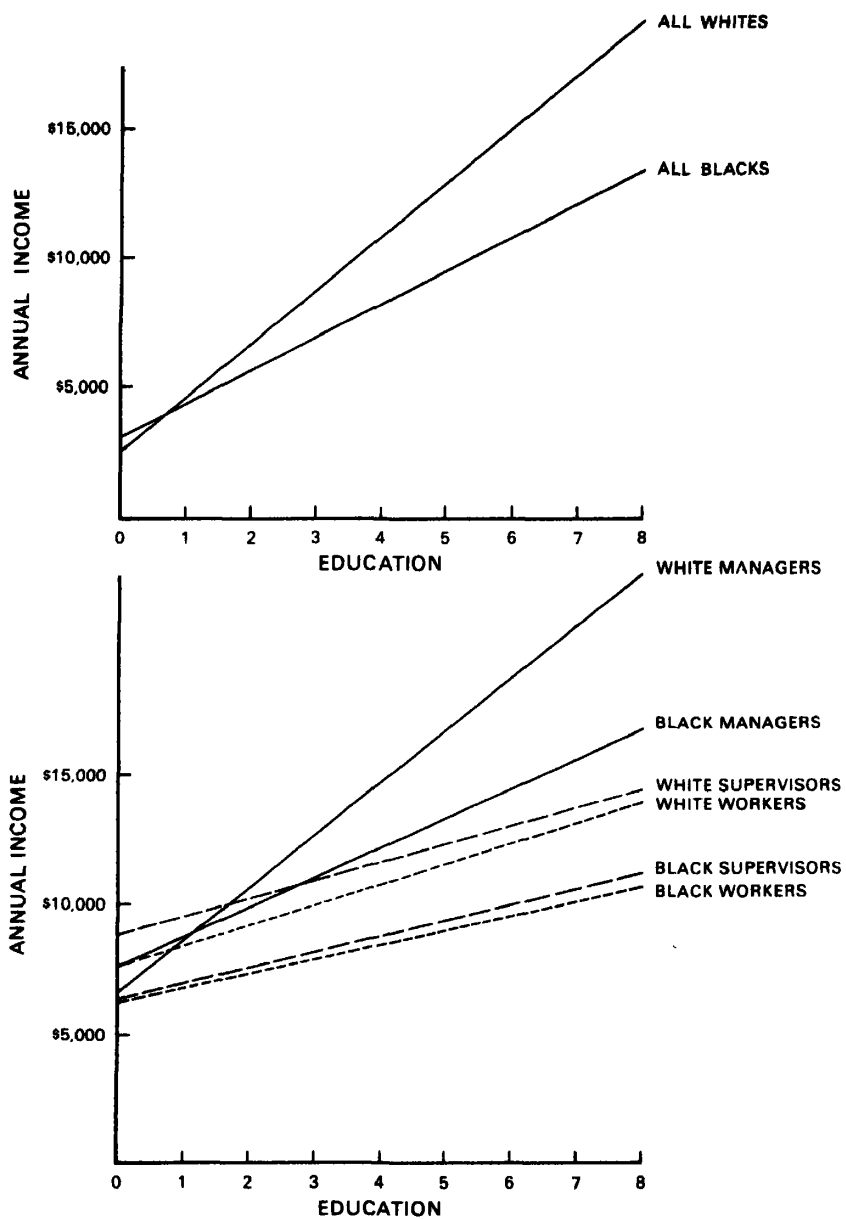
FIG. 3.—Returns to education for blacks and whites in different class positions

TABLE 6

COMPARISON OF RETURNS TO EDUCATION FOR BLACK AND WHITE MALES ACROSS CLASS CATEGORIES AND WITHIN CLASS CATEGORIES

	RETURNS TO EDUCATION	
	Eq. (1)	Eq. (2)
All black and white males:		
Slope difference	959	533
t-value of difference	8.1*	3.6*
Black slope as % white slope	49	54
% difference in eq. (1) eliminated by the controls in eq. (2)	...	44
Workers:		
Slope difference	192	7
t-value of difference	1.8	<1
Black slope as % white slope	76	99
% difference in eq. (1) eliminated by the controls in eq. (2)	...	97
Supervisors:		
Slope difference	93	−216
t-value of difference	<1	<1
Managers:		
Slope difference	938	1,991
t-value of difference	3.5*	6.3*

* Significant at the .001 level (one-tailed test).

class, on the other hand, the returns for black males are slightly over 75% of the returns for white males. What is more, when the controls in equation (2) are added, the returns for black male workers become virtually identical to the returns for all white workers, while the returns for all black males are still only 54% of the returns for all white males. This suggests that, within the working class, most of the difference between black and white males in returns to education observed in the simple regression of income on education is mediated by the control variables in equation (2), whereas this is not the case for all blacks and all whites.

Hypothesis 5: Within the supervisor category, the returns to education for black and white males will be more similar than for all blacks and all whites.—As in the case of black and white workers, this expectation is strongly supported by the results. In the simple regression, black male supervisors receive only $93 less returns to education than do white male supervisors, and in equation (2) they actually receive returns $200 greater (although the difference is statistically insignificant).

Hypothesis 6: Within the managerial category, black males will have lower returns to education than white males.—As predicted, in both regression equations black male managers receive significantly lower returns to education than white male managers. However, it was not expected that the returns to education among black male managers would be essentially zero

in the multiple regression equation. The expectation was merely that, because of restrictions of blacks to lower levels of the authority hierarchy, the hierarchical-promotion mechanism would be blunted among black managers, and thus the returns to education would be less among black than among white male managers. There was no a priori expectation that those returns would disappear entirely in equation (2).

One possible clue to these results might be found in the occupational distribution among white and black managers (table 7). As would be expected, black male managers are considerably more concentrated than white male managers among unskilled and semiskilled manual occupations (38.5% compared with 13%, respectively). What is somewhat surprising is the much higher proportion of black managers who are teachers compared with white managers (13% compared with 2%, respectively). Expressed in a different way, nearly 60% of the black managers in professional or technical occupations are teachers as compared with less than 10% of white professional-technical managers. Remember, these are proper managers, people who, unlike mere supervisors, state that they have some say in the pay and promotions of their subordinates. This implies that teacher-managers either occupy administrative positions within their educational institutions or direct research projects in which they authorize the pay and promotions of research staff (all but one of the teacher-managers were college or university teachers).

If the regressions in table 5 are rerun excluding teachers from the managerial category, the results conform much more to expectation (table 8). Black male managers still have significantly lower returns to education than do white male managers, but the returns are not nearly as small as returns in the regressions that included teachers.

I cannot offer a particularly coherent explanation for why the presence of

TABLE 7

OCCUPATIONAL DISTRIBUTION AMONG MANAGERS FOR BLACK AND WHITE MALES (%)

	White Males	Black Males
Professional, technical, and kindred	24.6	22.5
Professionals	19.3	9.3
Technicians	3.0	0
Teachers	2.3	13.2
Managers and administrators	35.2	25.0
Sales	4.7	.8
Clerks	1.9	1.7
Craftsmen and kindred	20.5	11.5
Operatives, laborers, and miscellaneous	13.1	38.5
Total	100	100

TABLE 8

RETURNS TO EDUCATION FOR BLACKS AND WHITES WITHIN THE MANAGER/SUPERVISOR CATEGORY, EXCLUDING TEACHERS

	TOTAL ANNUAL INCOME	
	Eq. (1)	Eq. (2)
Supervisors:		
White males	732	761
	(182)	(208)
Black males	506	934
	(217)	(350)
Difference	226	−173
t-value	1.1	<1
Managers:		
White males ($N=385$)	2,154	1,570
	(256)	(293)
Black males ($N=60$)	1,582	880
	(476)	(473)
Difference	572	690
t-value	2.0*	2.1*

NOTE.—Numbers in parentheses are standard errors.
* Significant at .05 level.

so many teachers among black managers should have such a drastic effect on the education coefficient in equation (2). Most likely, this result has something to do with the interrelationship of education, occupational status, and income among this specific subgroup of managers. To say this, however, merely describes the problem; it does not provide a theoretical explanation.

DISCUSSION: THE INTERPLAY OF RACISM AND CLASS RELATIONS

It would be a mistake to interpret these results as indicating that all racial discrimination is really disguised class oppression. While it is true that the differential returns to education for blacks and whites largely disappear when we control for class (except in the case of managers), this does not imply that race is an insignificant dimension of inequality in American life. The empirical and theoretical problem is to sort out the complex interplay of racism and class relations, not to absorb the former into the latter.

The most obvious way in which racism intersects class relations is in the social processes which distribute people into class positions in the first place. In recent years, sociologists have devoted considerable attention to the effects of racial discrimination on occupational mobility chances of blacks as compared with whites. To my knowledge, there have been no studies which systematically explore the role of racism in the distribution of individuals into different positions within the social relations of production. Of particular importance in such a study would be the social processes which

select people into the managerial/supervisory category and the mechanisms which regulate the promotion patterns in managerial hierarchies. Racism would affect the distribution of races within authority structures in two general ways. First, as in the sorting process for occupations, various forms of racial discrimination affect access to the mechanisms which sort people into the managerial hierarchy (educational credentials, connections, etc.). Since, as was argued earlier, people with lower credentials will tend not to be promoted above people with higher credentials, the result will be a higher concentration of blacks at lower levels of the managerial structure and a higher concentration of blacks in the working class. Second, and perhaps more important, because of the necessity to legitimate the social relations of domination embodied in managerial hierarchies, racism will tend directly to prevent the promotion of blacks above whites. Of course, this does not mean that blacks will never be promoted above whites. Particularly when strong political struggles against racism occur, corporations and bureaucracies may see the imperatives of legitimation as requiring the acceptance of some blacks into "token" positions of authority within managerial structures. But in the absence of such struggles, it would be expected that the logic of hierarchical domination within capitalist production relations and the necessity of legitimating that domination would generate racist patterns of recruitment into and promotion up managerial hierarchies.

The above argument about recruitment and promotion presupposes the existence of racism. Given the presence of intense racist beliefs, it is easy to explain why blacks will not be promoted above whites within hierarchies; but this begs the question about the existence of racism in the first place. It is beyond the scope of this paper to attempt a systematic account of the origins of racism and the social processes which reproduce it in contemporary American society. What I will do is very briefly indicate the essential thrust of a class analysis of the role of racism in American capitalism and show how the present study relates to that analysis.

A common mistake made by Marxists in analyzing racism is to assume that all forms of racial discrimination are unequivocally functional for the capitalist class. This is similar to analyses of the capitalist state which argue that every policy of the state is orchestrated by the capitalist class to serve its interests. Such "instrumentalist" views of the state and ideology minimize the intensely contradictory character of capitalist society.[11] Capitalism simultaneously undermines and reproduces racism, and it is essential to disentangle these two tendencies if one is to genuinely understand the relationship between class and race in contemporary capitalism.

One of the basic dynamics of capitalist development stressed by Marx, as

[11] For a critique of instrumentalist views in Marxist theory, see Gold, Lo, and Wright (1975) and Esping-Anderson, Friedland, and Wright (1976).

well as many non-Marxist theorists, is the tendency for capitalism to transform all labor into the commodity labor power and to obliterate all qualitative distinctions between different categories of labor. From the point of view of the capital accumulation process, the more labor becomes a pure commodity, regulated by pure market principles unfettered by personal ties and ascriptive barriers, the more rapidly can capitalism expand. In terms of the logic of accumulation developed by Marx in *Capital*, there will therefore be systemic tendencies in capitalism to reduce racial discrimination in the labor market and to treat black labor power as identical with any other labor power.

But this is only one side of the story. Capitalism is not just a system of capital accumulation; it is also a class system in which workers struggle against capitalists, both over their condition as sellers of labor power and potentially over the existence of the capitalist system itself. Whereas the essential dynamics of accumulation may lead to an undermining of racial differences in the labor market, the dynamics of class struggle tend to intensify racism. To the extent that the working class is divided along racial and ethnic lines, the collective power of the working class is reduced, and thus the capacity of workers to win demands against capital is decreased. The result will be an increase in the rate of exploitation of both white and black workers, although the effects may well be more intense for blacks and other minorities than for whites.[12] As a divide-and-conquer strategy, racism thus serves the interests of capitalists, both as individuals and as a class.[13]

We thus have a basic contradiction: capitalism tends to undermine all qualitative distinctions between categories of labor, but the capitalist class needs those qualitative divisions for its own reproduction as the dominant

[12] While Marxists have often claimed that racism hurts white as well as black workers, until recently there have not been any systematic empirical investigations of this proposition. Two recent studies deal directly with this question. Reich (1971, 1973) shows that, in the 50 largest SMSAs, the greater the racial inequality in median family earnings, the greater the inequality of earnings among whites (as measured by the Gini coefficient for earnings among whites) and the weaker the level of unionization. Szymanski (1976) shows that, for the 50 states, the greater the inequality between black and white median earnings, the lower the median earnings of white males and the greater the inequality among whites (again, measured by Gini coefficients). Both of these sets of results indicate that racism, in dividing the working class, leads to an increase in exploitation of all workers.

[13] The analysis of racism as a divide-and-conquer strategy has been perhaps the central theme in Marxist treatments of the subject. Marx emphasized this issue in his various discussions of the "Irish Question." In 1870, for example, he wrote, "The English bourgeoisie has not only exploited the Irish poverty to keep down the working class in England by forced immigration of poor Irishmen, but it has also divided the proletariat into two hostile camps. . . . In all the big industrial centers in England there is profound antagonism between the Irish proletariat and the English proletariat. The average English worker hates the Irish worker as a competitor who lowers wages and the standard of life. . . . This antagonism among the proletarians of England is artificially nourished and supported by the bourgeoisie. It knows that this scission is the true secret of maintaining its power" (Marx and Engels 1972, p. 162).

class.[14] Both forces operate. The actual balance between the two depends upon a variety of historical factors. For example, under conditions of extreme shortages of labor, obstacles to labor mobility in the labor market are likely to be rather costly to individual capitalists, and thus it would be expected that racial barriers would be more rapidly eroded by imperatives of accumulation. On the other hand, when the supply of labor is relatively abundant and when individual differences between laborers make little difference to productivity (because of routinization, automation, etc.), those strictly economic imperatives are likely to be weaker. The extent to which racial or ethnic divisions within the working class are being deepened or eroded in a given capitalist society cannot therefore be derived directly from the abstract theory of capitalist economic development. It is only when such abstract theory is linked to specific political and ideological developments that it becomes possible to assess the real dynamics of racism in a given society.

While the present study does not deal with this historical process, the data in the research nevertheless can be related to both sides of this contradictory tendency within capitalism, that is, both to the perpetuation of important racial divisions within the working class and to the common situation of all workers as workers, regardless of race.

The data presented in figure 3 and table 5 clearly indicate that, while black and white workers receive similar returns to education, black-worker income is less than white-worker income at every level of education. One way of assessing this gap in income is to see what the expected difference in income between a black and white male worker would be if they both had some intermediate value on the independent variables included in the equation. In table 9 this gap is calculated at levels of the independent variables halfway between the means for each group in the comparison being made.[15] As can be seen from this table, the income gaps between races are large and statistically significant for both regression equations within each class category.

[14] Both Marxist theory and neoclassical economics recognize that the inherent economic logic of capitalism is to reduce progressively economic divisions between races, in terms of both income and occupation. The difference (in these terms) between the two perspectives is that neoclassical economics treats capitalism solely as an economic system and ignores the fundamental class antagonisms within that system. Thus, the political and ideological imperatives of controlling the working class play no role in the theory. Instead of seeing the relationship of capitalism to racism as an intrinsically contradictory process, neoclassical economics typically treats racism as a problem of individual "tastes" for discrimination on the part of employers and workers (see Becker 1971, pp. 13–18).

[15] Thus, in the simple regression of income on education for the comparison of black and white workers, the income gap is assessed at a value of education equal to $(\bar{E}_{\text{black worker}} + \bar{E}_{\text{white worker}})/2$. The statistical significance of this income gap can be tested in a way exactly analogous to the test of slope differences, only in this case a *t*-test is performed on the differences between constant terms adjusted to the appropriate values of the independent variables (see Wright 1976*b*, pp. 55–57).

Furthermore, the addition of the various controls in the multiple regression equations reduces the total difference in mean incomes between races within classes by no more than 50%, indicating that a substantial part of the difference in mean incomes between races within classes should probably be directly attributed to racial discrimination. In terms of Marxist theory, these results strongly suggest that black workers are exploited at a higher rate than white workers and that racism has generated real, material divisions between races within the working class.[16]

TABLE 9

INCOME GAPS[a] BETWEEN RACES WITHIN CLASS CATEGORIES

	Mean Income ($)	Eq. (1)[b]	Eq. (2)[b]
All respondents:			
Gap in income	5,308	3,698	1,868
Black expected income as % of white expected income	64	73	85
% difference in means eliminated by controls	...	30	65
t-value of gap	...	16.4*	8.1*
Workers:			
Gap in income	2,870	2,203	1,428
Black expected income as % of white expected income	75	80	86
% difference in means eliminated by controls	...	23	49
t-value of gap	...	11.9*	8.9*
Supervisors:			
Gap in income	3,872	2,896	2,140
Black expected income as % of white expected income	69	76	82
% difference in means eliminated by controls	...	25	45
t-value of gap	...	8.6*	6.8*
Managers:			
Gap in income	5,051	3,707	3,011
Black expected income as % of white expected income	72	79	83
% difference in means eliminated by controls	...	27	40
t-value of gap	...	8.0*	6.8*

[a] Income gaps represent the difference in expected incomes for two groups evaluated at a level of the independent variables in the regression equal to the average of their respective means on the independent variables.

[b] Independent variables in eq. (1) = education only; independent variables in eq. (2) = education, age, seniority, background, occupational status, and annual hours worked.

* Significant at the .001 level (one-tailed test).

[16] In order to interpret these results as indicating a higher rate of exploitation of black labor, it is necessary to assume that two workers who have the same values on all of the variables in eq. (2) will have essentially similar complexities of labor (i.e., embodied labor in their own labor power) and intensities of labor (i.e., pace of work within the labor process). Since eq. (2) contains annual hours worked, if we accept the above two assumptions then two workers who have the same values on all the independent variables will produce the same amount of total value in a year. Any difference in their incomes would

Ultimately, the political thrust of the Marxist theory of racism hinges on the other side of the contradiction: that, in spite of the divisive character of racism and in spite of the material differences between black and white workers which racism generates, workers of all races nevertheless share a fundamental class situation and thus share fundamental class interests.[17] The central finding of this study—that black and white workers have very similar returns to education—reflects this common class situation. Table 5 also indicates that black and white workers have very similar income returns to occupational status (whereas whites taken as a whole have significantly greater returns than blacks taken as a whole), which again reflects the commonality of their class position.

Furthermore, it is easy to show that the income gaps between black and white workers, while significant, are much smaller than the gaps between either black or white workers and small employers. Figure 4 indicates the expected incomes of each race and class category assessed at the level of the independent variables of white employers.[18] The unadjusted mean income of white workers is less than one-half that of white employers; the mean income of black workers, on the other hand, is 75% of white workers' mean income. In absolute dollar amounts, the mean white workers' income is over $14,000 less than the mean white employers' income, whereas the mean black workers' income is only $2,900 less than the mean white workers'

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then reflect differences in the costs of reproduction of their labor power and thus differences in rates of exploitation. Two factors could undermine this conclusion. First of all, the reproductive costs of labor power are not represented simply by wages, but by fringe benefits, state subsidies for education and other services, etc. To the extent that such additional elements of the costs of reproducing labor power are themselves correlated with earnings, we are, if anything, underestimating the differences in rates of exploitation by looking exclusively at direct income. Second, if black workers as a whole are overqualified for the jobs which they hold, wage differences could reflect in part social waste of potential surplus value rather than superexploitation of black labor in the technical sense of the concept. If a college graduate works on an assembly line, he or she produces no more surplus value and is no more exploited than a high school dropout in the same production process. The complexity of the college graduate's potential labor is being socially wasted in such a situation. In the strategy used to compare black and white exploitation above, however, this situation would appear as a greater rate of exploitation of the college graduate. There is no way, in the present data, to differentiate between underemployment and more intense exploitation. For a much fuller discussion of the relationship between econometric models of income determination and the Marxist concept of exploitation, see Wright (1976*b*, pp. 120–31).

[17] "Fundamental" class interests refer to interests defined across modes of production (i.e., interests in capitalism vs. interests in socialism), whereas "immediate" interests refer to interests defined within a given mode of production. Black and white workers may well have conflicting immediate interests under certain circumstances (as do many categories of labor within the working class) and still share fundamental class interests. For a discussion of the importance of the distinction between fundamental and immediate interests, see Wright (1978, chap. 2).

[18] This procedure is basically similar to the familiar cross-substitution technique employed by Duncan (1969) and others.
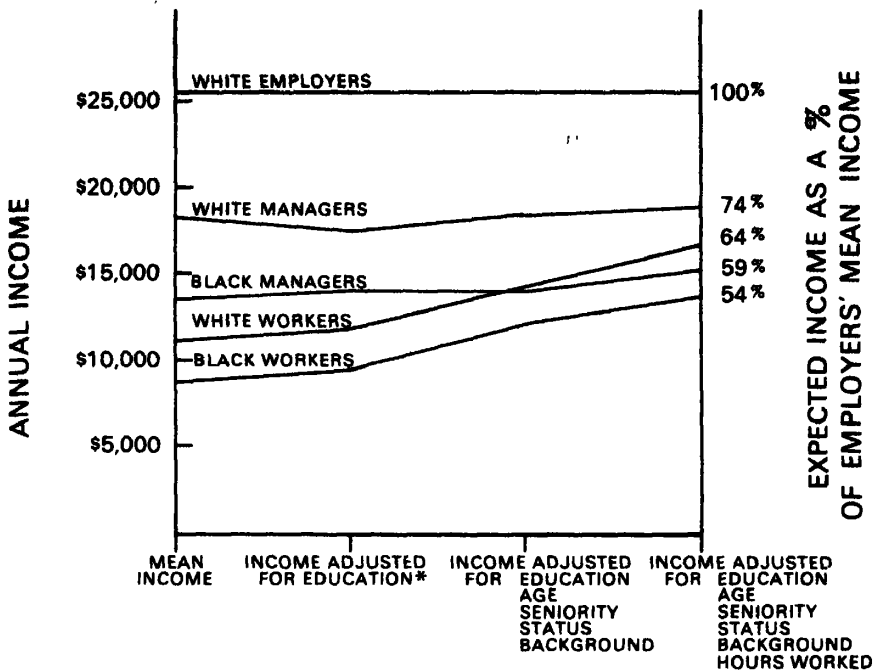
FIG. 4.—Comparison of income differences between races and classes

income. When the various controls in equation (2) are added, the expected incomes of both black and white workers (evaluated at the white employers' means on the independent variables) increase considerably. Yet, the difference between workers and employers is still considerably greater than the differences between workers of different races. These results indicate that compared with even small employers—let alone proper capitalists—the common position of black and white workers within the social relations of production generates a basic unity of economic situation.

The results of this study have important implications for both Marxist and non-Marxist social scientists. For Marxists, they suggest the fruitfulness of using quantitative techniques in exploring the interactions of class and race, especially when social relations of production are explicitly included in the research. Future work should attempt to examine longitudinal changes in the interactions of class and race in the income-determination process in order to specify more precisely the contradictory logic of the relationship between capitalism and racial divisions discussed above. In such research, it is particularly important to examine changes in racial inequality within

the working class (rather than globally within the population as a whole) in order to be able to assess the relative role of the accumulation and social control imperatives in shaping patterns of racial inequality in contemporary American capitalism.

For non-Marxist social scientists, the results of this study demonstrate the importance of social relations of production in understanding social inequality. At least in the study of racial differences in the income-determination process, the exclusion of class relations from the analysis leads to basic distortions in the results. For many reasons, Marxist categories have rarely been included in social surveys and virtually never in census surveys. While the findings of this study hardly establish the overall validity of the Marxist paradigm, they do indicate that any serious study of social inequality must attempt to measure social relations of production.

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On the Justice of Earnings: A New Specification of the Justice Evaluation Function[1]

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This paper describes a new specification of the justice evaluation function. It predicts precisely and unambiguously the kind of injustice (i.e., underreward or overreward) and the degree of injustice associated with given departures from perfect justice. The new justice evaluation function was inducted from extensive analyses of survey data on the perceived justice or injustice of earnings. It is directly generalizable to cover all socially distributed goods, and hence I propose it as a candidate for a universal Law of Justice Evaluation in distributional matters. Finally, I suggest that such a Law of Justice Evaluation performs one of the three tasks essential to a scientific theory of distributive justice.

This paper reports a new, empirically supported specification of the justice evaluation function, one that simply and so far accurately predicts the precise magnitude of injustice associated with measured departures from perfect justice in the distribution of earned income, and one that can be extended to cover all distributional matters.

Recent empirical work on the perceived justice of earnings (Jasso and Rossi 1977) explores the content of and the degree of consensus about the principles of justice governing earnings, and estimates just earnings amounts for incumbents of a variety of both occupational and other classifications in contemporary American society. Jasso and Rossi's (1977) research design uses judgments about the fairness or relative unfairness of earnings amounts randomly attached to fictitious persons and families in order to infer the just earnings for such units. Their major findings are interrespondent agreement on the principles of justice and a just earnings distribution characterized by small variance, highly restricted range, and monetary returns not only to education and occupation but also to sex and family claims.

In this paper I explore the justice evaluation function by assessing the magnitudes of injustice associated with given deviations from perfect justice. Jasso and Rossi (1977) recognize the existence of a justice evaluation

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variable and integrate it within their research design. They represent it as an Aristotelian dimension spanning the real-number line, with zero, the origin of the line, chosen to represent the point of perfect justice, the positive and negative segments to represent judgments of overreward and underreward, respectively, and the absolute value of the justice evaluation to represent the magnitude of injustice. They report quadratic justice evaluation functions and use them to estimate just earnings.

I suggest here that finding the "true" shape of the justice evaluation function is an imperative task. It is not enough to know the parameters of the just earnings distribution. We live in a world that rarely realizes congruence between actual earnings and just earnings. The major questions of social life are not limited to questions about "what is just." Rather, they include questions about how unjust is this or that aspect of social reality and about the responses of individuals and groups to the observed degree of injustice.

Social scientists have long recognized the importance of the response to injustice (e.g., Homans 1974, 1976; Berger et al. 1972) and have suggested that judging oneself overpaid produces guilt and judging oneself underpaid produces anger. They have not, however, had much to say about responses to judgments about injustice received by others. Moreover, the empirical record shows negligible support for the guilt hypothesis (see Adams and Freedman 1976, pp. 45–46).

I wish to suggest that, before there can be any precision in the determination of responses to the two kinds of injustice, there must be precise measures of the magnitudes of injustice and that unambiguous specification of the justice evaluation function is the immediate, essential work before us.

This paper begins that work. It reports a new quantitative specification of the sense of justice and injustice concerning the distribution of earned income. The specification I believe to be correct was inducted from extensive reanalyses of the survey data gathered by Jasso and Rossi (1977). Its many merits make it immediately a candidate for a universal Law of Justice Evaluation in all distributional matters.

I begin with a look at the requirements for an adequate justice evaluation function and a careful examination of previous formulations. I next describe the data set used in the present research and report the analyses that led to induction of the lawlike relation governing the sense of justice and injustice. Finally, I discuss the justice evaluation function and its implications.

SPECIFYING THE JUSTICE EVALUATION: A TASK IN THE STUDY OF DISTRIBUTIVE JUSTICE

The most ancient version of the problem of distributive justice searches for the one just distribution of resources; it seeks to know "what is just" and is

therefore a problem in normative philosophy. The scientist's version of the problem seeks, rather, to answer three questions: (1) What do people think is just? (2) Do individuals distinguish degrees of injustice, and, if so, how are these related to departures from perfect justice? (3) What is the behavioral response to injustice? This is a problem in descriptive ethics or, in more modern terminology, in scientific explanation.

This paper focuses on the second question. It asks about the sense of injustice that accompanies deviations from the just order. Are there magnitudes of injustice? Is underreward thought to be more unjust than overreward? What is the precise relation between the just reward and the justice evaluation? Not until these questions are answered can the search for responses to injustice yield quantified, precise results.

I now examine the requirements for an adequate specification of the justice evaluation function and then review the existing formulations.

Requirements for a Law of Justice Evaluation

We begin by formalizing the discussion so far, and doing so at the level of the individual and for the distribution of one good. Since the data to be analyzed in this paper concern earnings, we will use earnings as the good in question (while remembering that the formal structure can apply to any other good).

The following terms are used:

1. *Observer*.—This is the person making the judgments about the justice or injustice of the earnings received by any earner, including him or herself.

2. *Just earnings*.—This is the global amount the observer considers just for a given earner. The term is devoid of ontological content; it only represents the observer's judgment, whatever it may be. For example, the observer may judge that just earnings for a movie star are $25,000 a year and for the president of a corporation $100,000 or that just earnings for *all* earners are $8,000 a year.

3. *Actual earnings*.—This is the actual yearly earnings received by any given earner.

4. *Justice*.—This is the condition when an individual's just earnings exactly equal his or her actual earnings.

5. *Injustice*.—This is the condition in which an individual's just earnings and actual earnings are unequal. Injustice is of two kinds: (*a*) unjust underreward—the case when just earnings exceed actual earnings and (*b*) unjust overreward—the case when actual earnings exceed just earnings.

Now, what are the requirements for a Law of Justice Evaluation? What would such a law look like? It can readily be seen that it is easy enough to posit a specification for justice: justice exists whenever actual earnings equal just earnings; in fact, this is true by definition.

What is not so easy is to posit a specification of injustice. A genuine Law of Justice Evaluation must offer a single simple formula that, given two terms only, actual earnings and just earnings, accurately predicts every conceivable evaluation, including perfect justice and degrees of underreward and overreward. That is, our task is to specify the shape of the function (see Homans 1967, p. 18). An adequate justice evaluation function must predict exactly how the justice evaluation will change when the two terms, actual earnings and just earnings, change.

The value of the justice evaluation must be meaningful, independently of the good under consideration. That is, justice and injustice must be expressed in some meaningful, albeit nonfundamental, justice units, such that a given scale value always means the same thing, say, extreme injustice of the overreward kind or perfect justice.

Thus, we have two tasks: (1) to quantify justice and injustice and (2) to specify precisely the function that determines justice and injustice. A proposition that satisfies both these conditions will be regarded as a suitable candidate for a universal Law of Justice Evaluation.

Notice that such a law leaves entirely open the question of what comes to be regarded as just in the first place, a point which I return to later in the paper.

Formulas for the Justice Evaluation Function in the Literature

In this section I review the major works in distributive justice and examine the specifications of the justice evaluation function contained in them.

The work of Homans.—Homans (1974, 1976) followed Aristotle closely and suggested that justice is the result of local comparisons between the reward/investment ratios of individuals. Perfect justice is defined as equality of these ratios across persons. Injustice is defined as inequality of these ratios.

Stated formally, Homans's position, for the case of two individuals, A and B, is

$$\text{justice} \equiv \left[\frac{\text{A's reward}}{\text{A's investment}} = \frac{\text{B's reward}}{\text{B's investment}}\right],$$

which is equivalent to the following two expressions,

$$\text{justice} \equiv \left[\frac{\text{A's reward}}{\text{A's investment}} - \frac{\text{B's reward}}{\text{B's investment}} = 0\right],$$

$$\text{justice} \equiv [(\text{reward}_A/\text{investment}_A)/(\text{reward}_B/\text{investment}_B) = 1] .$$

The specification of justice made by Homans is unambiguous. Whichever of the equivalent expressions given above we use, the results are the same. Perfect justice is defined as zero difference between the two ratios or, in the alternative, as unity when we take the ratio of the two ratios.

However, the Homans proposition has two major weaknesses; both have

been pointed out by Berger et al. (1972), and both indicate its inadequacy as a justice evaluation function. First, it cannot specify the kind of injustice that results when the ratios are not equal; we have no way of knowing whether one person is underrewarded or the other overrewarded. Second, the Homans formulation cannot cope with the possibility that both A and B are unjustly rewarded (i.e., both equally underrewarded or equally overrewarded). Thus, the Homans proposition is of no use in the specification of the sense of injustice and may even be downright wrong in the prediction of justice.

That is, if we express Homans's proposition as a justice evaluation function,

$$\text{justice evaluation} = \frac{\text{A's reward}}{\text{A's investment}} - \frac{\text{B's reward}}{\text{B's investment}},$$

we see that equality of the ratios always produces a value of zero, but one which cannot be uniformly interpreted as justice, and that inequality of the ratios produces a number that not only varies with the position of A's and B's terms but also cannot be consistently interpreted as one or another kind of injustice.

And yet there remains an intuitive appeal in the Homans formulation. The notion of a ratio strikes responsive chords, as evidenced by the many scientists who have followed the pattern set by Homans, among them Adams (1965) and Waltster, Berscheid, and Walster (1976). We shall have occasion to return to this matter later in the paper.

The work of Berger, Zelditch, Anderson, and Cohen.—The model proposed by Berger et al. (1972) defines a situation as unjust if a real-world actor's level of a goal object differs from the level of the goal object received by his or her counterpart in a "referential structure." Berger et al. intuited an intrinsic aspect of the justice evaluation process when they posited a disembodied (Platonic) image of fairness. They observed correctly that the distinction between overreward and undereward is visible only in the presence of such a referential structure.

We can formalize their thinking as follows (for the sake of convenience, I use the term "reward" as shorthand for "level of a goal-object"):

$$\text{justice} \equiv [(\text{actual reward} - \text{just reward}) = 0] .$$

Their specification of an evaluation of justice is both unambiguous and correct. But what about injustice? We now state the proposition of Berger et al. as a justice evaluation function:

$$\text{justice evaluation} = \text{actual reward} - \text{just reward} .$$

If the justice evaluation is zero, the condition of perfect justice obtains, and we can infer that Berger et al. would use zero to represent the point of justice. If the actual reward exceeds the just reward, the justice evaluation

assumes a positive value, and we can infer that they would use the positive segment of the real-number line to represent unjust overreward. If the just reward exceeds the actual reward, the justice evaluation assumes a negative value, and, likewise, we can infer that they would use the negative segment to represent underreward.

Now this is a wonderful achievement. Berger et al. have posited a single relation between two terms that predicts outcomes (i.e., scores for individuals) spanning the full range of the human sense of justice and injustice.

But the proposition of Berger et al. has a major weakness. Whenever the actual reward and the just reward are not equal, that is, whenever the justice evaluation process results in the sense of injustice, the value of the justice evaluation appears in the reward's units and not in meaningful justice units. That is, what should be a magnitude of injustice is, instead, a positive or negative magnitude of the good whose distribution is under evaluation. If, say, actual earnings are $10,000 and just earnings are $8,000, the justice evaluation equals $2,000.

As noted earlier, the justice evaluation must be in justice units, units that are meaningful independently of the particular reward or good under consideration.

Moreover, there is an additional weakness in the formula of Berger et al. Whenever the absolute differences between actual earnings and just earnings are equal, the "injustice," albeit in wrong units, is equal. And this outcome appears to violate the human experience that deficiency is felt to be more unjust than a comparable excess.

And yet, the proposition of Berger et al. also has great intuitive appeal. I recognize the fidelity to common experience contained in the referential comparisons. And I see much merit in the conceptualization of injustice as some sort of difference between an actual reward and a just reward. We shall return to their formulation later in the paper.

Variants of the classical propositions.—In this section we will examine two formulations, one proposed as part of theory and descended from Homans (via Adams), the second inducted from empirical research and related to the formula of Berger et al.

a) The work of Walster, Berscheid, and Walster: Walster et al. (1976, pp. 2–4) offer a specification of justice founded on the Homans and Adams ratios (note: they use the terms "equity" and "inequity" as synonyms for "justice" and "injustice"):

> We define an "equitable relationship" to exist when . . . all participants are receiving equal relative outcomes. . . . [W]e have chosen the following formula for calculating whether or not a relationship is equitable:
>
> $$\frac{O_A - I_A}{(|I_A|)^{k_A}} = \frac{O_B - I_B}{(|I_B|)^{k_B}}$$

where O designates a scrutineer's perception of A's and B's outcomes, I designates his perception of A's and B's inputs, and $|I|$ designates the *absolute value* of A's and B's inputs (i.e., the perceived value of A's and B's inputs, disregarding sign). The exponents k_A and k_B take on the value $+1$ or -1, depending on the sign of A and B's inputs and A and B's gains (Outcomes − Inputs). [$k_A = \text{sign}(I_A) \times \text{sign}(O_A - I_A)$ and $k_B = \text{sign}(I_B) \times \text{sign}(O_B - I_B)$.] A participant's relative outcomes will be zero if his outcomes equal his inputs. His relative outcomes will be positive if his $O > I$, and negative if his $O < I$. Thus, the sign and the magnitude of this measure indicates how "profitable" the relationship has been to each of the participants.

The formula of Walster et al. has strengths and weaknesses. It makes significant progress toward a formula that will produce justice evaluation scores for individuals. Walster et al. do this by showing that each participant has a relative outcomes score of his or her own and that this score can take on values that span the real-number line.

On the other hand, the formula has the same fundamental weaknesses of the Homans ratios. First, it cannot deal adequately with situations in which both (or all) parties are unjustly rewarded. Since they have defined equity as equality of relative outcomes, they still have to call it equity when the participants have either negative or positive equal relative outcomes scores. A second weakness is that it is unduly complicated, as the ad hoc signs of the exponents indicate.

Moreover, even as it travels toward individuals' justice scores, the formula is plagued by the same two problems that befell the scores of Berger et al. First, the scores are in outcome/input units and not in justice (or equity) units (as evidenced by the fact that equity exists whenever the relative outcomes are equal, thereby eliminating the possibility of congruence between a justice scale and the relative outcome dimension). Second, equal differences between outcomes and inputs produce equal absolute magnitudes on the individuals' relative outcomes scores, so the formula again fails to reflect the common opinion that deficiency is more unjust than excess.

b) The work of Jasso and Rossi: In their analyses of the earnings data, Jasso and Rossi (1977) found empirical support for a model remarkably similar to that proposed by Berger et al. Using conventional techniques, they regressed the observed justice evaluations on the attributes of the fictitious units and fit a model which suggests that the justice evaluation process involves three analytic steps. First, the value, in justice units, of actual earnings is calculated. Second, the value, in justice units, of the set of justly perceived claims to earnings is calculated. Third, the justice value of the sum of the claims is subtracted from the justice value of the earnings terms, thus yielding a justice evaluation in justice units.

This formula has strengths and weaknesses. Its main merits are that (1) it translates all value or worth into justice units that are meaningful;

(2) it produces an unambiguous justice evaluation; that is, zero always means justice, a positive number always means unjust overreward, and a negative number always means unjust underreward; and (3) by incorporating a quadratic term for earnings, the formula produces a justice evaluation of greater absolute magnitude for underreward than for overreward.

But it is marred by several major difficulties. First, the best equation Jasso and Rossi were able to fit to the data was additive with a single nonlinearity in actual earnings, a nonlinearity they represented by inclusion of both a linear and a quadratic term for earnings. They did not investigate the possibility that the nonlinearity might be more faithfully represented in other ways.

Second, the Jasso and Rossi formula, by making the regression coefficients of paramount importance, is valid for only one spatiotemporal point. Without the regression coefficients, no prediction of the justice evaluation is possible. Since the slopes estimate the worth of particular claims to particular goods in a particular historical setting, they are ad hoc quantities which do not elucidate any general (theoretical) justice process.

Third, the Jasso and Rossi formula is made unduly complicated by its reliance on regression coefficients and quadratic terms, like that of Walster et al., by the use of ad hoc exponents; indeed, both are reminiscent of the blissful spawning of epicycles that characterized pre-Keplerian astronomy.

Now, if reality is faithfully approximated by their quadratic function, then we must swallow its complexity; if the data on earnings contain no underlying theoretical relation, then we must accept its particularity. But before reaching any conclusion, the data merit reanalysis.

In the remainder of the paper, I reanalyze the Jasso and Rossi data set and indeed find a spartan and general specification of the justice evaluation function and one related to the work of both Homans and Berger et al.

METHOD

A major task of this paper is to describe the empirical analyses that produced the new specification of the justice evaluation function. In this section I first describe the data set and then the mathematical analyses.

The Data

I reanalyzed a set of data collected and first analyzed by Jasso and Rossi (1977). They elicited from a probability sample of 200 respondents a set of almost 12,000 judgments about the justice or relative injustice of the earnings received by 600 distinctly different fictitious individuals and families. The judgments were obtained in household interviews by presenting to the respondents a set of descriptions of the fictitious units (termed "vignettes") and asking them to rate each description on a justice evaluation scale.

The respondent sample.—The respondent sample consisted of 200 white adults residing in private households in Baltimore City. Block quota sampling methods were used to choose the sample. Sex quotas were fixed to ensure that equal numbers of adult females and males were in the sample.

The vignette sample.—The vignette sample was constructed by means of a technique developed by Rossi and described in Rossi et al. (1974). Briefly, the fictitious units are generated by a random combination of preselected personal attributes, the major virtues of the procedure being the randomization of the experimental "stimuli" (i.e., the attributes) and the subsequent minimization of the problem of multicollinearity, and the principal shortcoming being the artificially platykurtic distributions of the variables.

The attributes used to describe the fictitious persons were as follows: (1) sex; (2) marital status, single or married; (3) number of children, zero to six, in increments of one; (4) education, years of formal education ranging from completion of seventh grade to college graduation, in increments of one year; (5) occupation, 96 occupational titles whose prestige scores, standardized by Bose (1973) for the Baltimore area, ranged from 8.0 to 95.8, plus the designation "unemployed and looking for a job" and the sex-restricted title "housewife"; (6) earnings, 15 unequally spaced levels of annual earnings ranging from $2,000 to $40,000 (thus, a married couple may have combined earnings ranging from $2,000 to $80,000); and (7) age held constant at 37 years for males and 35 years for females.

Half the vignettes describe one adult person designated as either married or single but provide no information about the married person's spouse. These vignettes mirror some real-world situations, either of single persons living alone or of married persons about whose spouse one has no knowledge. The other half of the vignettes describe married couples, with all information given for both husband and wife. The vignettes were generated by computer and printed on IBM cards. Restrictions to eliminate nonsensical combinations (e.g., a physician with an eighth-grade education) were imposed a priori. To eliminate the possibility of zero earnings on a vignette, the unemployed and housewife designations were restricted to one such title per married couple.

Ten samples of 60 vignettes each were drawn, for a total of 600 distinctly different vignettes. The samples were drawn so that each contained five vignettes describing single males, five describing single females, 10 each of vignettes describing married males and married females (with no information about a spouse), and 30 vignettes describing married couples. The 10 vignette samples were systematically distributed with equal probability across respondents, each sample being given to 20 respondents, for a possible total of 12,000 judgments. To preclude order effects, each vignette set was well shuffled.

Inclusion of the unemployed and housewife designations resulted in 281

distinctly different two-earner married-couple vignettes and 19 one-earner married-couple vignettes.

The rating task.—Interviews lasting about half an hour were conducted by experienced professional interviewers in early 1974 in Baltimore. Each respondent was handed a set of vignettes and a box with nine slots, each labeled with a number and a description corresponding to a level of the justice evaluation variable, ranging from "−4 Extremely Underpaid," through "0 Fairly Paid," to "+4 Extremely Overpaid." The task given to the respondent was to place each vignette in the slot that corresponded to the respondent's justice evaluation of each vignette.[2] The interviewers reported that the respondents generally approached the rating task with seriousness, a view borne out by the number of ratings made, a total of 11,806 or over 98% of the possible ratings.

Data Analyses

The Jasso and Rossi (1977) research was so designed that it produced data pertinent to two major questions of distributive justice as regards earnings: (1) What is regarded as just earnings? (2) In the respondents' view, how unjust are departures from just earnings? Indeed, answers to the first question are inferred from answers to the second. While in this paper we are mainly interested in the second question, that is, in specifying the justice evaluation function, the route to answering it takes us by the first question. I shall also present my own relevant findings.

Like Jasso and Rossi, I analyze each kind of vignette unit separately, for example, the set of single-person vignettes and the set of married-couple vignettes.

The original analyses, reported in Jasso and Rossi (1977), fitted quadratic regressions to the data. These were described as the best-fitting curves. The use of polynomial regression usually implies that the observed data curve resembles one or another of the polynomial curves of association (e.g., a parabola), and that the regression curve cannot be linearized by some transformation (e.g., root x). Now, scattergrams are essential for curve

[2] The actual wording of the item was as follows: "People and their jobs differ in a lot of ways. We have made up descriptions of different kinds of people and jobs. Here is a box with nine different slots in it. Here is a card with a description of a person and job. (*a*) Please put the card in the slot labelled Fairly Paid if you think that the salary is about right for the person, that is, if you think that the salary on the card is fair payment. (*b*) Put the card in the slot labelled Extremely Overpaid if you think that the money is way too much payment for these people, that is, if, taking everything into account, you think that they're very much overpaid. (*c*) Put the card in the slot labelled Extremely Underpaid if you think that the money is way too little payment for these people, that is, if taking everything into account, you think that they're very much underpaid. (*d*) If you think that the salary is a bit too much or a bit too little, that is, if you think that it belongs somewhere between the most overpaid and the most underpaid, then just put the card in the slot that you think matches the fairness or unfairness of the salary."

fitting (see, e.g., Snedecor and Cochran 1967, pp. 447–71), just as they are for valid interpretation of the arithmetical results of the usual regression computations (see, e.g., Glass and Stanley 1970, p. 127). Since Jasso and Rossi reported no scattergrams, I decided to begin my reanalysis with close scrutiny of the original data. I plotted the observed justice evaluations on the actual earnings for the separate subsets of vignettes. As expected, plots of the raw observations produced relatively uninformative rectangular scatters. However, plots of the mean, median, and modal values of the observed justice evaluations produced curves of a very definite nonlinear shape. For example, a plot of the mean justice evaluation on the 51 observed values of combined earnings for the two-earner married couples produced a curve with a rather steep ascent to the vicinity of zero (the point of fair earnings) at around $20,000 on the X-axis, followed by a long, slow climb to 2.949 at $58,000, next a dip to 1.718 at $68,000, and finally a recovery to 2.525 at $74,000. Because the N's for each mean (of the 51 plotted means) ranged from 17 to 279, there was some uneasiness about the results. But a plot of the mean justice evaluation of each of the 281 distinctly different two-earner married-couple vignettes (the N's for each of the 281 plotted means range from 16 to 20) on the combined earnings again resulted in a peak mean fairness rating at $58,000 and, more important, a similarly shaped curve. The curve itself provides some useful information: dollar increments of fixed size appear to "buy" more underpayment than overpayment.

These curves, plotted for all the subsets of the data, look amenable to representation either by a logarithmic curve or by a second-order polynomial (see Snedecor and Cochran 1967; Guilford 1954). Trying both, I found that fitting a parabola results in substantial distortion, for it is incapable of representing the postpeak recoveries in the justice evaluation that are observed in the data set (as in the case described above).

However, a logarithmic transformation of earnings produces curves that are both highly linearized and of unusual fidelity. Plots of both the mean justice evaluation for the natural logarithm of each value of earnings (for example, in the two-earner married-couple case, $N = 51$) and of the mean justice evaluation for the logarithm of earnings for each distinctly different vignette (e.g., in the same case, $N = 281$) indicate unmistakably that the justice evaluation is linearly related to the logarithm of earnings.

Thus, the plots of the original data furnished one important clue in my search for the "true" underlying justice evaluation function: the justice evaluation is a linear function of the logarithm of actual earnings. But unless all other things are equal, the logarithm of actual earnings is not sufficient to predict justice evaluations. So the search for the rest of the function continued.

Jasso and Rossi (1977) reported statistically significant terms for all the

other vignette attributes. So my next step was to regress the observed justice evaluations on all vignette attributes, with earnings represented solely by their natural logarithm (a more economical model than the two earnings terms required in the quadratic regressions). All quantitative variables were coded in their own units; sex and marital status entered the regressions as dummy variables, with the categories "single" and "male" coded zero; earnings were coded in thousands of dollars.

Table 1 reports the results of these regressions. The coefficients of multiple correlation are extremely close to those for the quadratic regressions (e.g., in the case of all one-person vignettes, the quadratic R^2 was .6616, while the new R^2 is .663), again confirming the view that whatever nonlinearities were in the data and were tapped by the quadratic term are faithfully approximated by the logarithmic transformation of earnings.[3] Moreover, the plots show that the logged equations are more faithful by far than the quadratic ones.

Many additional models were tried on the data, for example, a variety of interaction terms both with and without the log of earnings. No set of interaction terms increased the coefficients of multiple correlation by as much as .01. The equations reported in table 1 represent the absolutely best-fitting curves to the data set.

Now, we think these equations are most faithful, yet we are still seemingly confined to prediction by a set of structural coefficients. If we wish to predict the justice evaluation for a given person of given attributes and given earnings, we literally have to take each attribute, multiply it by its *b* weight, and add the products—a disappointment.

Happily, that initial impression turns out to be wrong. It is not necessary to take a sum of products on a string of attributes in order to predict a justice evaluation. Using algebra, we can restate the same justice evaluation functions reported in table 1 in a variety of equivalent expressions, finding at last the truly superior justice evaluation function—the candidate I am offering for the Law of Justice Evaluations.

In order to make the sequence of equivalent expressions as clear as possible, we begin with what may appear to be a sidetrack but in fact is not, the estimation of just earnings functions. If we wished to employ the equations reported in table 1 to estimate just earnings for particular earners, we would do the following: set the justice evaluation at zero (the point of perfect justice on the justice evaluation scale) and solve for earnings.

We now derive the underlying justice evaluation function using the data in the single-person equation. The same can, of course, be done for all the

[3] Regressions of the observed mean justice evaluation at the level of the vignette, rather than of the raw observation at the level of the individual rating, produce R^2s close to unity. E.g., in the case of the one-earner married couples, at vignette level (N = 19), R^2 = .9718, while at rating level (N = 379), R^2 = .710.

TABLE 1

ESTIMATED JUSTICE EVALUATION FUNCTIONS

Vignettes	Justice Evaluation =	R^2	N
One-person-single	−3.784 +2.2364 ln earns. −0.0779 ed. −0.0154 occ. +0.3469 sex	.685	1,994
One-person-married	−3.830 +2.0949 ln earns. −0.0423 ed. −0.0192 occ. +0.1887 sex −0.0748 ch.	.651	3,986
All one-person	−3.661 +2.1389 ln earns. −0.0556 ed. −0.0181 occ. +0.2440 sex −0.0756 ch. −0.1827 MS	.663	5,980
Two-earner married-couple	−5.554 +2.4691 ln comb. earns. −0.0378 HEd −0.0269 WEd −0.0158 HOcc −0.0097 WOcc −0.0424 ch.	.501	5,447
One-earner married-couple	−4.478 +2.2262 ln earns. −0.0582* ed. −0.0135* occ. +0.3696* sex −0.0204 ch.[a]	.710	379

NOTE.—All R^2 and *b*-weight values are statistically significant beyond the .001 level except as noted below. ch. = children; MS = marital status; HEd = husband's education; WEd = wife's education; HOcc = husband's occupation; WOcc = wife's occupation.

[a] N.S.

* $P < .05$.

other equations. We begin by deriving the just earnings function.

$$\ln \text{just earnings} = \frac{3.784 + 0.0779 \text{ ed.} + 0.0154 \text{ occ.} - 0.3469 \text{ sex}}{2.2364}$$

Next, restating the original equation,

$$\text{justice evaluation} = 2.2364 \ln \text{earnings} - 2.2364 \times \left[\frac{3.784 + 0.0779 \text{ ed.} + 0.0154 \text{ occ.} - 0.3469 \text{ sex}}{2.2364}\right]$$

Hence:

$$\text{justice evaluation} = 2.2364 \ln \text{actual earnings} - 2.2364 \ln \text{just earnings}$$

it then follows that

$$\text{justice evaluation} = 2.2364 (\ln \text{actual earnings} - \ln \text{just earnings})$$

and that

$$\text{justice evaluation} = 2.2364 \left[\ln \frac{\text{actual earnings}}{\text{just earnings}}\right].$$

This means that the justice evaluation is a logarithmic function of the ratio of actual earnings to just earnings or, alternatively, a function of the signed difference between the logarithms of actual earnings and of just earnings. This formula produces a justice evaluation in meaningful justice units, and it quantifies the human experience that deficiency is felt more keenly than excess. The multiplicative constant transforms the raw justice evaluation into the units of the particular justice scale used. For example, in the present case, table 1 shows that the constant hovers about 2.2 and varies only slightly for the two-earner married couples.

In the next section I shall discuss in detail the form of the new justice evaluation function and its implications. For the moment, let us review what we have done empirically to reach it. First, we fit the best and most faithful representation of the data, and, second, we found within the equation of that best-fitting curve a new and more general specification of the justice evaluation function, one that may turn out to be a universal Law.[4]

[4] The justice evaluation function provides a good illustration of Blalock's (1971) remarks on the inappropriateness of standardized regression coefficients for theory construction. E.g., if the justice evaluation function were operating, i.e., if it were in effect a law, then empirical regression analysis would always yield unstandardized coefficients for actual earnings and just earnings of about 1 and −1, respectively; but the beta weights would fluctuate wildly with the observed variances of the two predictor terms, variances which may differ in fact across populations or which may differ artificially in experimental settings. Like the betas, the notions of variance decomposition and of variance explained by

As noted earlier, finding the best-fitting curve also produces information on the question of the determinants of just earnings. In our derivation of the justice evaluation function we expressed the equations reported in table 1 as estimates of just earnings. Now, by restating those equations, we can derive a new just earnings function. Continuing with the equation we have been using for illustrative purposes, that for the one-person-single vignettes, we derive the just earnings function:

$$\ln \frac{\text{just}}{\text{earnings}} = \frac{3.784}{2.2364} + \frac{0.0779 \text{ ed.}}{2.2364} + \frac{0.0154 \text{ occ.}}{2.2364} - \frac{0.3469 \text{ sex}}{2.2364},$$

$$\ln \frac{\text{just}}{\text{earnings}} = 1.692 + 0.0348 \text{ ed.} + 0.0069 \text{ occ.} - 0.1551 \text{ sex},$$

$$\frac{\text{just}}{\text{earnings}} = (5.4303)(1.0354^{\text{ed.}})(1.0069^{\text{occ.}})(0.8563^{\text{sex}}).$$

Table 2 reports the final just earnings functions for all subsets of the vignettes. It is immediately clear that there are important commonalities between all these equations and that they have an intuitively clear meaning. They are exponential functions in which the attributes of the earner produce returns calculated on a principal amount.

These just earnings functions represent faithfully the relations in the data set. But they do not have the same status as our candidate for Law of Justice Evaluation.[5] We cannot claim universality for them. We cannot claim that the respondents would recognize the procedure. It is possible that the respondents are using some other measure of just earnings, say, the mean or the median of earner classifications, a measure that may be coincidentally correlated with the predictions made by the just earnings functions.

On the other hand, we note briefly the possibility that these just earnings functions mirror a process in which the principal represents basic or minimum earnings for all earners (perhaps even a minimum allowance for all adults) based on the principle of need and that the other terms represent just re-

each of the two independent terms are inappropriate and misleading. On the other hand, while the betas are worthless in the task of confirming general laws, they do provide one highly important piece of information about observed conditions in the research setting (natural or artificial). The term with the largest beta has the largest variance. Hence, the betas indicate unmistakably what sort of dispersion is deemed just in a particular population. E.g., in the data analyzed here, actual earnings "account for about 90% of the explained variation in the justice evaluation" and just earnings (understood as the set of justly perceived claims on earnings) "account for the remainder"; hence, we can infer that the acceptable variation in just earnings is considerably smaller than that put experimentally into actual earnings. Note that this illuminates our understanding of "what people think is just," while remaining neutral to the task of specifying the justice evaluation function.

[5] I am grateful to an anonymous referee for emphatically pointing this out.

TABLE 2

Estimated Just Earnings Functions

Vignettes	Just Earnings =	R^2	N
One-person-single	$(5.4303)(1.0354^{ed.})(1.0069^{occ.})(0.8563^{sex})$	.685	1,994
One-person-married	$(6.2227)(1.0204^{ed.})(1.0092^{occ.})(0.9138^{sex})(1.0363^{ch.})$	.651	3,986
All one-person	$(5.5378)(1.0263^{ed.})(1.0085^{occ.})(0.8922^{sex})(1.0359^{ch})(1.0892^{MS})$	.663	5,980
Two-earner married-couple*	$(9.4821)(1.0154^{HEd})(1.0110^{WEd})(1.0064^{HOcc})(1.0039^{WOcc})(1.0173^{ch.})$	.501	5,447
One-earner married-couple	$(7.4745)(1.0265^{ed.})(1.0061^{occ.})(0.8470^{sex})(1.0092^{ch.})$	.710	379

Note.—See notes to table 1.

* Just combined earnings.

turns based on the principle of desert. I shall not comment further on the just earnings functions in this paper.

In the next section, I discuss the justice evaluation function.

THE NEW JUSTICE EVALUATION FUNCTION

The Form of the Function

The present data indicate that the justice evaluation of earnings is a logarithmic function of the ratio of actual earnings to just earnings:

$$\text{justice evaluation} = \ln \frac{\text{actual earnings}}{\text{just earnings}} .$$

In other words, when an observer judges the fairness or unfairness of an individual's earnings, the mental operation involves a ratio procedure. A mental image of what just earnings would be for such a person is quickly retrieved or formed, a ratio made of the actual earnings to the just amount, and "something" that can be formally represented by a logarithm taken of the ratio.[6]

The justice evaluation score associated with an individual who earns exactly his or her just earnings would be zero, which is the logarithm of the ratio one. Zero represents the point of justice, and such a person would be considered justly rewarded. When actual earnings exceeds just earnings, the ratio is larger than unity and its logarithm is a positive number, indicating unjust overreward. When just earning exceeds actual earnings, the ratio is a proper fraction and its logarithm is a negative number, indicating unjust underreward.

This means that the magnitude of injustice is equal for any fraction of one and for its reciprocal, because the logarithm of such a fraction and of its reciprocal are identical except in sign. To illustrate: the magnitude of injustice associated with an individual who earns one-half of his or her just earnings (whatever that amount may be) is −0.6931 (rounded), which is the natural logarithm of the ratio 0.5. The justice evaluation score of −0.6931 indicates that the individual is unjustly underrewarded and that the injustice is of a magnitude of 0.6931. The justice evaluation score of an individual who earns twice his or her just earnings is +0.6931 (rounded),

[6] The form of the justice evaluation function is similar to that of Bernouilli's utility function and of Weber–Fechner's sensation function (Stevens 1975). However, in both the utility and the sensation functions, the numerator of the ratio is larger by definition than the denominator (in the case of utility, the numerator contains an increment to the denominator; in the case of sensation, the numerator is a stimulus greater than a threshold value placed in the denominator), thereby producing functions that range only over the positive segment of the real-number line. To my knowledge, the justice evaluation function is the first logarithmic specification proposed in the sociobehavioral sciences in which two terms are freely varying and whose predicted values span the full real-number line.

which is the logarithm of the ratio 2. This score of +0.6931 indicates that the individual is unjustly overrewarded and that the magnitude of the injustice is 0.6931. Similarly, the magnitude of the injustice associated with an individual who earns one-third of his or her just earnings would equal that associated with an individual who earns three times his or her just earnings, and so on. For example, if just earnings are $10,000, then the absolute magnitude of injustice is 0.6931 equally for actual earnings amounts of $5,000 and of $20,000; in this case, a deficiency of $5,000 produces the same magnitude of injustice as an excess of $10,000.

These properties of the justice evaluation function may be summarized by saying that the geometric progression of the ratio of actual earnings to just earnings produces an arithmetic progression in the corresponding sequence of justice evaluation scores and that the common difference in the sequence of justice scores is the logarithm of the common ratio of the geometric progression.

Thus, to produce an increment of x in the justice evaluation score, a given ratio of actual to just earnings would have to be multiplied by the antilogarithm of x; to produce a decrement of x in the justice evaluation score, a given ratio would have to be divided by the antilogarithm of x.

This means that, holding just earnings constant, to produce an increment of size x in the justice evaluation, it is necessary to increase actual earnings by adding to it an amount equal to $(e^x - 1)$ of actual earnings; to produce a decrement of size x in the justice evaluation, it is necessary to decrease actual earnings by subtracting an amount equal to $(1 - e^{-x})$ of actual earnings.

Now, for all positive x, the relative decrement $(1 - e^{-x})$ is smaller than the relative increment $(e^x - 1)$. Formally,

$$[(1 - e^{-x}) < (e^x - 1)]\,, \quad x > 0\,.$$

This inequality quantifies the common human experience that deficiency is felt more keenly than excess.

The ratio of the relative decrement $(1 - e^{-x})$ to the relative increment $(e^x - 1)$ is $(1/e^x)$. This ratio indicates that the dollar amount of the deficiency is related to the dollar amount of the excess as the reciprocal of the antilogarithm of the absolute value of the justice evaluation. Thus, in the case above where, given just earnings of $10,000, actual earnings of $20,000 and of $5,000 produced justice evaluations of 0.6931 and −0.6931, respectively, the deficiency of $5,000 is $(1/e^{0.6931})$ or one-half the excess amount of $10,000.

Finally, we note that the logarithms of the ratios of actual earnings to just earnings may be regarded as "raw" justice scores. Multiplication by a constant transforms them to justice scores on particular justice scales. For example, a constant of about 2.2 transforms the raw scores to the nine-point

justice scale used in the present research. But, whatever the transformation, the relation remains invariant: it is judged equally unfair to earn one-half or two times one's just earnings.

The Justice Evaluation Function in Classical Perspective

Let us see now how the new specification fares when placed in the classical contexts of social science discourse. We return to the models proposed by Homans (1974, 1976) and by Berger et al. (1972) discussed earlier.

The Homans model.—With a little nimbleness, we can equate Homans's reward term with the actual earnings term and his investment term with the just earnings term (for just earnings is a function of the various claims on earnings allocation, a claim is akin to an investment, and, of course, both terms must be expressed in the same currency) and produce a ratio that Homans (or Aristotle) might recognize. Now, if these ratios are equal for two earners, Homans would say that justice obtains in the system. This is a perfect consequence of his position that "whatever is, is right," as long as whatever is, is consistently and impartially so. Our formulation, on the other hand, would produce equal justice scores, but not necessarily justice. Equal justice scores entail no specific condition of justice; they may be both -3 or both zero; that is, they may be both excruciatingly unjust or both perfectly just.

But now look at the present formula a little closer. If an observer indeed judges that "whatever is, is right," that, say, whatever a worker manages to get on the market is fair, then such an observer makes justice evaluations in this manner: he or she looks at the numerator, places that same amount in the denominator, and the result is perfect justice. This is a useful feature for observers who regard injustice primarily as underpayment and who have difficulty with the notion of "unjustly too much." Our formula, then, has the virtue of providing an unambiguous measure of the justice evaluation, even under conditions of idiosyncratic rules of just allocation. Hence, the investigator needs to know only an observer's standards of justice for any earner (or any category of earner) to predict his or her justice evaluation for any earner. Of course, consensually held standards of justice are much more powerful, for then the investigator does not need to ascertain an observer's standards of justice in order to predict his or her justice evaluations.

The model of Berger et al.—Our new specification is an accurate, but more extensive and precise, representation of the model proposed by Berger et al.; it uses the same two terms, for the real-world actor's level of a goal-object can be construed as actual earnings, and the "referential structure" level of a goal-object can be construed as just earnings. Our formula takes these terms and specifies the nature of the comparison as one not of arithmetic difference but, more precisely, of proportion (or the difference between two

logarithms). Our model leads to a statement of the precise sort of injustice, that is, of deficiency or of excess, and it predicts the exact magnitude of injustice.

The models of Homans and Berger et al.—Examination of the two classical models in light of the justice evaluation function sharpens them, enabling us to see clearly that the major distinction between them lies in the etiology of the just earnings term. We think that both Homans and Berger and his associates would agree that our formula accurately quantifies their thinking on justice evaluations and on types and degrees of injustice; Homans would no doubt feel comfortable with the logarithm of a ratio, and Berger et al. would no doubt feel comfortable with the difference between two logarithms. The disagreement between them seems to be exclusively about just shares, about whether the principles of justice actually held by individuals and societies are based on existential or utopian criteria. Put simply, their argument is not about how people judge relative injustices after they have formed notions of justice but, rather, about how they first acquire those notions of justice.

Generalization of the Justice Evaluation Function

Empirical support for our specification of the justice evaluation function has been reported for the case of earned income. I now propose its extension to cover all socially distributed goods. The most general statement is

$$\text{justice evaluation} = \ln \frac{\text{actual amount of good}}{\text{just amount of good}} .$$

Stated in this manner, it can be applied to whatever good one pleases, to land, livestock, inheritance, children's allowances, hours of overtime work, incentive pay, and so on.

It can readily be seen that the justice evaluation function, applied to any good of distribution, always produces precise and unambiguous evaluations in meaningful justice units. It is, therefore, a strong candidate for a universal Law of Justice Evaluation.

SUMMARY AND A LOOK AHEAD

In this paper I have reported the empirical induction of a new specification of the justice evaluation function. It has been shown to be well supported for the case of earnings. I have proposed it as a universal Law of Justice Evaluation about shares of all goods of social distribution.

Let us see now the place such a Law occupies in the scientific study of the sense of justice and injustice. We consider that a theory of distributive justice must contain at least three Laws, that is, that it must perform three

tasks. First, it must specify determination of the "just" term, of what is actually considered just by individuals and societies. Second, it must specify the kind and degree of injustice associated with given departures from justice, that is, it must specify the justice evaluation. Third, it must specify behavioral consequences of the justice evaluation.

I submit that we have accomplished the second of the tasks requisite to a theory of distributive justice. Work must now proceed on the other two.

With respect to the first, I believe that the lines of the argument have been well drawn by Homans and Berger et al.: Are persons' notions of "what is just" formed by reference to "what is" or to "what ought to be"? In the course of daily life, I have observed the operation of both existential and utopian criteria, and so I suggest that the question may be more fruitfully posed as follows: What accounts for the observed variability in individuals' formation of the just term?[7]

The third task involves determination of the following: (1) both individual and social responses (2) to both kinds of injustice (3) judged both about oneself and about others. The third task in the construction of a theory of distributive justice is perhaps the most difficult. But many investigators have been gathering data in many settings—in the historical record, in the laboratory, and in contemporary society. Correlation analysis of observed behaviors and observed justice evaluations may soon make possible explanation and prediction of many personal and social phenomena.

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[7] I consider that much of the dynamics of social life centers on determination of the "just" term and that the force of ideology is here particularly visible. A practical implication of this view is that the strategy of choice for revolutionary propagandists is not persuasion of the citizenry that a given actual condition is unjust but, rather, persuasion of the citizenry that some other, nonfactual, very precisely described condition is just. For once the citizenry accepts as just some envisioned state of things, the judgment that the actual condition is unjust follows inexorably.

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Threshold Models of Collective Behavior[1]

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Models of collective behavior are developed for situations where actors have two alternatives and the costs and/or benefits of each depend on how many other actors choose which alternative. The key concept is that of "threshold": the number or proportion of others who must make one decision before a given actor does so; this is the point where net benefits begin to exceed net costs for that particular actor. Beginning with a frequency distribution of thresholds, the models allow calculation of the ultimate or "equilibrium" number making each decision. The stability of equilibrium results against various possible changes in threshold distributions is considered. Stress is placed on the importance of exact distributions for outcomes. Groups with similar average preferences may generate very different results; hence it is hazardous to infer individual dispositions from aggregate outcomes or to assume that behavior was directed by ultimately agreed-upon norms. Suggested applications are to riot behavior, innovation and rumor diffusion, strikes, voting, and migration. Issues of measurement, falsification, and verification are discussed.

BACKGROUND AND DESCRIPTION OF THE MODELS

Because sociological theory tends to explain behavior by institutionalized norms and values, the study of behavior inexplicable in this way occupies a peripheral position in systematic theory. Work in the subfields which embody this concern—deviance for individuals and collective behavior for groups—often consists of attempts to show what prevented the established patterns from exerting their usual sway. In the field of collective behavior, one such effort involves the assertion that new norms or beliefs "emerge"

[1] This report is heavily indebted to three co-workers. Christopher Winship is responsible for important aspects of the formalization, Douglas Danforth and Bob Phillips have carried out a large part of the mathematical analysis and computer programming. All have given valuable substantive help as well. Specific contributions are footnoted in the text. I have also benefited from the thoughtful comments of participants in seminars and colloquia I have given at Harvard, Wesleyan, Columbia, Stanford, Stony Brook, UCLA, and Temple. The work has been done mainly in the stimulating atmosphere of the Center for Advanced Study in the Behavioral Sciences, where the criticisms of Robert Axelrod, Brian Barry, and Arthur Goldberger led to improvements. The center's support was made possible in part by the Andrew Mellon Foundation and the National Science Foundation. Partial support for the research was provided by a sabbatical leave from Harvard University and by National Science Foundation grant SOC 76-11185 to the author.

in situations where old ones fail or few precedents exist (Turner and Killian 1957; Smelser 1963).

Such arguments are an advance over crude psychologizing about crowds' stripping away the "veneer" of civilization from their participants. But I will argue here that knowing the norms, preferences, motives, and beliefs of participants in collective behavior can, in most cases, only provide a necessary but not a sufficient condition for the explanation of outcomes; in addition, one needs a model of how these individual preferences interact and aggregate.

Because theories oriented to norms lack such a model, they end up assuming, implicitly, a simple relation between collective results and individual motives: that if most members of a group make the same behavioral decision—to join a riot, for example—we can infer from this that most ended up sharing the same norm or belief about the situation, whether or not they did so at the beginning.

The models I will describe, by contrast, take as the most important causal influence on outcomes the *variation* of norms and preferences within the interacting group. It will be clear even in the simplest versions of these models that the collective outcomes can seem paradoxical—that is, intuitively inconsistent with the intentions of the individuals who generate them. This possibility is foreclosed if we insist that collective outcomes reflect norms, whether old or new, of most of the participants. Further, once we abandon the definition of collective behavior situations as those in which people develop new norms or abandon existing ones, the range of situations which can be considered broadens. Thus, these models can be applied to processes not usually called "collective behavior," such as voting, residential segregation, diffusion of innovations, educational attainment, strikes, migration, and markets—as well as the more typical processes of crowd behavior and social movements.

It is best to stress at the outset what is not attempted. These models treat the aggregation of individual preferences; they do not consider how individuals happen to *have* the preferences they do. That very important question is outside the main concern of this paper. I begin with preferences and go from there. Most existing literature, by contrast, channels its main effort into determining how norms, motives, and preferences are caused and assumes that nothing more need be done to explain collective behavior. I maintain instead that once these are known, there is still a great deal to be done, and that outcomes cannot be determined by any simple counting of preferences. This will be particularly clear in cases where a very small change in the distribution of preferences generates a large difference in the outcome. Analysis focusing only on determination of preferences could not explain such a phenomenon.

Threshold Models of Collective Behavior

The models of this paper treat binary decisions—those where an actor has two distinct and mutually exclusive behavioral alternatives. In most cases the decision can be thought of as having a positive and negative side—deciding to do a thing or not to, as in deciding whether to join a riot—though this is not required for the formal analysis. A further requirement is that the decision be one where the costs and benefits to the actor of making one or the other choice depend in part on how many others make which choice. We may take riots as an example. The cost to an individual of joining a riot declines as riot size increases, since the probability of being apprehended is smaller the larger the number involved (see, e.g., Berk 1974).

The individuals in these models are assumed rational—that is, given their goals and preferences, and their perception of their situations, they act so as to maximize their utility. Individual differences are a main focus of the models. Different individuals require different levels of safety before entering a riot and also vary in the benefits they derive from rioting. The crucial concept for describing such variation among individuals is that of "threshold." A person's threshold for joining a riot is defined here as the proportion of the group he would have to see join before he would do so. A "radical" will have a low threshold: the benefits of rioting are high to him, the cost of arrest, low. Some would be sufficiently radical to have a threshold of 0%—people who will riot even when no one else does. These are the "instigators." Conservatives will have high thresholds: the benefits of rioting are small or negative to them and the consequences of arrest high since they are likely to be "respectable citizens" rather than "known rabble-rousers." Thresholds of 80% or 90% may be common, and we may allow for those individuals who would not join under any circumstances by assigning them a threshold of 100%.

It is not necessary, in fact, to be able to classify a person as radical or conservative from his threshold, and one strength of the concept is that it permits us to avoid such crude dichotomies. Since a threshold is the result of some (possibly complex) combination of costs and benefits, two individuals whose thresholds are the same may not be politically identical, as reflected in the popular expression, "strange bedfellows." The threshold is simply that point where the perceived benefits to an individual of doing the thing in question (here, joining the riot) exceed the perceived costs.[2]

[2] I have adapted the idea of behavioral thresholds from Schelling's models of residential segregation (1971*a*, 1971*b*, 1972), where thresholds are for leaving one's neighborhood, as a function of how many of one's own color also do so. The present paper has Schelling's aim of predicting equilibrium outcomes from distributions of thresholds but generalizes some features of the analysis and carries it in somewhat different directions. The model has some resemblance also to one known in psychology as "behavioral contagion" (for a review see Wheeler 1966). In Wheeler's formulation, the cost-benefit analysis described here is seen as

The focus of this paper is the formal model and *not* the substantive question of riot behavior. In describing the model I will nevertheless usually talk about "riot thresholds" because this is a convenient and colorful illustration; but it has no special conceptual status, and the reader should keep in mind that the analysis is meant to apply to any appropriate binary decision. Before beginning formal analysis, therefore, I will suggest a catalog of other binary-choice situations where threshold models could be applied.

1. Diffusion of innovations. Women in Korean villages may be wary of adopting birth control devices and wait to do so until some proportion of their fellow villagers do. Different women will have different thresholds, depending upon their education, age, husband's opinions, position in a hierarchy of informal leadership, or personal tastes (see Rogers 1975; Dozier 1977).

2. Rumors and diseases. In order to spread a rumor, one must hear it from another person. But people vary in their credulity and some may need to hear it from more than one other before they will believe it enough to spread it. These levels of credulity are the same as thresholds. A formally identical situation is the spread of a disease, where credulity is replaced by "vulnerability": people differ in how many infecteds they must be exposed to before they too catch the disease.

3. Strikes. Workers deciding whether to strike will attend carefully to how many others have already committed themselves, since the cost of being one of a small number of strikers is high, especially in a vulnerable employment situation. One would thus expect teachers without tenure to have higher strike thresholds than those with tenure.

4. Voting. One's decision to vote for a particular candidate may depend heavily on how many others have already decided to do so, partly because

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an "approach-avoidance conflict," and "contagion" occurs at the point where observing another individual's behavior pushes the approach tendency above the avoidance tendency --nearly equivalent to the definition above of threshold as the point at which benefits exceed costs. No consideration is given, however, to how many individuals one might need to observe before this point is reached, or of cumulative effects of those observed before the final person. In the broader context of threshold models, the idea of "contagion" seems inappropriate, since much more is involved than mere imitation of the last person observed. There is also some similarity between the present models and models used in epidemiology (as in Bailey 1976), the diffusion of information (Bartholomew 1967) and innovations (Hamblin, Jacobsen, and Miller 1973), and the evolution of behavior in groups over time (Coleman 1965, chaps. 10 and 11). To develop these analogies in more detail would require that (1) my models, expressed below as difference equations in discrete time, be translated into differential equations in continuous time, and that (2) some way be found to introduce the "threshold" concept into these other models, which generally do not stress individual differences (cf. especially Coleman's discussion of "heterogeneity"). While some work in this direction has been accomplished, it is incomplete and could not be adequately presented in a brief way. Hence, it is deferred to future publications.

of social influence, partly because one does not want to waste one's vote. One outcome of this situation is what we call "bandwagon effects."

5. Educational attainment. The decision to go to college depends in part on what proportion of one's cohort does so. This is partly because of peer-group influence, partly because a large attendance in one's cohort raises the general level of credentials in the labor market, making it more difficult to find suitable employment without a college degree (see, e.g., Berg 1970).

6. Leaving social occasions. We have all had the experience of sitting impatiently at a boring lecture, unable to leave because not enough others have yet done so. People vary in their thresholds for leaving lectures, cocktail or dinner parties, or other occasions. The variation is composed partly of personality traits—politeness, timidity—and partly of the press of other obligations.

7. Migration. It is well known that migration decisions depend heavily on those of others, as in "chain migration" (MacDonald and MacDonald 1964). Those with low migration thresholds are likely to have more psychological and economic resources than those who migrate later.

8. Experimental social psychology. Experiments on conformity achieve varying results depending on the number of confederates introduced (Asch 1956; Milgram, Bickman, and Berkowitz 1969). "Risky shift" experiments could be reanalyzed to study the time sequence in which people change from less to more risky alternatives (see Pruitt and Teger 1971). Situations of bystander intervention could be described by means of "helping thresholds."

Equilibrium Outcomes in Simple Threshold Models

For all the examples above, the aim of the formal model presented here is the same: to predict, from the initial distribution of thresholds, the ultimate number or proportion making each of the two decisions. Mathematically, the question is one of finding an equilibrium in a process occurring over time. A simple example will make the procedure clear.

Imagine 100 people milling around in a square—a potential riot situation. Suppose their riot thresholds are distributed as follows: there is one individual with threshold 0, one with threshold 1, one with threshold 2, and so on up to the last individual with threshold 99.[3] This is a uniform distribution of thresholds. The outcome is clear and could be described as a "bandwagon" or "domino" effect: the person with threshold 0, the "instigator," engages in riot behavior—breaks a window, say. This activates the person with threshold 1; the activity of these two people then activates the person

[3] Depending on the substantive situation, thresholds may usefully be described as either proportions or absolute numbers. The mathematical analysis is the same in either case. For convenience, numerical examples given in this paper use groups of 100 people, so the reader may think of thresholds as proportions or numbers.

with threshold 2, and so on, until all 100 people have joined. The equilibrium is 100.

Now perturb this distribution as follows. Remove the individual with threshold 1 and replace him by one with threshold 2. By all of our usual ways of describing groups of people, the two crowds are essentially identical. But the outcome in the second case is quite different—the instigator riots, but there is now no one with threshold 1, and so the riot ends at that point, with one rioter.

Even this simple-minded example makes the main point suggested earlier: it is hazardous to infer individual dispositions from aggregate outcomes. Newspaper reports of the two events would surely be written as, in the first case, "A crowd of radicals engaged in riotous behavior"; in the second, "A demented troublemaker broke a window while a group of solid citizens looked on." We know, however (since we constructed the example), that the two crowds are almost identical in composition; the difference in outcome results only from the process of aggregation, and in particular from the gap in the frequency distribution in the second case.

The reader may want to try out the example as well on the cases of innovation adoption, rumor or disease spreading, strikes, voting, going to college, leaving social occasions, migration, or conformity. Bandwagon effects can be imagined for each, but the sensitivity of such effects to exact distributions of preferences is rarely appreciated. Threshold models may be of particular value in understanding situations where the average level of preferences clearly runs strongly in favor of some action, but the action is not taken. The usual sociological models of action have limited value in such cases.

It is possible to give a mathematically exact account of how one goes from a frequency distribution of thresholds to an equilibrium outcome.[4] Denote thresholds by x, the frequency distribution by $f(x)$, and the cumulative distribution function (c.d.f.) by $F(x)$—where the c.d.f. indicates the proportion of the population having threshold less than or equal to x. Call the proportion of the population who have joined a riot by time t (using discrete time periods) $r(t)$. Suppose we know $r(t)$ for some t—for example, suppose we know that after two time periods ($t = 2$) 60% of the crowd has joined in. Then what proportion of the crowd will be rioting at $t = 3$? It must be, by definition of thresholds, exactly that proportion of the crowd whose thresholds are less than or equal to 60%. It follows immediately that the process is described by the difference equation: $r(t + 1) = F[r(t)]$.

Where the frequency distribution has a simple form, the difference equation can be solved explicitly to give an expression for $r(t)$ at any value of t. Then, by setting $r(t + 1) = r(t)$, the equilibrium outcome may be

[4] This analysis and that of fig. 1 are due to the efforts of Christopher Winship.

found. Where the functional form is not simple, the equilibrium may nevertheless be computed by forward recursion. In this simple version of the model, where no provision has been made for "removal" of participants, oscillatory behavior of $r(t)$ is not possible, and an equilibrium will always be reached.

Some graphical observations show that equilibrium points can be computed without manipulating difference equations or engaging in forward recursion. In figure 1, we graph thresholds (x) against the c.d.f. [$F(x)$]. Suppose, as before, that $r(t)$ is known. Since $r(t + 1) = F[r(t)]$, we may find the proportion rioting in the next time period by following the leftmost arrow from $r(t)$ to the point immediately above it on the c.d.f. To locate this point again on the x-axis, we follow the horizontal arrow to the 45° line, $F(x) = x$. This procedure can then be repeated to find $r(t + 2) = F[r(t + 1)]$, and so on. For the c.d.f. drawn in figure 1, we can see that the
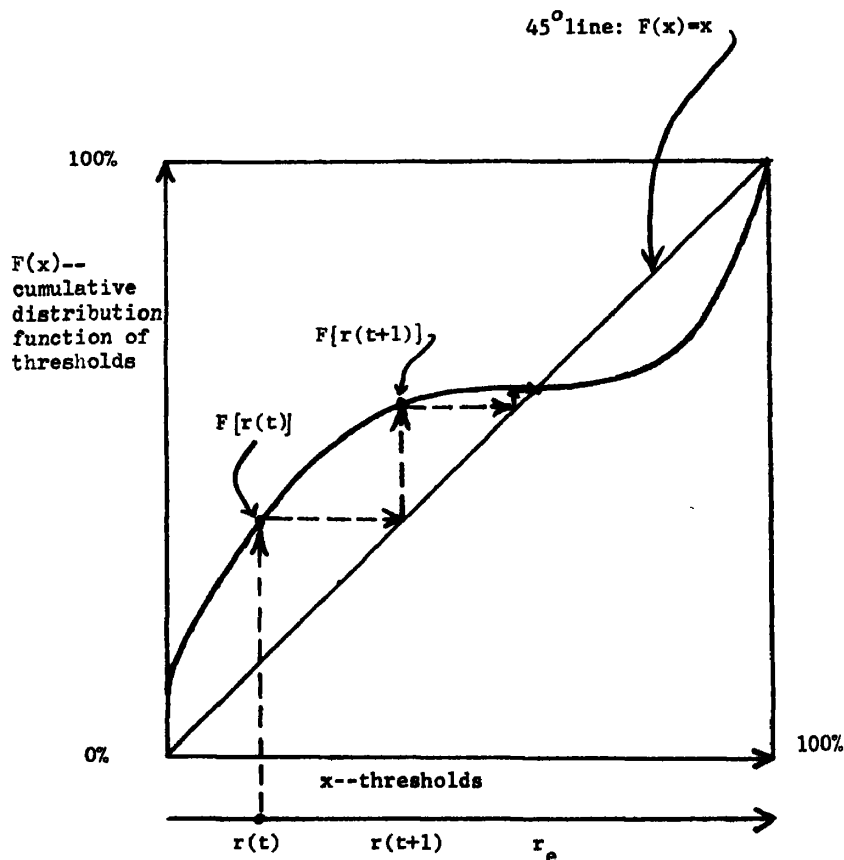
FIG. 1.—Graphical method of finding the equilibrium point of a threshold distribution. $r(t)$ = proportion having rioted by time t.

horizontal length of the arrow goes to zero, and $r(t)$ goes to a limiting value called r_e—the equilibrium point. That limit is the point where the c.d.f. first crosses the 45° line from above. Algebraically, the point is denoted by the equation $F(r) = r$.

Without empirical or theoretical reason to expect one particular distribution or another, it is not fruitful to pursue extensively the behavior and equilibria of large numbers of functional forms. However, I will present some results obtained from normal distributions of thresholds, using the equilibrium analysis of figure 1. The normal frequency distribution is of interest here because we may take it to be characteristic of populations where no strong tendencies of any kind exist to distort a distribution of preferences away from its regular variation about some central tendency. Yet, the results obtained are striking and counterintuitive, showing that paradoxical outcomes are not limited to special distributions such as the uniform.[5]

Consider, again, 100 people; let their thresholds now be normally distributed, with mean threshold equal to 25. (Those thresholds below zero may be regarded, for practical purposes, as equivalent to zero. Theoretically they may be seen as degrees of radicalism which may have ideological significance but lead to the same action. Similar comments apply to thresholds above 100.) We may now ask what the effect is on the equilibrium outcome of varying the standard deviation of our normal distribution, leaving the mean fixed. The surprising result is graphed in figure 2, a plot of r_e against σ, the standard deviation. Up until a critical point, σ_c, the equilibrium number of rioters increases gradually to about six. Then after this point, approximately 12.2, the value of r_e jumps to nearly 100, after which it declines. (The limiting value, as σ increases without bound, is 50, since eventually all the area to the right of the mean can be seen as beyond 100, all the area to its left below 0.)

Mathematically, this is easily explained. Equilibrium is found by noting the first intersection of the c.d.f. with the 45° line, from above. The normal c.d.f. may intersect the line either three times, twice, or once. For values of σ below σ_c, the first intersection from above is at a low point and is followed by one from below and, later, by another from above. At the critical point, σ_c, the first two intersections are combined in a point tangent to the 45° line, and there is one further intersection above. After this point, the only intersection occurs near 100, dropping off gradually as the probability density flattens.

This mathematical account, however, has no substantive companion. There is no obvious sociological way to explain why a slight perturbation of the normal distribution around the critical standard deviation should have a wholly discontinuous, striking qualitative effect. This perturbation

[5] The analysis of normal distributions of thresholds is due to the efforts of Bob Phillips.

might correspond to a minor fluctuation in the composition of a crowd, or to some change in the situation which altered the distribution of thresholds a bit—a cause which would seem so insignificant in relation to its effect that causal attribution would never be made. This is particularly the case since sociological theory is not at all oriented to analyzing the effects of changes in exact distributions of properties but concentrates, rather, on the effects imputed to average values. This example shows again how two crowds whose average preferences are nearly identical could generate entirely different results. As in the cases described earlier, we would be highly unlikely to infer accurately the structure of preferences that led to the outcomes without an explicit model of the aggregation process.

The Stability of Equilibrium Outcomes

Effects of friendship and influence.—The above discussion of equilibrium outcomes points up the need for systematic treatment of the stability of the equilibrium which follows from some given distribution of thresholds. For dynamic analysis this is indispensable, since a variety of influences may intervene in real situations to modify existing distributions. Whether these influences have small or large effects depends on these stability considera-
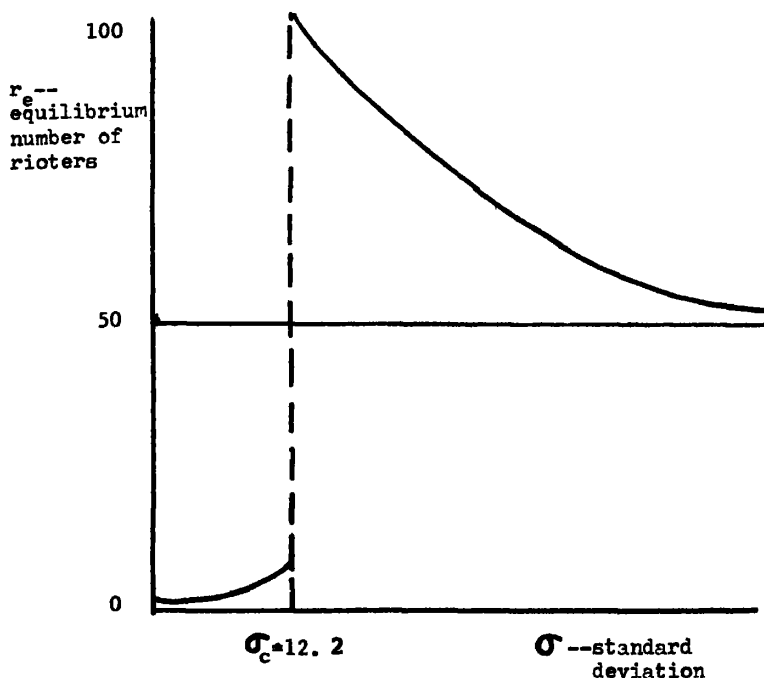
FIG. 2.—Equilibrium number of rioters plotted against standard deviation of normal distributions of thresholds with mean = 25, N = 100.

tions. One of the most obvious ways for the distribution to change is for some individuals to enter or leave the situation. We can take the varying of standard deviations in normal distributions or the changing of a uniform to a perturbed uniform distribution, described above, as a way of thinking about what happens to a distribution when such entry and departure occur. We find, then, that the true uniform distribution and the normal one near its critical standard deviation have highly unstable equilibria.

An eventual aim of the analysis of threshold models is to develop mathematical procedures for assessing the stability characteristics of any distribution's equilibrium under a variety of possible perturbations. In this section and the next, I discuss two particular factors which may play important roles in changing the effects of threshold distributions: social structure and the spatial/temporal dispersion of social action.

By "social structure" I mean here only that the influence any given person has on one's behavior may depend upon the relationship. Take a simple case, where the influence of friends is twice that of strangers, and assume that thresholds are given in terms of reaction to strangers. Consider an individual with threshold 50% in a crowd of 100, where 48 individuals have rioted and 52 have not. In the absence of social structure, such an individual would not be activated. But if he knows 20 people in this crowd of whom 15 have already joined the riot, then each friend is to be counted twice. Instead of "seeing" 48 rioters and 52 nonrioters, our subject "sees" $[(15 \times 2) + (33 \times 1)]$ rioters and $[(5 \times 2) + (47 \times 1)]$ nonrioters, leading him to form a ratio not of 48/100 but of 63/120 = .525. What we may then call the "perceived proportion of rioters" in the previous time period now exceeds his threshold, and he will join.

For any given situation, this procedure allows the computation of equilibrium results by forward recursion as long as we have the distribution of thresholds, a sociomatrix, and a weight for each sociomatrix cell, corresponding to how much more influence i has on j than a stranger would have. To make more general statements about the effects of social structure on outcomes, we need to say more systematically what parameters of social structure are of interest. Does a high or a low density of friendship ties, for instance, have the greater effect in modifying the equilibrium result of some particular distribution of thresholds? This question could be posed as well for lesser or greater weights.

For a given friendship density, however, even a moderate number of people generates an almost limitless number of possible sociomatrices. If we fix a threshold distribution and make the weights of friends all identical, each such matrix yields an equilibrium outcome, and the set of matrices gives a frequency or probability distribution of equilibria. Repeated attempts to derive these distributions analytically have failed, yielding only unmanageable convolutions. Some partial results are available, however, if

we do not require friendship to be symmetric. We may pose the null hypothesis that introduction of social structure makes no difference in the equilibrium generated by a given threshold distribution.

Consider the perturbed uniform distribution described above: one person with threshold 0, two with threshold 2, one with 3, one with 4 . . . one with 99. The null hypothesis is an equilibrium of one—only the instigator with threshold 0 would riot. Our analysis indicates that the null hypothesis becomes increasingly improbable as the weight attached to friends' behavior increases;[6] this seems intuitively reasonable. The results for acquaintance volume are more surprising; the largest effects occur where people know, on the average, about one-quarter of the rest of the group—a moderate level of friendship. The explanation is neither intuitively nor analytically obvious and will require further study.

To get fuller results on overall probability distributions of equilibria, we have had to construct computer simulations of the process in which the computer fixes parameters, constructs one sociomatrix after another from the class to be sampled, and generates an equilibrium by forward recursion. Simulations of the perturbed uniform distribution yield results quite close to our analytical ones and indicate that the symmetry of ties has little effect on outcomes. Simulations allow us to look not only at the null hypothesis of no change but also at the typical extent of change. For this particular distribution, even when the null hypothesis of no change is false, *the equilibrium changes very little and rarely exceeds five to 10 rioters.* Thus, the underlying outcome, one rioter, is relatively stable in the face of social structural influences. By contrast, simulation of social structural effects on the true uniform distribution of thresholds shows that its equilibrium of 100 rioters is unstable against almost any kind of social structural influence. For most combinations of weights and acquaintance volume tested, the modal equilibrium result is one rioter.

The general goal of such analysis is to specify the impact of social structure on collective outcomes. Most collective-behavior literature proceeds as if the groups discussed contained only people who are strangers to one another (an exception is Aveni 1977). I believe that social structure within interacting groups has important but complex relationships to results. When threshold distributions have very stable equilibria it may make very little difference; when these equilibria are unstable, however, the effects of social structure may overwhelm those of individual preferences. Sorting out the circumstances under which one effect is more important than another will improve our understanding of both social structure and collective behavior.

[6] These results and the subsequent computer simulations are the work of Douglas Danforth. More detail on both is presented in an earlier draft of this paper, available from the author.

Spatial and temporal effects.—Social structure is one reason why the simple form of threshold models may not provide an adequate account of events. Another is that the simple model makes an implicit assumption of complete connectedness which is often inappropriate: that each individual is responsive to the behavior of *all* the others, regardless of the size or spatial or temporal dispersion of the aggregation. Margaret Stark et al. (1974), for example, in an analysis of the 1965 Watts riot, report that rather than being a single incident it consisted of more than 1,850 separate cases of riot action in five days of rioting over a wide area.

Modeling the effects of spatial and temporal dispersion on equilibrium outcomes presents greater mathematical difficulties than those described in the previous sections, and progress has been slower. A few simple results will suggest, however, that interesting possibilities arise.

For simplicity, imagine a large population in some city with a distribution of riot thresholds equal to the uniform distribution discussed above: 1% of the population has threshold 0%, 1% has threshold 1%, 1% has threshold 2% . . . 1% has threshold 99%. Suppose also that when a crowd gathers it consists of a random sample from this large population and that its size is always 100. Recall that when a group with this *exact* distribution of thresholds gathers, the (deterministic) equilibrium is that everyone riots. But sampling variability changes this equilibrium conclusion.[7] If on one of these occasions, for example, the crowd that gathers contains no zero percenter (instigator), the resulting equilibrium is zero. The probability of this occurrence can be computed if we think of the 100 crowd members as having been drawn by Bernouilli trials, with probability of success equal to .01 ("success" = drawing a zero percenter). The probability of no successes in 100 trials is then $(1 - P)^{100} = .37$. Further, the chance of drawing one zero percenter but no one percenters—so that the equilibrium result is one rioter—is the product of the two probabilities, and thus equals $[\binom{100}{1}(.01)(.99)^{99}] \times (.99)^{100} = .14$. This means that in over half the cases (.37 + .14 = .51) the equilibrium result is either no rioters or one rioter. We see in still another way how vulnerable the equilibrium given by a uniform distribution of thresholds is to perturbation. The example suggests that in cities where a fairly constant distribution of riot thresholds exists the outcome of one crowd may nevertheless differ radically from that generated by another, at some earlier or later time, for reasons that have nothing to do with differences between the occasions or intervening events but involve only sampling variability. This will occur if the underlying distribution yields an equilibrium which is easily disrupted by changes. It then also

[7] While it may stretch the imagination a bit to argue that all crowds are simple random samples from the population at risk, there is a good deal of evidence for the more-or-less random character of casually forming groups of people (see Coleman 1965, pp. 361–75; White 1962; Cohen 1971).

follows that in two cities which have the same underlying distribution of riot thresholds one may experience a large riot and the other not, for reasons which do not reflect intercity differences.

Spilerman attempted to determine what characteristics of cities might have contributed to the number and severity of racial disorders experienced from 1961 to 1968 (Spilerman 1970, 1971, 1976). For both dependent variables, the only city characteristics which had an important impact, in stepwise multiple regression, were the absolute size of the black population and, much less significant, a dummy variable for region: South or non-South. Since Spilerman's main concern was to argue that particular city conditions did not affect the probability of riots—that the phenomenon was a national one—he spent little time discussing the substantial correlation with absolute black population, suggesting only that this variable "relates directly to the ability of the Negro community to mobilize a disorder and also to the number of incidents occurring in a ghetto which might precipitate a disturbance. . . . Disorder proneness is inherently a personal attribute, a response to factors which are exogenous to the community but visible in all ghettos. The community propensity, in this formulation, is an aggregate of the individual values and would therefore reflect the numerical size of the Negro population" (1970, pp. 643–44).

The temporal sampling variability discussed above suggests a mechanism which could explain Spilerman's correlation but would involve more than only "personal attributes." Suppose that before an incident is reported as a "riot" the equilibrium number of rioters must reach some level, and that a city has, each time a crowd gathers, the same probability of reaching this particular equilibrium. (Such a probability is determined by the underlying distribution of riot thresholds from which each crowd is drawn.) If this probability is, say, .10, and we argue as Spilerman does that the larger the black population, the larger the number of incidents which occur, then we may think of each incident as a Bernouilli trial with probability of success (a large riot) of .10. It is then clear that the expected number of large riots is a function of the number of incidents, since the mean of the binomial is the number of trials multiplied by the probability of success. In a community sufficiently small that only one "trial" occurred over a specified time period the chances are 90% that no large riot would occur. But in a larger city where 10 incidents occurred the chance of no riot falls to $(.90)^{10} = .35$, even though the distribution of thresholds is the same.

Sampling variability would have spatial as well as temporal effects. One can think of multiple samples from an underlying population as representing discrete clusters of individuals over some large area during the same time period. Then it follows that, despite being drawn from the same distribution, to the extent that the equilibrium is an unstable one there would be wide divergence from one place to another in the local outcome. A next natural

question would concern movement from one cluster to another and its effect on the overall number of rioters. One might ask, for example, what level of movement among clusters would have the most incendiary effect. The answer is not straightforward, since too much movement out of a cluster which had reached a high equilibrium may have the effect of deactivating some rioters, which would then deactivate others, until a new and possibly much lower equilibrium was reached—a reverse bandwagon. Thus, for some threshold distributions small movements among clusters may have greater effects than large ones. Much more mathematical work is needed to make this statement precise.

The introduction of spatial considerations makes the model more reasonable for large riots and generally for fragmented situations where there is some modest level of connection among fragments. A natural situation to model in this way would be the adoption of innovations where the main decisions take place in fairly discrete units and where there is some movement among units, for example, the adoption of family planning by women in Korean villages where there is some intervillage migration (Rogers 1975). The general import of these models is that threshold distributions, friendship structures, and migration patterns could have more impact on level of adoption ("equilibrium" results) than any features of the programs themselves. Analysts of family planning are often frustrated when the same program has different outcomes in areas where the average preferences are nearly identical. Threshold models may explain how this can occur. Another situation which could be modeled with spatial considerations is recruitment into political parties and spread of a party across a country. Relevant thresholds are those for joining the party, for example, the spread of Nazi party membership in Weimar Germany.

SOME THEORETICAL AND EMPIRICAL CONSIDERATIONS

Having sketched threshold models of collective behavior, I want to explore further their conceptual underpinnings, their relation to more usual explanations of collective behavior, and the possibilities for falsifying or verifying such models.

Thresholds, Game Theory, and Norms

Threshold models share with game-theoretic models the assumption of rational actors with complete information. Much recent literature on collective behavior has reacted against the older notion that irrationality is the key to explanation; theorists as diverse as Tilly (1975) and Banfield (1970) agree that collective behavior often results from rational, sometimes calculated, action.

Since the best-developed formal account of rational action in situations

of mutual interdependence is given by game theory (see Luce and Raiffa 1957), and since promising work has been done on game-theoretic models of collective behavior, it is worth saying what I think to be the advantages of threshold models.

Roger Brown's pioneering account (1965) reduced many typical collective behavior episodes—such as the stampede from a burning theater—to a Prisoners' Dilemma game (see Luce and Raiffa 1957, pp. 95–97); he collapsed the actions of a large number of people into the analysis of a two-person game in which each person "plays" against all the others taken collectively. Economists employ a similar strategy in explaining bank failures, refusals of farmers to cut production voluntarily, and inability of citizens to organize to achieve "public goods" (see Samuelson 1967, p. 12; Luce and Raiffa 1957, p. 97; Olson 1965).

All these examples have considerable intuitive appeal since they display situations where rational individual action, in pursuit of well-defined preferences, leads to outcomes undesirable to the actors and surprising, given their intentions. But reduction to a two-person game is possible only if all the actors are homogeneous in their preferences. A general analysis of collective behavior cannot be satisfied with such a severe limitation, since many situations of interest involve participants with widely different goals. While a body of theory does exist on games involving more than two people ("*n*-person games"), the analytical situation is far less satisfactory than for the two-person situation. The larger the number of distinctly different actors, the more difficult it becomes to characterize the outcome without introducing arbitrary assumptions (see Rapoport 1970).

Another difficulty is that game theory typically assumes that all actors' decisions are made simultaneously; no one's decision is *contingent* on anyone else's previous one. Both difficulties can be seen in Berk's stimulating game-theoretic analysis of a riot situation (Berk 1974). In his treatment, even the presence of two distinct sets of actors (militants and moderates) leads to considerable complication; yet it is hard to imagine that division into these two categories accurately depicts the range of variation present in the crowd. Further, Berk's adherence to the principle of simultaneous decisions leads him to try to explain the outcome solely in terms of events that occur *before* any riot action begins—namely, crowd members' attempts to change one another's perceived payoffs for various outcomes (p. 364). The decision of an individual to act—that is, join the riot—"rests on expectations of what others will do, [therefore] enough crowd members must arrive at parallel assessments which make action for all a good bet before activity is likely to begin" (p. 368). Here Berk not only forecloses the possibility of evolution of the process *after* riot activity starts but also requires a convergence among crowd members before it begins which is reminiscent of the notion of "emergent norms." In fact, Berk argues ex-

plicitly that "the gaming perspective fills important gaps in emergent norm theory"—namely, explaining the origin of such norms (p. 372).

Threshold models take the two elements of collective behavior which game theory handles only with difficulty and makes them central: substantial heterogeneity of preferences and interdependence of decisions over time. This is possible because the n-dimensional payoff matrix of game theory is replaced by a one-dimensional vector of thresholds, one for each actor. This allows enormous simplification in the ensuing analysis.

Like all simplifications, this one carries a cost. The payoff matrices of game theory allow us to investigate, for any particular actor, which outcome maximizes his utility and whether outcomes are Pareto optimal for the whole set of actors. Threshold analysis does not permit this. When an individual is activated because his threshold is exceeded, he acts so as to maximize his utility under existing conditions. The resulting equilibrium may or may not maximize anyone's overall utility. From the distribution of thresholds alone, nothing can be said about this. Thresholds do not give information about the utility to an individual of each possible equilibrium outcome.[8]

The equilibrium reached may well be suboptimal for most actors. Consider, for example, Matza's account of the behavior of delinquent boys whom he interviewed (1964, chap. 2). Most did not think it "right" to commit illegal acts or even particularly want to do so. But group interaction was such that none could admit this without loss of status; in our terms, their threshold for stealing cars is low because daring, masculine acts bring status, and reluctance to join, once others have, carries the high cost of being labeled a sissy or "faggot." Bandwagon effects occur like those of the formal models. But note that the suboptimality of the outcome for most boys would not be known to us if all we had was information on their thresholds; to know this, we require Matza's interviews as well.

This example shows also that thresholds are quite different from "norms"—another construct frequently invoked in the explanation of collective behavior. The boys act because their threshold is exceeded, and their utility is maximized, given the situation, by joining in the criminal activity. But in so doing they act contrary to norms they actually hold. That this is so indicates not that norms are irrelevant, but rather that they are only one causal influence on behavior and are not always decisive. The concept of threshold, then, is purely behavioral, connoting nothing about what the actor thinks is the "right" thing to do.

[8] Schelling (1973) describes models of binary choice situations where full utility functions are given explicitly. Most of his analysis requires every actor to have the same function, but see the brief discussion of how one might relax this limitation (pp. 415–22). The time sequence of choices is not treated.

The Nature and Determinants of Thresholds

Thresholds are different from norms but result in part from them; Matza's boys would have even lower thresholds for delinquent activities, one supposes, if they did not feel they were wrong. Thresholds are also affected by most of the causal variables typically studied as determinants of individual behavior—background characteristics, social class, education, occupation and social position; these all help establish the valuation given by an individual to different outcomes in a situation. The nature of the situation itself contributes to defining what outcomes are possible. Since behavior is partially determined by all the usual characteristics studied, it is not surprising that they have some moderate correlation with behavior. But if we take the threshold model seriously, it also follows that correlational studies will miss the dynamics of aggregation and thus be unable to provide more than this moderate level of correlation. In situations where we have, for example, two nearly identical distributions of thresholds which generate very different outcomes, correlational studies making predictions in the usual way from multiple-regression procedures will predict the outcome to be the same.

Thresholds are situation-specific. An individual's riot threshold is not a number that he carries with him from one riot to another but rather results from the configuration of costs and benefits, to him, of different behaviors in one particular riot situation. Inevitably, some situations will engage an actor more ideologically than others; one will seem more dangerous, one more exciting. What is argued is only that thresholds in many situations remain the same long enough for a predictable equilibrium to be reached.[9]

Great changes in behavior must not be confused with corresponding changes in threshold. We may imagine that individuals in a lynch mob are transformed by the situation—that their dispositions, values, and preferences are changed and deranged. The threshold model suggests instead that there is continuity of behavioral *dispositions* before, during, and after the lynching process—that mob members bring certain contingent dispositions to act into the situation (their lynching thresholds), and that while behavior changes, these do not.

This is not to say that there are not situations where individuals' central preferences and values (thus, most likely, their thresholds) do change. This may occur where people are subjected to great emotional shocks, as in religious revival meetings where people undergo "conversion experiences." But even this case is unclear; accounts of such meetings strongly suggest bandwagon effects, and those who undergo conversion often appear, on later analysis, to have been predisposed by a life of trouble and despair.

Thresholds may also change in the course of a situation because some-

[9] I am indebted to Lewis Coser for insisting on the implications of this point.

thing happens which changes the costs and benefits of the two possible decisions. In a riot, for example, the arrival of a heavy contingent of "law and order" forces may greatly increase the cost of participation; for those with a particular ideological orientation, however, this event may also increase the subjective benefits.

That thresholds may change because of emotional or situational changes does not invalidate the underlying model. It does, however, point up the importance of analyzing the stability characteristics of equilibria for threshold distributions. The persistence of riot behavior (or of any particular binary decision), in the face of changes which one might expect to have considerable impact on thresholds, may be difficult to explain if we do not understand that the initial threshold distribution had an equilibrium which was unusually resistant to perturbation.

The Falsification of Threshold Models

One may reasonably wonder whether there are not situations where the behavior of individuals cannot usefully be summed up and predicted by the proportions of others who engage in one or another of two possible behaviors. An extreme case is where individuals who appear to react to one another are actually all responding to an external influence. "Thus, if at the beginning of a shower a number of people on the street put up their umbrellas at the same time, this would not ordinarily be a case of action mutually oriented to that of each other, but rather of all reacting in the same way to the like need of protection from the rain" (Max Weber [1921] 1968, p. 23). But even Max Weber may be amended: there are surely some whose umbrella behavior is determined in part by that of others around them.

Here it is important to say that assigning someone a threshold of $x\%$ for raising his umbrella is not at all to say that the behavior of others is the most important influence on his behavior; it is only to make the statement that he will leave it closed until $x\%$ have opened theirs, then open it. The behavior of others may be only a marginal influence; but if the behavioral statement can be made, a threshold model can be constructed. A person in bad health, who detests being wet and wears a suit marked "dry-clean only," has strong nonsocial influences operating on his behavior. These are not ignored in the threshold model but rather contribute to the threshold itself: such a person's threshold will be lower, other things being equal, than that of someone not subject to such influences. Where the threshold model is of little interest, and may be said to have been, in effect, falsified, is where all or most thresholds are at 0% or 100%—that is, where most people's behavior is not, in fact, contingent on that of others.

A different difficulty arises if people do not make accurate judgments of the number or proportion of others who have made one or the other decision.[10] We may distinguish three different cases: (1) People may systematically misperceive the proportion of others who have, say, joined a riot, because of elements of the situation (ecological barriers) or personal characteristics (ideological "wishful thinking"). If this misperception is the same for all, as where everyone over- or underestimates by the same amount, the model is unchanged. Adjustments are not difficult if the level of misperception varies systematically with one's threshold—as when radicals overestimate and conservatives underestimate riot participation. If misperception is more random, the situation is more complex and the stability of the underlying equilibrium becomes particularly important. (2) Inaccurate judgments may occur because those who have decided to do something do not make their action public. What Merton has called "pluralistic ignorance" may occur where the costs of being known as one of a small number who, for example, adopt family planning, are seen as high. Thus, many thresholds may have been passed, but the actors who have them do not know this and hence do not act. Here, the threshold model is not only not falsified but is a valuable guide for the policymaker who wants to understand the forces which prevent full adoption of the innovation. (3) Individuals may be unable to make sufficiently fine distinctions for their true thresholds to have operational meaning. Someone with a riot threshold of 17% may be unable, for example, to distinguish among values between 15% and 20%; studies of human information processing suggest limits to the number of distinctions which can be made along one sensory dimension (Miller 1956). In such a case, the only threshold which has behavioral meaning is the lowest one which will be perceived by the individual as representing his true threshold. In this case, the operational threshold becomes 15% and enters the model in the usual way.

The threshold models described in this paper do require that one's reaction to others' behavior have a relatively simple form. Figure 3 graphs for some arbitrary individual, whose threshold is 38%, the net benefit to him (total benefit minus total cost) of joining a riot, for various levels of participation of the entire group. The threshold, as required by its definition, is the point where the net benefit first becomes positive. The curve is intentionally not monotonic, since the threshold concept does not require it to be. What the present models stipulate is that this curve not cross the x-axis more than once. If some individuals' curves do so, more complex models are needed. Suppose you are in an unfamiliar town and enter an unknown restaurant

[10] This paragraph has benefited particularly from comments of Robert Merton, Everett Rogers, Eugene Weinstein, and Larry Kincaid.

on Saturday evening at seven o'clock. Whether or not you decide to take a meal there will depend in part on how many others have also decided to do so. If the place is nearly empty, it is probably a bad sign—without some minimal number of diners, one would probably try another place. But the curve will cross the x-axis again at a later point—where the restaurant is so crowded that the waiting time would be unbearable. Some cautious individuals might join a riot when 50% of the others had but leave when the total passed 90% for fear that so large a riot would bring official reprisals. In principle, one may imagine the net-benefit curves of figure 3 crossing the x-axis any number of times; in practice, it is hard to think of examples where more than two seems reasonable. In the case of two, results can still be computed by forward recursion, but equilibria cannot be guaranteed and a somewhat different analytical apparatus would be needed. Empirically, if we found that aggregate behavior oscillated from one level of participation to another even though the situation seemed stable in its basic cost-benefit configuration, we might guess that at least some participants had net-benefit curves which crossed the x-axis more than once.
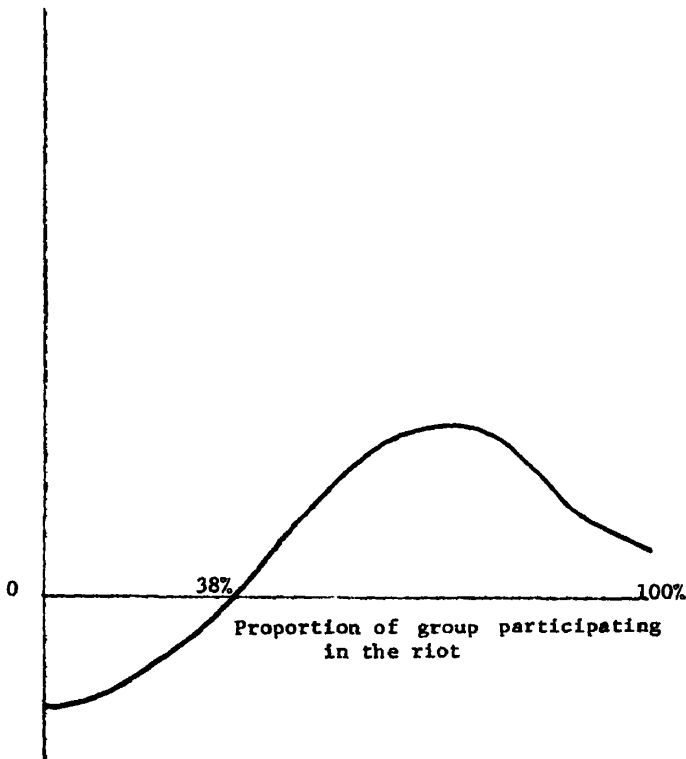
FIG. 3.—Net benefit to an individual, with threshold 38%, of joining a riot, plotted against the proportion of the group participating. (Total benefits minus total costs.)

Measurement and Verification

Because thresholds are behavioral dispositions, they are difficult to measure with confidence before the behavior actually occurs. The situation is similar to that in microeconomics, where the theory is built on the notion that each consumer or producer has a demand or supply schedule—a quantity of the commodity which he will buy or produce at any conceivable price. (Actors in threshold models have, correspondingly, a behavior they will enact for any possible distribution of others' decisions. In both cases, the schedules derive from underlying utility functions.) Since most possible prices are never observed, and economists rarely study the schedules of particular individuals directly, there is normally little direct evidence about these curves which provide the basis for microeconomic theory.

The two main factors which prevent this from causing impossible difficulties in economics are similar to factors operating for threshold models as well. First, much information about supply and demand schedules can be inferred from consumer characteristics, product substitutability, and aggregate outcomes, without direct measurement. Second, the most important thing to know about these schedules is not their exact details, but the level of change in prices to be expected when economic conditions change and the range over which the equilibrium price implied by the curves can be expected to be stable.

For threshold models, we can attempt to index an individual's threshold by the exact number of others who have made a decision before he does. Consider, for example, the adoption of an innovation in some rural village. A complete threshold distribution can be inferred from the exact time sequence of adoptions. This tautological procedure would not afford much explanatory power but could at least allow us to investigate the relation between thresholds and background characteristics, attitudes, values, and social positions. One could then regress thresholds on these various independent variables and use the resulting equations to predict threshold distributions—and hence outcomes—in villages not yet exposed to the innovation.[11]

Such a procedure carries at least two important potential sources of error: (1) Thresholds may be only imperfectly measured by one's position in the time sequence of adoption; measurement error, imperfect information, and chance personal events unrelated to the innovation may result in this. (2) Where the R^2 of multiple regressions falls much under 1.0, the predicted threshold distributions may vary significantly from the true ones. Villages whose distributions have highly stable equilibria might nevertheless yield good predictions from this procedure.

[11] I am indebted to Michael Hannan for this suggestion.

After-the-fact distributions also give us the opportunity to see what kinds of situations characterize villages which had successful versus unsuccessful levels of adoption. Of interest would be whether the results were largely impervious to change or whether small differences might have generated much higher or lower levels of success—as would be the case if distributions had unstable equilibria. Either would have important policy implications.

Dozier (1977) tested threshold models with data from 23 rural Korean villages which gave, for each woman, the month, if any, of first adoption of family planning practices over the period 1964–73. (These data are described more fully in Park et al. 1974.) He split the sample into 18 villages from which he computed multiple-regression equations for thresholds of individual women and five villages for which he used these equations to predict threshold distributions and thence, by forward recursion, outcomes. He then compared predictions obtained in this way with predictions obtained from simple-regression methods without threshold models, comparing both to the true outcomes.

Results suggest that when the multiple regression explains less than 50% of the variance in observed thresholds predictions are worse than those obtained from the standard methods. But as the variance explained edges over 50%, "the accuracy of the recursion model improves dramatically" and it predicts far better than the alternatives (Dozier 1977, p. 230). Many such predictions approach 100% accuracy in these data, which would be a powerful result from a model which predicts only 50% of the variance in thresholds. Caution is necessary, however, since some of the variables in the regression equations could not have been measured before the time period covered by the data and are highly correlated with thresholds in ways which deprive them of true explanatory power (see Dozier 1977, chap. 8).

While much more needs to be said about the problems of empirical application of these models, this is not the main intent of the present paper, which means only to summarize the theoretical development to date.

SUMMARY

This paper presents models of collective behavior, based on behavioral thresholds, which account for collective outcomes by simple principles of aggregation. The models are particularly valuable in helping to understand situations where outcomes do not seem intuitively consistent with the underlying individual preferences. Such "paradoxes" may occur far more than we realize, since we observe mainly outcomes and tend to assume that the preferences generating them were consistent with rather than opposed or unrelated to them. Our reluctance to recognize paradox is consistent with an everyday social construction of reality which construes social

systems as smoothly operating entities, without discontinuities or incomprehensible events (see Berger and Luckmann 1968).

By explaining paradoxical outcomes as the result of aggregation processes, threshold models take the "strangeness" often associated with collective behavior out of the heads of actors and put it into the dynamics of situations. Such models may be useful in small-group settings as well as those with large numbers of actors. Their greatest promise lies in analysis of situations where many actors behave in ways contingent on one another, where there are few institutionalized precedents and little preexisting structure. These situations are central in social life but not an important focus of theoretical analysis either in micro- or macrosociology. Providing tools for analyzing them is part of the important task of linking micro to macro levels of sociological theory.

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University Social Structure and Social Networks among Scientists[1]

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Findings are presented that describe the pattern of research communication among faculty in the six physical science departments of an elite American university. The findings provide a basis for modifying and extending Peter Blau's analysis of the relationship between university social structure and the pattern of communication among university faculty. Blau regards the formation of integrative multidisciplinary social networks within university communities as highly problematic; he suggests that academic departments are the primary site of integrative social networks within universities. My findings suggest that academic departments are not appropriate units for describing the pattern of research communication among university faculty, at least in the physical sciences, and that university social structure can foster an integrative social network which is multidisciplinary in composition. Proposals are introduced that relate facets of university social structure to the formation of integrative multidisciplinary social networks. A perspective on the role of universities in fostering the coherence of the scientific elite is outlined.

Nearly all investigators of social networks among scientists have looked at those connections that are based on the scientists' activities in a single subfield, specialty, or discipline (Breiger 1976; Crane 1969, 1972; Crawford 1970; Gaston 1973; Griffith, Jahn, and Miller 1971; Griffith and Miller 1970; Griffith and Mullins 1972; Mullins 1972; Mulkay, Gilbert, and Woolgar 1975; Price and Beaver 1966). Consequently, very little is known about the morphology of those social networks that involve scientists from different disciplines. There are some data which confirm the impression that the boundaries of research areas are very much open (Crane 1972; Crawford 1970). Metaphors such as "overlapping neighborhoods" (Polanyi 1962), "honeycomb structure" (Crane 1972), and "fish scales" (Campbell 1969) have been used to describe the general pattern of interdisciplinary communication that is believed to exist. But thus far only Nicholas Mullins (1966, 1968) has examined a social network composed of scientists at work

[1] This paper is based on the author's dissertation work, which was conducted at the University of Chicago. I am deeply indebted to Charles Bidwell, my principal adviser, and to Robert Dreeben, Mark Joseph, and the anonymous referees who supplied valuable comments.

in different disciplines (primarily in the biological sciences). Mullins's study of this multidisciplinary population suggests that it is joined by a structurally diffuse and loosely meshed social network.

Social network morphology may have important implications. It is plausible that scientific information of various kinds is transmitted more easily in networks where pairs of persons are on the average connected by a host of fairly short communication paths than in networks where pairs of persons tend to be connected by only a few paths of short length; accordingly, different social network patterns may be differently conducive to the diffusion of scientific knowledge and to the visibility of scientific role performance (see Cole and Cole 1968; Coleman, Katz, and Menzel 1966; Granovetter 1974; Kerckhoff, Back, and Miller 1965; Merton 1968, pp. 390–407). Different social network patterns may also be differently conducive to the crystallization and reinforcement of moral consensus among scientists and to the capacity of scientists to rouse themselves for collective action (cf. Festinger, Schachter, and Back 1950; Laumann and Pappi 1976; Mitchell 1969; Riley and Cohn 1958). Social networks have repeatedly been pointed to as significant phenomena; but at present this idea, that the morphology of social networks has important social correlates, is a gleam in the eyes of network theorists.

Although our knowledge of the relationship between network morphology and various social phenomena is rudimentary, network analysts tend to agree that larger numerical values on such morphological parameters as network density, compactness, and mesh are associated with socially integrative effects (Barnes 1972; Mitchell 1969). Elizabeth Bott perhaps most explicitly asserts this fundamental preconception of much network analysis:

> When many of the people a person knows interact with one another, that is when the person's network is close-knit, the members of his network tend to reach consensus of norms and they exert consistent informal pressure on one another to conform to the norms, to keep in touch with one another, and, if need be, to help one another. . . . But when most of the people a person knows do not interact with one another, that is when his network is loose-knit, more variation on norms is likely to develop in the network and social control and mutual assistance will be more fragmented and less consistent. [Bott (1957) 1971, p. 60]

Bott's perspective reflects a tradition in social analysis that has its roots in early sociometric studies (e.g., Moreno 1934) and in later graph theoretic formulations (e.g., Harary, Norman, and Cartwright 1965), a tradition which is now being vigorously carried forward in the work of certain sociologists (e.g., Granovetter 1973, 1976; Laumann 1973; Laumann and Pappi 1976) and social anthropologists (see the reviews of Barnes 1972; Mitchell 1969; Whitten and Wolfe 1973). My present work is informed by this tradition, which has pointed to the significance of social networks and their

variations in morphology. In light of this tradition, it appears problematic whether Mullins's data indicate the kind of social network morphology among scientists which can be an effective basis for the diffusion of scientific information across disciplinary lines or which Polanyi (1962) suggests might be the "seat" of a uniform scientific opinion.

No one has previously examined a social network composed of scientists at work in different disciplines within a single university. There are, of course, research-oriented universities that support concentrations of active scientists at work in a variety of disciplines. My present work was motivated by the idea that these research-oriented universities are likely sites for finding more structurally cohesive multidisciplinary networks than Mullins's data suggest exist in science.

Peter Blau (1973), who recently proposed that the academic departments of universities can be sites of cohesive social networks, is skeptical of the idea that universities can be loci of cohesive multidisciplinary social networks:

> The differentiation of academic institutions into specialized departments, far from integrating them by making them highly interdependent, weakens their integration by creating obstacles to communication among them. [P. 265]
>
> In other organizations, the differentiation of the common task into interdependent functions creates simultaneously a basis for integration, because the interdependence of parts requires them to cohere and helps integrate them. But the academic specialties in a university are not directly interdependent. The members of each can pursue their research and teaching independently of the work of others, and the high degree of specialization makes communication between different fields difficult. The fact—assuming it is a fact—that integration is more problematical in universities is ironical, since the very term "university" implies an integrated whole. . . . [P. 215]

In this paper I present some findings on the pattern of research communication among 128 faculty in the six physical science departments of an elite American university. I propose an explanation of how university social structure may act to foster the structural cohesiveness of multidisciplinary research networks. Finally, I speculate on some of the implications of my findings for the cohesiveness of the scientific elite.[2]

[2] The study deals with relationships in which, according to faculty reports, there is an ongoing substantive discussion of scientific ideas. Beyond these faculty reports, there are *grounds for confidence that a social network is being scrutinized which is as professionally* meaningful as those based exclusively on relationships among members of the same specialty or discipline. Pilot interviews, conducted with a small number of the faculty on the subject of the initiation of formal and informal research collaborations, first suggested that this university's faculty might draw extensively on the professional resources available not only *in their own departments but also in the other departments of the university.* Subsequently, it was found that at least 51% of the 128 faculty had published a paper

METHODS

The data of the present study were gathered from the population of assistant, associate, and full professors who had appointments in at least one of the physical science departments of a single university: astronomy, chemistry, geosciences, physics, mathematics, and statistics. These faculty were sent a questionnaire which listed the names of all other faculty in this population and requested the recipient to indicate those on the list with whom he had at least three conversations about *research problems* during the academic year. The possible number of survey respondents was 133; 71 faculty responded (53%). Analysis revealed that of the 133 faculty, 128 were linked in a single network of research communication, that is, a network in which each of the 128 faculty members was joined, directly or indirectly, to each of the other faculty members by one or more communication paths. The study deals with this network of 128 faculty.[3]

The network of physical scientists will be characterized as a whole, in terms of its density, compactness, and mesh. This network will be compared with another network of comparable size that consisted of scientists at work in a single specialty. This latter network I derived from Crawford's (1970) data on the pattern of research communication among scientists engaged in psychophysiological studies of sleep.[4]

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with a fellow faculty member and that at least 41% were involved in a formal collaboration at the time of the survey. (These data are based on an item in the survey which requested faculty to indicate those faculty members with whom they had published a paper and/or with whom they were currently engaged in a research collaboration.) The distribution along departmental lines of these instances of (past and current) formal collaboration is isomorphic with the distribution of informal research relationships: 39% of the informal relationships in the network are interdepartmental, and 35% of the formal collaborations occurred among these interdepartmental relationships. These faculty are actively engaged in research programs: during 1973–75, at least 84% of the 128 faculty members published one or more research papers, and of these at least 63% published three or more papers. It is also noteworthy that 21 of these physical scientists are members of the National Academy of Sciences. I believe, in sum, that an inquiry into the presence of a structurally cohesive social network among the membership of this kind of multidisciplinary population may be a worthwhile endeavor. Some possible implications of such social networks are alluded to in the paper. But it must be clearly recognized that it is not the design of this study actually to demonstrate the nature of outcomes that either periodically or constantly flow from multidisciplinary research networks possessing certain morphologies. The study's primary focus is on the extent and manner in which university social structure may foster the production of a cohesive multidisciplinary research network.

[3] A network with 128 members has 8,128 possible relations in it, where a relation is defined as an undirected path connecting two members through no intermediaries. The presence of a relation is indicated when a respondent names a person as an informal communicant. If a named person also acknowledges a relation with the respondent, we have a redundant, though reaffirming, piece of information on the presence of a relation. In the count of relations a reciprocal acknowledgment of communication produces one relation. The total number of such relations among the 128 faculty is 559.

[4] See Crawford's (1970, p. 79) map of informal communication relations among scientists in sleep and dream research: I focused on the large network of 160 scientists and eliminated

The network of physical scientists will also be examined by department: department networks (which are zones within the whole network) will be characterized in terms of their density, compactness, and mesh and compared with one another. Finally, department networks will be decomposed into parts that roughly correspond to specialty groups, and the pattern of relations among these specialty groups will be examined. In combination with department literature on the research pursuits of their faculty, hierarchical-cluster analysis (Johnson 1967) proved to be an invaluable aid in defining clusters with fairly homogeneous research interests.[5]

The three characteristics of networks examined in this study are density, compactness, and mesh. Density is the most widely reported of various network characteristics. It is a measure of how nearly a network approaches

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the network's peripheral members, i.e., those who have only one connection to the network. The effect of this elimination of peripheral members is to bias upward the density of Crawford's network. The other effect is to make the two networks, mine and Crawford's, equal in size: by an incredible coincidence the elimination reduced the size of her network to 128 members, the same size as mine. The exact wording of Crawford's questionnaire item is worth noting: "Are there scientists with whom you personally and frequently communicate information about your work in sleep and dream research? This does not include persons on a routine mailing list or student-teacher relationships, but individuals whom you often contact to discuss in a substantive way aspects of research or developments in this field, or to request an opinion. Please list all such persons whom you have contacted three or more times during the past year concerning your work in sleep and dream research" (p. 121). Aspects of Crawford's item may have led her respondents, in comparison with mine, to a more stringent interpretation of who were the appropriate persons to name as their communicants; furthermore, it should be recognized that I provided a list of persons for respondents to check and that Crawford requested her respondents to write in the names of their communicants. These features may have biased downward the density of Crawford's network relative to mine. At the same time, my network underrepresents the number of communication relations since it includes nonrespondents (Crawford's network consists entirely of respondents). I have no way of assessing the severity and direction of the overall bias that may be based on these and other differences between Crawford's and my methods. I am of the opinion, however, that the data are good enough to support a conservative interpretation, which is sufficient for my purposes.

[5] In combination with department literature concerning the current research pursuits of faculty, the hierarchical-cluster analysis was helpful in making decisions about (*a*) combining memberships of several small specialty areas into larger groupings and (*b*) assigning faculty members with several specialty-group affiliations to one and only one group. The cluster analysis was performed on the networks of the departments of chemistry, physics, and geoscience where decisions of the foregoing sort were called for. The cluster analysis requires a measure of the proximity of pairs; I tested out various measures by taking the results of the cluster analysis to faculty members and asking them whether their being clustered with particular other faculty members made sense in terms of a commonality of research interest. I should like to report that considerable success was finally achieved with the following measure of proximity: $A(100) + B + (C/1{,}000)$, where A equals one if a pair is directly joined and zero if it is not, B equals the number of paths through one intermediary joining the pair, and C equals the number of paths through two intermediaries joining the pair. There are parallels between this measure of proximity and the one being used by citation analysts to define homogeneous clusters of research articles. Consult the author for more details.

the state in which each member is directly linked to every other member. Density is calculated as the ratio of observed to possible direct relations between persons in a network.[6] With regard to a network's compactness and mesh, there are no conventions for measurement; accordingly, my use of them is briefly discussed.

Harary et al. (1965) distinguish between joining and reaching. The difference between the two is that joining ignores the direction of linkages while reaching does not. In a directed graph composed of arrows and points, one point is said to reach another if a path between them can be traced by following the direction of the arrows. In a general sense, joining refers to the existence of a connection. Both reaching and joining are accomplished in a certain number of steps: one step where there is a direct linkage, two steps where the linkage occurs through one intermediary, etc.

Accordingly, a network's compactness may be measured by calculating the cumulative proportion of the possible pairs of persons in a network that are joined, or reached, successively by one-step, two-step, three-step paths, and so on.[7] When a substantial proportion of pairs in a network are joined by fairly short paths, a network is compact; it is more compact than another in which a smaller proportion of pairs are joined by paths of comparable length. For example, a network in which 80% of its possible pairs are joined by paths of one or two steps is more compact than a network in which only 30% of its pairs are joined by paths of one or two steps. Under some circumstances, one might also say that in the former network persons on the average reach 80% of the population through paths of one or two steps, whereas in the latter network persons on the average reach only 30% of the population through paths of one or two steps.[8]

The distinction between network compactness and mesh can be expressed most clearly in terms of "ego-anchored" networks. For each ego or point in a network one may produce a "tree" involving other members of the network and showing the shortest routes between himself and others. Ego's tree expresses graphically his reach in the population. In figure 1 ego reaches 18 points in four steps. If the total population of the network, from which ego's tree is isolated, consists of these 18 persons plus ego, then ego's four-step reach is 100%. The tree in figure 1 does not necessarily involve all the

[6] Density equals $2A/N(N-1)$, where A equals the actual number of undirected links in a network and N equals the number of persons in the network.

[7] Only the shortest paths which join pairs are considered in this calculation.

[8] Average reach equals $(X_1 + X_2 + \ldots X_N)/N(N-1)$, where X_1, X_2, etc., are the numbers of persons that each person is joined to by paths of a certain length and N is the size of the network. The proportion of joined pairs equals $2A/N(N-1)$, where A equals the number of pairs joined by paths of a certain length and N equals the size of the network. $A = (X_1 + X_2 + \ldots X_N)/2$ in networks that can be represented by a symmetric adjacency matrix.
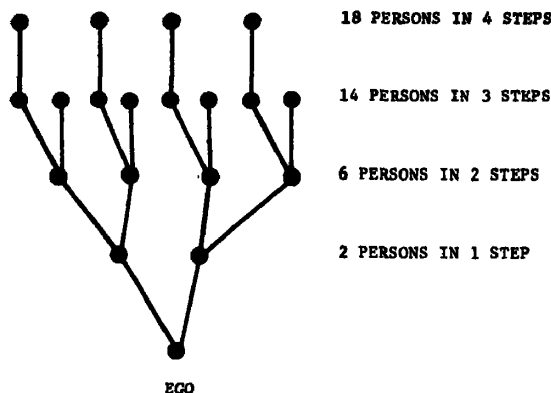
FIG. 1.—Graph of an ego's reach in a hypothetical network

connections present in a network, for members of ego's tree may be linked with one another; such links, however, are irrelevant in ascertaining ego's reach. It is from this standpoint that a distinction is made between reach and mesh. When there are many connections among members of ego's tree, his network is tightly meshed.

The measure of network mesh which I employ is based on the idea that a tightly meshed network is one in which pairs tend to be joined by multiple and preferably short paths. Accordingly, involved in the measure is the calculation of the average number of two-step paths which join the pairs that are joined by either one- or two-step paths. For example, if one- or two-step joined pairs of some hypothetical network are joined by an average of 10 two-step paths, whereas the one- or two-step joined pairs of another network are joined by an average of five two-step paths, the first network is defined as more tightly meshed than the second. Also, in order to assess a network's mesh, I calculated the number of three-step paths that on the average join all the pairs of a network that are joined by paths of three steps or fewer.[9]

The substantive implications of the distinction between network compactness and mesh are that, while reachability implies the opportunity for information transmission, the presence of multiple pathways implies a heightened probability of such transmission actually occurring.

SOCIAL NETWORK DENSITIES OF SEVERAL ACADEMIC DEPARTMENTS

Were the 559 communication relations in my sample distributed independently of department boundaries, 20% would have been intradepart-

[9] In the enumeration of joining paths, redundant paths are excluded, i.e., paths in which the same element occurs more than once. Ross and Harary's (1952) algorithm for finding the number of nonredundant paths up to four steps in length was used for this enumeration.

mental. Instead, 60% of the relations occur inside the departments: three times as many as expected. Although communication relations tend to fall inside department boundaries, they do not uniformly do so: the six physical science departments vary from 18% to 69% in the density of communication relations inside them (see table 1). With the exception of the mathematics department, the rule is that the larger the department, the lower its network density. In other words, the absolute number of observed relations between department members does not increase in direct proportion to the number of possible relations: there is an inelasticity in the increase of the number of relations between department members as the total number of possible relations increases.

In figure 2 a line is drawn which shows the number of relations required to achieve a 70% density (approximately the density of the two smallest departments) as the number of potential relations increases. The dots in the figure show the actual position of the six departments relative to this requirement. To be sure, the number of relations among department members does increase with department size, but this increase is not sufficient to maintain in the larger departments the density found in the smaller departments and so becomes a decreasing function of department size.

A negative association between network density and size is, of course, what we would expect when considering networks whose sizes vary considerably, since in progressively larger networks it becomes less and less feasible for persons to maintain direct and regular contact with all the other members of the network. However, in this sample of departments the range in size is not extreme; hence the inelasticity in the amount of department relations is somewhat surprising. For the moment, a discussion of data which bear on the explanation of this inelasticity is postponed in order to address some of the consequences of variation in the departments' network density.

TABLE 1

CHARACTERISTICS OF SIX PHYSICAL SCIENCE DEPARTMENTS WITHIN A UNIVERSITY

	Characteristics			
Departments	Network Density* (%)	Size	Intra-departmental Relations	Survey Non-respondents
Statistics......	69	9	18	5
Astronomy....	66	10	29	2
Mathematics...	21	21	33	11
Geoscience....	33	22	69	7
Chemistry.....	31	25	80	10
Physics........	18	41	109	22

* Network densities are adjusted for inequalities of survey response rate across departments.

CONSEQUENCES OF VARIATION IN DEPARTMENTAL NETWORK DENSITY

The greater the network density, the more compact is the social network inside departments. Figure 3 shows for each department the cumulative proportion of pairs that are joined by paths involving one-step, two-step, three-step, and four-step paths, respectively. The proportion of pairs joined by one-step paths is, of course, the network's density.[10] The rank order of network densities corresponds to the compactness of department networks. Mathematics is again the exception to the rule. Astronomy and statistics have the most compact networks, since no person in them is separated from others by more than one intermediary. The geoscience and chemistry departments also have relatively compact networks: 75%–80% of the pairs in them are joined either directly or indirectly through one intermediary. Physics and mathematics have the least compact networks; only one-half of the pairs in physics are joined directly or through one intermediary, and in mathematics only slightly more than one-half of the pairs are joined by paths involving three intermediaries or fewer.
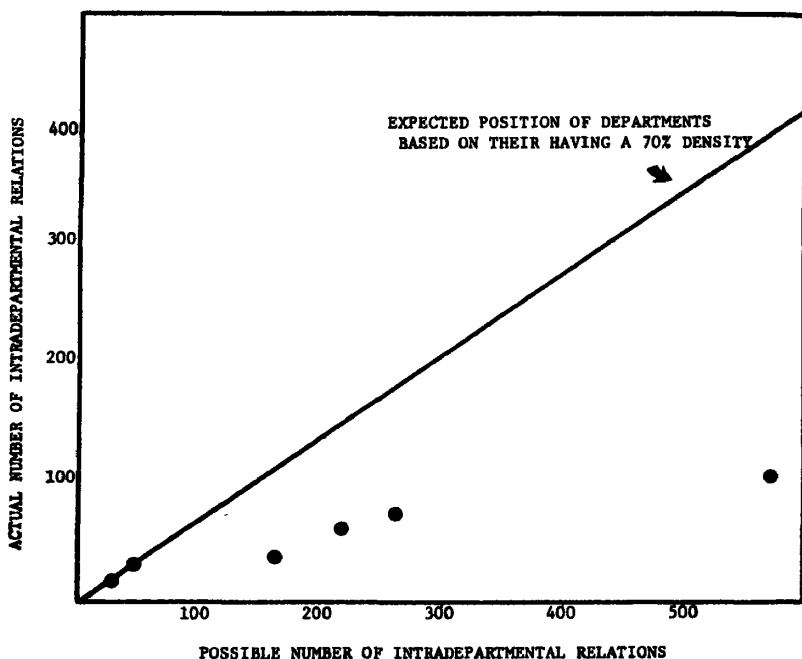
FIG. 2.—Inelasticity of increase in the amount of actual intradepartmental relations to increases in the amount of possible relations where the number of possible relations is adjusted for inequalities in the number of nonrespondent pairs.

[10] These densities are not adjusted for inequalities of response rate across departments, unlike the densities reported in table 1.

The anomalous position of the mathematics department is accounted for by a segmentation of communication relations within the department. As figure 4 shows, the department is divided into two clusters of faculty, composed separately of applied and pure mathematicians, and not a single research relation joins them. It is this segmentation that has reduced both the density and the compactness of the mathematics network. Apart from the special case of a segmented network, the factor which determines a network's compactness appears to be simply the absolute amount of relations maintained within a network's population relative to the population's size.

In mathematics and in physics, which have the least compact and least dense networks, faculty pairs are joined by fewer paths than are faculty
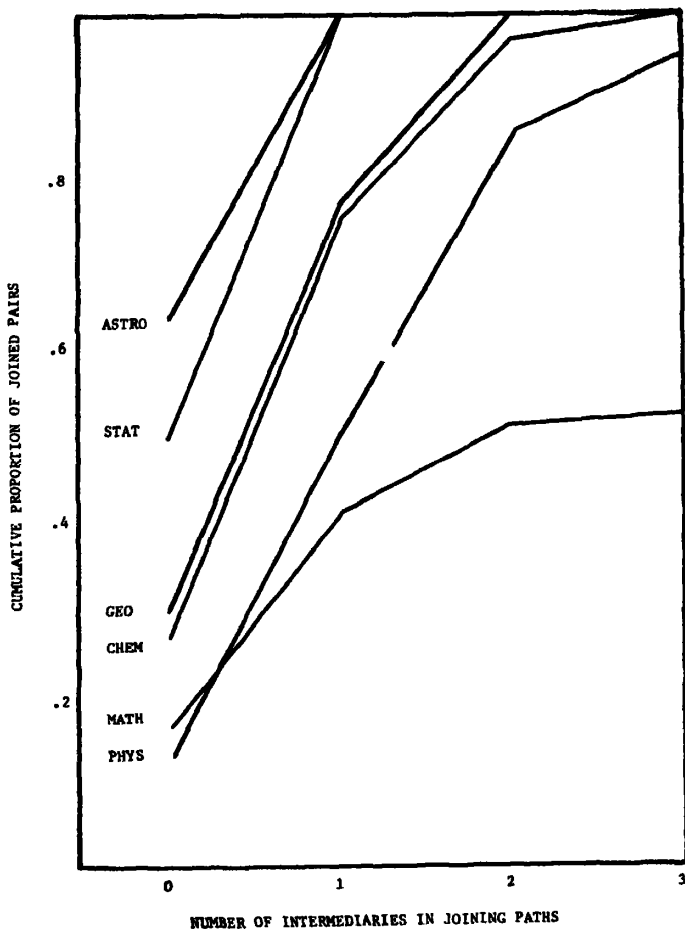
Fig. 3.—Compactness of department networks as measured by the cumulative proportion of pairs that are first joined by paths involving zero, one, two, and three intermediaries, respectively.

PURE MATHEMATICIANS

APPLIED MATHEMATICIANS
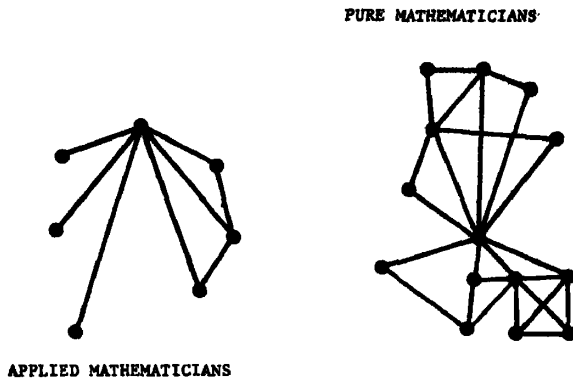
FIG. 4.—Segmentation in the mathematics department

pairs in the other departments. In mathematics, for example, pairs that are joined by one- or two-step paths are joined on the average by one two-step path, whereas in chemistry, pairs that are joined by one- or two-step paths are joined on the average by three two-step paths. The difference in mesh between the mathematics and chemistry networks appears more substantial when pairs joined by paths of three steps or fewer are considered; these pairs in chemistry are joined on the average by 16 three-step paths, whereas in mathematics such pairs are joined on the average by two three-step paths. These data and the comparable data for the other departments are shown in figure 5.

In the case of mathematics and chemistry, a difference in network density is positively related to a difference in network mesh. Similarly, the somewhat higher density of astronomy relative to statistics is positively related to the number of joining two-step paths—three in astronomy versus two in statistics—and this difference cascades into a substantial difference in the number of joining three-step paths: 14 in astronomy versus six in statistics. However, the mesh of statistics relative to chemistry, geosciences, and physics is unexpected; moreover, the slightly higher density of geosciences relative to chemistry is not associated with a higher number of joining paths. The relationship between network density and mesh, in other words, is not as neatly formulated as is the relationship between network density and compactness.

Additional parameters besides sheer density clearly enter into the determination of network mesh. In very small networks, for example, the absolute number of indirect paths of two or more steps is radically constrained; thus it is that in figure 5 astronomy and statistics, which have only 10 and nine members, respectively, are unable to maintain their higher mesh relative to the chemistry and physics departments when paths of three steps are considered.

Other factors affecting network mesh must affect the arrangement of relations in a network. In networks of comparable size, density, and compactness, different arrangements of relations would result in considerable differences in network mesh. But in suggesting that other factors besides density determine the mesh of a network, I do not mean to minimize the importance of the density factor. I believe, on the contrary, that differences of density must play a considerable role in determining mesh, but this role has not been adequately revealed by my data. The absolute number of communication relations relative to a network's size ought to place a constraint on the possible degree of mesh, and it must be within the context of such constraint that differences of pattern affect network mesh. In other
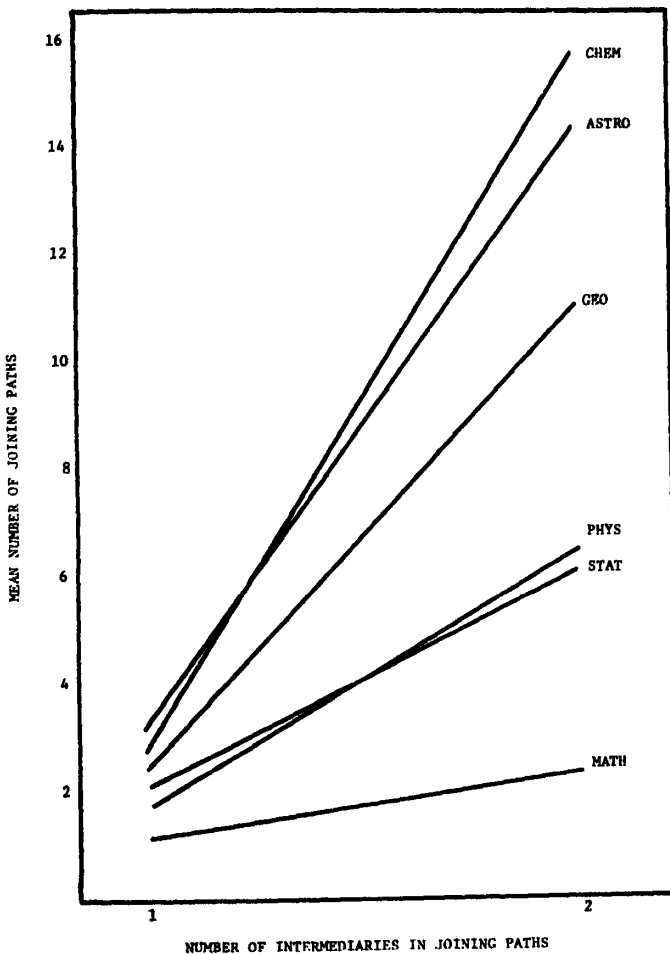
Fig. 5.—Mesh of department networks as measured by the mean number of paths of different length that join pairs.

words, one may presume that density (or the absolute number of relations between the members of a population relative to the population's size) remains the paramount factor in determining network attributes.

In this light, recall that the analysis has shown an inelasticity in the amount of intradepartmental communication relative to increases in the size of the departments. It is worthwhile, accordingly, to explain the basis of this inelasticity. The obvious explanation, which I shall explore, is that a large department does not necessarily provide more opportunities to its members for informal collaboration than a smaller department if the large department is composed of different research specialties.

SPECIALTY CLUSTERS INSIDE DEPARTMENTS

Figure 6 presents the sociograms of the astronomy and statistics departments; I suggest that these networks may be treated as two specialty clusters which happen to have department autonomy. I will show that the larger departments are simply collections of such clusters and that communication relations tend to fall inside clusters rather than inside departments: these twin features of department networks would account for the inelasticity of intradepartmental communication.

The presence of specialty clusters inside departments has already been
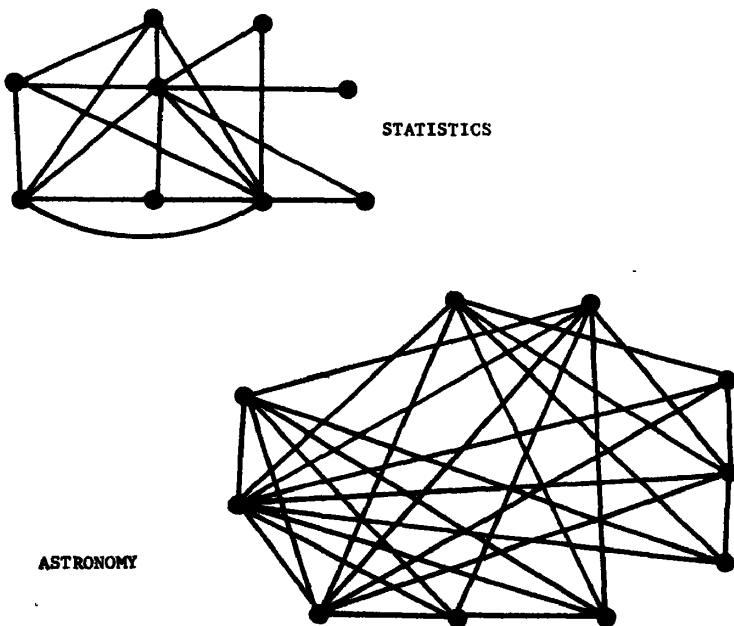
Fig. 6.—Social networks of the astronomy and statistics departments

shown in the case of mathematics where two clusters were described, composed separately of applied and pure mathematicians. Since not a single communication relation joins the two clusters, affiliation with the same department is clearly not in itself a sufficient condition for a social network to extend throughout a department's membership.[11] The segmentation which is seen in mathematics does not occur in any of the other departments and is probably a rare phenomenon; I suspect that research pursuits of department members typically overlap to generate networks without segmentation.

With hierarchical cluster analysis plus department literature about the research pursuits of faculty members, clusters of faculty having fairly homogeneous research interests were defined. Three clusters were defined in chemistry corresponding to (1) geochemistry, nuclear, and cosmochemistry; (2) physical chemistry and chemical physics; and (3) organic, inorganic, and biological chemistry. Five clusters were defined in physics corresponding to the research areas of (4) high-energy physics, (5) solar physics, (6) atomic and molecular physics, (7) astrophysics, and (8) solid state physics. Three clusters were defined in geoscience corresponding to the areas of (9) paleontology and stratigraphy, (10) fluid dynamics, and (11) a conglomeration consisting of geochemistry, geology, mineralogy geophysics, and crystallography. Including (12) astronomy and (13) statistics, as well as the two clusters in mathematics, all but one of the 128 faculty members were uniquely assigned to one of 15 specialty clusters.

Figure 7 plots the network densities of the specialty clusters (denoted on the figure by stars) and the cluster interfaces (filled or empty circles) against the number of possible relations in a cluster or at a cluster interface. Cluster interfaces consist of pairs of faculty who belong to different clusters. There are two types of interfaces: those, denoted on the figure by empty circles, which involve two clusters from the same department (e.g., in the physics department, the interface of solid state and high-energy physics) and those, denoted on the figure by filled circles, which involve two clusters from different departments (e.g., the interdepartmental interface of the physical chemistry and the solid state physics clusters).

Figure 7 makes two statements. First, with the exception of one specialty cluster (pure mathematicians), all the network densities of the clusters are higher than the densities of the cluster interfaces. In other words, communication relations tend to fall inside the clusters. Second, the densities of intradepartmental cluster interfaces are not consistently higher than the

[11] I should perhaps underscore the fact that this study deals with research relations. Consequently the segmentation which occurs in terms of these mathematicians' research interests does not necessarily occur in their nonprofessional associations with one another.
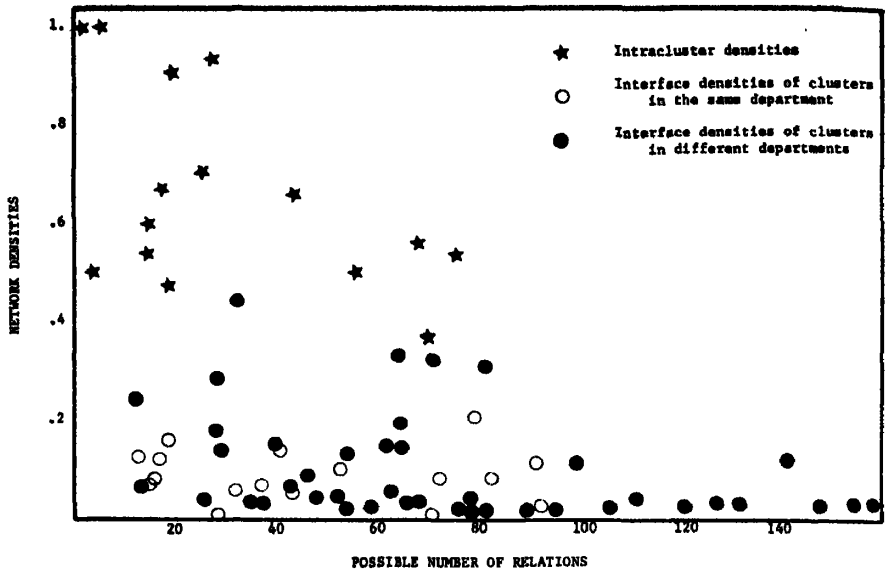
FIG. 7.—Network densities of research clusters and their interfaces controlling for possible number of relations where the number of possible relations is adjusted for inequalities in the number of nonrespondent pairs. Excluded from the figure are 46 densities that are interface densities of clusters in different departments and that have density values of zero.

densities of the interdepartmental cluster interfaces. Indeed, the six highest interface densities involve clusters from different departments. This suggests that department boundaries have little effect on the pattern of intercluster relations.

There thus appears to be a distinctive framework of research relationships among the physical scientists. That is to say, there are nodes in the network which are composed of faculty in the same department who have similar specialty interests and which are constituted by a greater thickening of interpersonal research relationships relative to the background network of these relationships (see the observed tendency for research relations to occur between members of the same specialty). Among the nodes there are certain major lines of interaction; these major interactions involve faculty who belong to different departments and are constituted (like the nodes themselves) by a greater thickening of research relationships relative to the background network (see the observed tendency for the relationships to fall on the interdepartmental rather than the intradepartmental interfaces). In sum, it appears that the physical science sector of this university may be conceived simply as composed of specialty clusters inside of which research communications tend to occur and which are interrelated with one another independently of department boundaries.

MULTIDISCIPLINARY INTEGRATION INSIDE THE UNIVERSITY

While almost half (48%) of the total number of research relationships fall within the specialty clusters, only 12% of the relationships occur between the specialty clusters of the same department. A substantial amount (39%) of the research relationships in the network are interdepartmental. The major framework of research relationships—consisting of the specialty clusters and the six strongest interdepartmental cluster interfaces—accounts for 65% of the total number of the relationships. There is, in other words, a substantial number of crosscutting research ties in this network: 23% of the total number of ties not only crosscut department boundaries, but also crosscut the main lines of research interaction between the specialties of different departments.[12] These data suggest a basis of structural cohesion in academic communities that is more inclusive than the departmental units Blau has considered. Accordingly, an explanation is proposed on the issue of how parameters of university organization may foster a multidisciplinary integration based on structurally cohesive research networks.

In a massive and decentralized scientific enterprise such as exists in the United States, a large portion of scientists' professional contacts inevitably involve persons who are outside the organizations where they are located. Nevertheless, the larger and the more heterogeneous the body of active researchers within a university, the greater will be the absolute amount of intra- and interdisciplinary exchange occurring within it: when two fields of science are joined by a sharing of techniques, this connection can be manifested inside the universities that support scientists in both fields. In universities that support active researchers in many fields, a host of such connections between fields can be manifested. The larger the average size of the fields inside a university, the greater should be the absolute number of opportunities for research exchange. The formulation above is hardly problematical.

What is problematical is the amount of absolute increase in research exchange that we can expect to occur with increases in the average field size and the diversity of fields inside a university: the formation of a compact and tightly meshed multidisciplinary research network within a large university would require the occurrence of a considerable amount of exchange among its faculty. Accordingly, I would like to suggest something in addition to the idea that the average faculty member of a large heterogeneous university will less frequently have to go outside his university to find research colleagues and technical resources than will the faculty members of smaller, more homogeneous universities.

[12] Intracluster relations (269), relations on intradepartmental interfaces (69), relations on the six strongest interdepartmental interfaces (93), relations on the weaker interdepartmental interfaces (126), relations of the faculty member who was not assigned to a cluster (2), total: 559 relations.

University social structure may foster the production of a structurally cohesive research network among its faculty by conditioning the choice of those colleagues to whom faculty members go for advice and technical resources. Hypothetically, what is the probability that a scientist who is seeking a particular resource will go to the one faculty member on his own campus who possesses it, if nine other scientists scattered about the country also possess the resource? Usually, I suspect that the probability would be more substantial than one in 10, that is, that the attractiveness of potential suppliers of scientific resources would vary according to whether they are located inside or outside the same university.

Geographical proximity is, of course, one factor that might underlie the preferential selection of fellow faculty as exchange partners, for collaboration between persons who are in close proximity is often more convenient than long-distance collaborations. Even though long-distance collaboration is feasible, closer tabs can be kept on the progress of research and the motivations of its participants in short-distance collaborations. The advantages of short-distance collaboration, which are probably fairly trivial under "normal" circumstances, may become considerable when the research problem that is the subject of a collaboration is of a nonroutine nature or when competitive pressure is severe.

The culture of a university is an additional factor that might underlie the preferential selection of fellow faculty members as research-exchange partners. A scientist going to a fellow faculty member may be more sure of the response he will get to his request for advice or resources, for shared university citizenship often entails a tacit obligation of receptiveness, if not compliance, to requests for assistance from within the community. Furthermore, persons within the same faculty may be preferred as informal collaborators because they can usually be trusted to guard the crystallizing ideas involved in tooling up for a research project. While scientists with research in progress need not in general be secretive (cf. Hagstrom 1967), they have possibly a greater need of secrecy during the initial stages of a research project: if a scientist can establish a lead on his potential competitors, it is unlikely that they will be willing to commit the resources and energy required to catch up to him (unless of course the problem is considered worth the risk or they believe that the leader's approach is in error). The culture of a university—the strength of which is reinforced by a university community's capacity to impose, if need be, social and material penalties on deviants from its culture—makes the university a place where new ideas and technologies can be explored and developed with relative ease and security.

In sum, I have argued not merely that a university is a place where research exchange occurs more or less frequently depending on the uni-

versity's size but that it further promotes the amount of exchange among its faculty by presenting a field of opportunities that, for a variety of reasons, powerfully intervenes (see Stouffer 1940) between a member of its science faculty and other opportunities existing elsewhere.

An important issue still remains: that is, even if we accept the pertinence of this intervening-opportunities formulation, to what degree can we expect university social structure to promote the cohesiveness of a multidisciplinary research network? Is the overall effect of university social structure trivial, or can it promote a high level of cohesiveness?

While I cannot provide a definitive answer to this question, I do find that, taken as a whole and in comparison with an equally large network of researchers communicating about problems in a single specialty (Crawford 1970), the physical science network is more compact and tightly meshed (see table 2). Persons in the physical science network are on the average joined to 40% of their fellows by one- or two-step paths, whereas persons in the sleep-research network are joined on the average to 18% of their fellows. Pairs of researchers who are joined by one- or two-step paths in the physical science network are joined by an average of two two-step paths, whereas in the sleep-research network such pairs are joined by an average of one two-step path. The difference in the two networks' mesh appears more pronounced when longer paths between joined pairs are dealt with.

Thus, on the basis of the measures utilized in this study, the communication network of a diversely specialized population of physical scientists is more structurally cohesive than one composed of more homogeneously specialized scientists. Specialization per se does not appear to constrain automatically and severely the possible degree of structural cohesion (see Durkheim 1933). These data suggest, therefore, that settings may exist in science where a fairly high degree of multidisciplinary integration is systematically fostered and that the research-oriented universities provide likely sites of such integration.

TABLE 2

COMPACTNESS AND MESH OF THE PHYSICAL SCIENCE AND SLEEP RESEARCH NETWORKS

Number of Intermediaries in Joining Paths	Cumulative Proportion of Joined Pairs (%)		Mean Number of Joining Paths	
	Physical Scientists	Sleep Research	Physical Scientists	Sleep Research
None	7	4	...	...
One or less	40	18	2.3	1.4
Two or less	76	47	13.9	3.2
Three or less	96	75	134.0	11.9

CONCLUSION

I believe that universities of the type examined here are in general good places to find structurally cohesive multidisciplinary research networks. To be sure, the university chosen as the setting of this research is unlike other universities in important respects. It is one of a small set of prestigious universities that are able to carry out doctoral training and to support distinguished faculty in each of the central fields of scientific training and research. Moreover, this university differs in important ways from the majority of the most prestigious universities. It is not as large as many of these universities, and it has institutionalized arrangements, particularly among its physical science faculty, that purportedly foster an unusual amount of interdisciplinary communication.[13] In other words, there are few other settings in which the occurrence of a cohesive multidisciplinary research network is more likely. If, therefore, such a network is not present in the physical science sector of this university, I believe that it will not be found with substantial frequency elsewhere (that is to say, a negative finding in this setting can be taken as presumptive evidence against the occurrence of structurally cohesive multidisciplinary research networks elsewhere). Of course a single positive finding may not be generalized: the finding of a cohesive network among the physical science faculty of this university does not imply that equally cohesive networks are to be found elsewhere. Accordingly, while keeping in mind that this university's network could be unique with respect to its morphological attributes, I suggest that parameters which are common to university organization may be more influential than idiosyncrasies of university organization in determining the structural cohesiveness of the research network that I have examined. Whether such is the case is a matter for further empirical investigation. It is to be hoped that this study warrants further research designed to elaborate or refute my findings and proposals.

I should like to close by suggesting the outlines of a broader viewpoint on the occurrence of cohesive multidisciplinary research networks inside universities. The production of these networks inside the universities where the membership of the scientific elite is concentrated is possibly not as problematical as is the production of a cohesive network composed of these

[13] It falls outside the feasible, and intended, scope of this paper to assess the significance of those factors which may uniquely distinguish this university from others. The thrust of the paper is to point to the existence of a structurally cohesive multidisciplinary research network within a university community and to describe this network's morphology. The presence and structure of the network have been interpreted on the basis of parameters which are common in university organizations. In so doing, I have sought to open further the door which Peter Blau (1973) has pried open for the large-scale network studies which will be necessary in order to assess accurately the variety of factors, and their relative importance, which affect the pattern of informal relations among university faculty.

concentrations of the elite at different universities. That is, the presence of cohesive multidisciplinary networks within the elite universities, although probably a necessary condition, is not a sufficient condition for a structurally cohesive interorganizational network that involves the total (or a substantial fraction) of the basic science elite. To be sure, evidence of various studies of invisible colleges suggests that the members of the elite within the same discipline or specialty keep in touch with one another regardless of their geographical location. Consequently, these invisible-college associations should be a basis of connectivity among university communities. But it is reasonable to suspect that, as the American academic system has become more massive and decentralized, the interorganizational network which involves the scientific elite has become progressively less compact and tightly meshed. If such is the case, then alongside Ben-David and Zloczower's argument concerning the benefits of academic decentralization with respect to scientific innovation (Ben-David 1960; Ben-David and Zloczower 1962) there might be placed a complementary argument about the costs of academic decentralization with respect to the social cohesion of the scientific elite.

Some initial steps toward such an argument have already been taken by Mulkay (1976), who suggests that the concentration of the British scientific elite in a few universities has fostered their cohesion and, in turn, their ability to resist governmental threats to scientific autonomy. While, as Mulkay suggests, a cohesive elite may be of importance in considering science's relationship to polity, I should also think that a cohesive elite is of importance in considering the internal affairs of science. The present paper, however, is not the place to engage this controversial issue of whether a cohesive scientific elite (composed of the elites of different scientific fields) is a phenomenon of importance to science. But this issue clearly merits further study. Especially now—when questions about the equitable distribution of scientific resources are compelling attention—we ought forcefully to address the issue of on what bases, if any, it is important to maintain universities that are centers of excellence across a broad range of scientific fields and the issue of whether the further decentralization of the scientific elite is a process which will have significant negative consequences for the scientific institution.

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The Structural Sources of Adventurism: The Ca
of the California Gold Rush[1]

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Adventurism, as a type of risk behavior, is a form of social ɛ having considerable historical and sociological importance an that has not been adequately studied. As an attempt to define of the sociological dimensions of adventurism, this study preser analysis of "adventure" migration to the California gold Through a comparative examination of the social origins o California argonauts, several conclusions are offered: (1) structure influences differentially the amount and kind of risk p are willing to take. (2) The tendency to engage in adventuri directly related to intense status involvement in one's native so (3) Because it consists of individuated actions taken in purs particularistic goals, adventurism undermines disciplined, orga group activity.

This paper attempts to analyze sociologically the actions of a ty individual called the "adventurer."[2] In learned parlance this label is ap to a variety of people whose circumstances differ considerably. Partici in the "Age of Discovery," as well as modern historians, have found tl apt title for the European explorers of the New World and for the ma merchants traveling to the Spice Islands and beyond to Cathay. ' adventurers are a distinctive type of migrants who seek wealth and beyond the geographical, but not the social, confines of their own so But "adventurer" is also an appellation applied to persons who seek social advancement without migrating. The courtier, the courtesar politician, the commercial speculator, and a variety of other type occasionally called adventurers. Thus the factor determining the a priateness of the label is not the occupancy of a particular role or eve performance of a general set of actions. Instead, it is the combination

[1] I would like to thank Ruth Dixon, John Lofland, Pierre van den Berghe, Neil Sı Ben Orlove, Bruce Hackett, and two anonymous reviewers for their comments earlier draft of this paper. I am, of course, responsible for the remaining errors.

[2] Insofar as I am aware, no attempt has been made to conceptualize the adventur sociological type, other than Weber's (1958, p. 58) brief characterization. Numerou cepts have been developed which directly relate to the adventurer. This is parti the case with the "sojourner" (Siu 1952; Bonacich 1973) and the "stranger" (S 1950).

act and the motivation behind the act. For the purposes of this study, the combination of the act and the motivation will be termed "adventurism."

Adventurism, accordingly, has two definitional components. First, it refers to the adventure, the act of taking great risks whose outcomes are not calculable in advance. Second, it signifies that such actions are undertaken for sizable social, political, or economic gains that might occur if the venture is successful. Because of the nature of the odds involved as well as the nature of what is being gambled (one's personal safety), risk taking of this type differs qualitatively from the risk taking associated with bourgeois capitalism. Max Weber (1958, pp. 20–21, 58), in particular, has shown this distinction to be of major historical importance by linking rational risk taking to the "spirit of capitalism" and adventurism to plunder (booty capitalism). By concentrating his efforts on the analysis of capitalism, however, Weber provided only few insights into adventurism. This paper can be seen as an attempt to augment somewhat our understanding of this type of high-risk behavior.

Defined in this way, adventurism is a historically common, sociologically important form of social action. At first glance, however, it is a type of action that defies sociological understanding, primarily because it blends two characteristics not often associated with one another: charisma and worldly gain.[3] Like the charismatic, the adventurer departs from the routine expectations of everyday life, appeals to chance to guide his way in the face of uncertainty, and engages in concerted though often sporadic activity. He is often a tradition breaker. But, like the entrepreneur, the adventurer is oriented to this world—to the attainment of worldly goods and accolades and to the manipulation of people and circumstances insofar as conditions permit. The active search for sudden wealth or recognition, perhaps the most common forms of adventurism, simultaneously circumvents the established modes of upward mobility and substantiates the importance of the social climb. To undertake such risks requires audacity—a foolhardiness or courage, depending on one's perspective—that is not found in every individual or even in every stratum of society. Because this behavior can be considered marginal and, at times, antagonistic to normative social activity, adventurism is an important source of social change. Yet, as a social phenomenon, adventurism is little understood, a fact perhaps indicating the diversity of the actions that could be called adventurous, of the outcomes that result from such actions, and of the individuals who attempt them.

This study is an exploratory investigation of adventurism in the context

[3] In several locations, Weber (1951, pp. 86, 104; 1958, pp. 20–21, 58; 1968, pp. 244–45) briefly mentions this association in the context of booty (adventure) capitalism. Also see Bendix (1962, p. 306).

of a single historical event, the California gold rush. The purpose of examining this event is to arrive at some preliminary understanding of the social factors influencing individuals to engage in adventurism and hence of the underlying sociological dimensions of adventurism. The following essay will be structured with this purpose in mind. First, the gold rush is examined as an instance of what will be termed adventure migration. The effort here is to show that adventurism is a patterned activity with individuals from some class positions and from some societies being more likely to be adventurers than individuals from other origins. Second, an attempt is made to explain why particular types of people or people in particular types of societies are more inclined toward adventurism than other types. To fashion such an explanation, adventurism is viewed as a mobility strategy, a course of action designed to alter one's social standing. The explanation then revolves around what stratificational conditions influence the use of this strategy as opposed to less adventurous strategies. The study concludes with a comparison between adventurism and bourgeois capitalism as types of rational calculated risk taking.

THE SETTING: MIGRATION TO THE GOLD FIELDS, 1849–51

The first great surge of worldwide migration to the United States during the 19th century came as a direct result of the discovery of gold in California (Thomas 1973, p. 94). As compared with the waves of migration that followed later in the century, the initial years of the gold rush (1849–51) are not typical. In fact, in the history of world migrations, the California gold rush is quite unusual. Seldom has a more rapid accumulation of a more ethnically diverse or more totally male population converged on a location where so few facilities existed for newcomers, each of whom was intent upon achieving the same goal—to strike it rich (Paul 1947, 1967; Caughey 1975).

There are other characteristics that further separate this migration from the typical one. For the majority of the early arrivals, it was a temporary sojourn, often lasting only one or two years. This was a "focused" migration, based more on the chance for sudden wealth than on the undesirability of the place of origin. Perhaps its most unusual characteristic was the suddenness of the rush to California. Tens of thousands of individuals almost simultaneously made the decision to migrate without the benefit of preexisting migration chains to organize their movements and with only minimal information of conditions to be encountered upon arrival.

Given the distance and conditions of travel, the migration was rapid, massive, and temporary. At the time of the discovery, the population of California totaled around 15,000, excluding the native Indians (Wright 1940, p. 323). By the end of 1849, after the international gold rush had been

under way for nearly six months, the total population had passed 90,000. By 1852, it had reached well over 220,000 (Loosley 1971). Of these migrants, most were between 20 and 40 years of age, and over 90% were male. By 1850, the second year of the gold rush, individuals from over 40 different nations and from every state and territory in the Union had landed in California (Wright 1940, pp. 338–41). During the initial years the great majority of these migrants were concentrated in the gold fields, mostly mountainous terrain inhabited before 1848 only by the native Indians. And approximately 70% of the numbers arriving in San Francisco by sea (over 175,000) between 1849 and 1853 had left California by the same means before the end of 1853 (Wright 1940, p. 341). No figures are available for those who left California overland or who—by accident, disease, or design—perished there during these years.

The singularity of this migration, as compared with other migrations of comparable size, stems from its being almost totally composed of adventurers—individuals who had made the decision to quit temporarily their affairs in their places of origin in order to search for sudden wealth in California. According to W. Petersen (1970, p. 63), the "free migration of individuals," a type similar to that which is here termed adventurer migration, is "always rather small." Because the gold rush is one of the few exceptions to this rule, it illustrates on a large scale that which usually occurs on a small one. For this reason, the California gold rush provides one of the best contexts in which to study adventurism.

SOCIAL AND GEOGRAPHICAL CHARACTERISTICS OF THE ARGONAUTS

What sorts of individuals are able to adopt adventurism as a means to attain their worldly ambitions? In what type of society is adventurism likely to be adopted as a strategy of upward mobility? In other words, if one views adventurism as a means to an end, then what social factors influence the use of this strategy as opposed to alternative and less adventurous strategies? Consider, more concretely, the rush to the California gold fields. What made the possibility of discovering gold and becoming rich so attractive to some individuals that they knowingly took several years out of their lives in the society of their aspirations to pursue the venture? Although these questions cannot be easily answered, they seem to be central to a sociological understanding of adventurism.

The data that can be teased out of the voluminous primary and secondary materials on the gold rush do not permit definitive answers. Although these materials record life in the gold fields in considerable detail, they offer little on the argonauts' motivations for migrating and on their plans for using their anticipated wealth. Nevertheless, census data and other

primary sources permit descriptions of the demographic characteristics of the argonauts,[4] from which one can approach the questions indirectly.

Most eyewitness accounts of the gold rush suggest that the migration to California was more likely to be undertaken by certain types of individuals and by individuals from certain societies than by others. First of all, there was an upper-class bias to the migration; those who came by sea (80% of the total) usually had to pay for their own passage in advance. Said one observer in 1850 (Nasatir 1964, p. 122):

> This mining population is well worthy of interest. In it all levels of civilized society are represented, intermixed, and it is not at all those on the lowest rung of the social ladder who are the most numerous; on the contrary they are the fewest because everyone cannot afford such a trip. . . . Engineers are in good company with scholars, professors, artists, men of letters, doctors, captains who have left their ships, officers who have abandoned their rank, and lawyers their cases. Everyone has rushed here: merchants, workmen, clerks, women, old men, children, everyone, from the peasant to the man of the world and even (who would believe it?) the *rentier*, who hoped to triple his income and who is eating it up.

The fact that most accounts of the gold rush were written by fairly well-educated people probably exaggerates the impression of the argonauts' upper- and middle-class origins. Nonetheless, such empirical data as exist on the class origins of the migrants substantiate this impression (Bancroft 1888, pp. 227, 248–50; Read and Gaines 1949, pp. xxxix–xl). For instance, some of the early accounts of the gold rush list the members of companies traveling to California. In 1849 from Ohio, one "company going to the promised land . . . consisted of a son of the Hon. Thomas Ewing, Secretary of the Interior . . . a constable . . . a State Attorney . . . a shrewd lawyer . . . a farmer . . . a merchant . . . a druggist . . . a potter" (Morgan and Scobie 1964, pp. 2–3). In the most extensive studies of gold mining companies, O. T. Howe (1923, p. 5) notes that "of professional men there were always an abundance." Most companies included physicians, lawyers, and clergymen in addition to skilled craftsmen.

Precise information on the class origins of foreign-born migrants is not available. A number of observers, however, did comment on the types of upper- and middle-class individuals found in California. From France came many of noble birth (Nasatir 1964, p. 20); from England, many "gentlemen"; from Chile, many upper-class *mestizos* (Monaghan 1973, pp. 42–53; Faugsted 1973, p. 20; Giacobbi 1974, pp. 7–8); from Germany and America,

[4] It should be noted that the 1850 U.S. census data for California are judged unreliable (Bancroft 1888, p. 158; Loosley 1971) and thus can be used only to give indications of patterns.

many professionals (Howe 1923, pp. 4, 180; Read and Gaines 1949, p. xxxii); and from all these places, many artisans and merchants.

Individuals from lower-class origins were also present in California, but the poor who did not come overland often traveled at the expense and for the benefit of others. Upper-class Chileans brought paid laborers with them to mine their gold (Giacobbi 1974, pp. 18–23; Monaghan 1973, pp. 74, 173). Although not peons in the sense that they were bound to their master by ties of patronage, such paid laborers formed a sizable percentage of the Chilean argonauts.[5] Some sea captains even booked these laborers without passage money in order to sell them as bond servants in San Francisco (Monaghan 1973, p. 53). Once in California, Chilean masters formed associations designed to prevent desertions of their laborers, though often to no avail (Monaghan 1973, pp. 61, 127).

France, like Chile, contributed many poor argonauts whose passage was primarily for the benefit of the rich (Shepard 1955). When the gold mania seized the rich Parisians, they often bought shares in joint stock companies, some of which paid passage or extended credit to poor Frenchmen in exchange for a percentage of the gold they might discover in California. At least 83 such companies formed, some dangling the phrase "millions for a sou" before wealth speculators (Nasatir 1964, pp. 20–24). The most ignoble French effort to supply California with miners was initiated by the French government of Louis-Napoleon Bonaparte.[6] Under the direction of Bonaparte, the government organized an immigration company, Société des Lingots d'Or, and a lottery to finance the passage of impoverished Frenchmen to California. In addition to lesser prizes, the government offered as grand prize a golden ingot worth 400,000 francs. This attempt to "rid France of her undesirables" (Nasatir 1964, p. 20) sent almost 4,000 paupers to California, a fact bemoaned by Karl Marx (1969, p. 84) in his well-known phrase that "golden dreams were to supplant the socialist dreams of the Paris proletariat." All of the 83 companies organized in France disbanded upon docking in California. Former members, both rich and poor, went their own way.

The largest number of lower-class sea arrivals came from China. Most of these Chinese argonauts were from peasant backgrounds. They generally financed their passage to California in one of two ways (Williams 1971):

[5] William Perkins (Morgan and Scobie 1964, p. 222), an argonaut whose journal provides much of what is known about Latin Americans in the gold rush, writes as follows: "[As compared to other Latin American countries] the immigration of Chile is much more of a mixed character. Men of all classes have come from there; for Chile is not only a seaboard country, but its people are infinitely more enterprising than any other of the Spanish Republic of South America."

[6] In his analysis of this government's first years, Karl Marx (1969, pp. 84–85) has an excellent description of this effort to send "Parisian vagabonds to California."

Some obtained passage money from their families, lineages, or villages, which would pool their resources in order for one of their members to try their luck in California. However, the majority appear to have used the credit-ticket system: The migrant received his passage on credit, which obliged him to pay off the debt with a portion of the gold he found.

Except for Chinese migrants and poorer Americans who traveled overland, adventurers from lower-class rural origins were the exception. Moreover, the poor who did migrate from Latin America and France were not peasants, but rather urban poor. In Chile, according to Monaghan (1973, p. 5), most of the poor migrants were *rotos*, "landless vagabonds," and not *inquilinos*, "sharecroppers, serfs, peons . . . [who were] too poor and closely attached to the land to leave their ancestral allotments." For France, Marx's analysis (1969, pp. 84, 125) seems to hold true: Poor emigrants were "Parisian vagabonds," who made up the city's vast lumpenproletariat, and not peasants, who lived in "stupefied seclusion." By contrast, Chinese peasants, whose attachment to the land is legendary, left their ancestral plots with relative ease, a characteristic, as it will be shown, that favors adventurism.

If the California sojourn was more likely an adventure for upper- than lower-class individuals, it is even more the case that it was an adventure undertaken by people from only some societies. Here the evidence is more clear.[7] Migration from Chile outnumbered many times the combined total from all of the rest of South and Central America. China alone supplied over 95% of all migrants from the Far East and the Pacific. About 90% of European migrants came from countries in western Europe (Great Britain, France, and Germany), 4% from southern Europe (Portugal, Spain, and Italy), 1% from eastern Europe and the Baltic region, and the remaining 5% from northern and central Europe (Scandinavia, Switzerland, Belgium, and Holland).

The majority of all migrants to California came, of course, from the United States (somewhere around 65% of the total), but even from the United States there are significant sectional differences.[8] In 1850, 46% of

[7] DeBow (1854, p. 118) and Wright (1940, 1941) give the 1850 census figures upon which this evidence is based. Other records concerning the migration of specific groups are probably more accurate. Based on Chinese associational records, Speer (1856) estimates the number of Chinese in California in 1855 to be 48,000. Based on consul registers and passenger lists, Nasatir (1964, p. 26) estimates the French in California in 1851 at 20,000. Based on passport applications, Giacobbi (1974, p. 22) estimates the Chileans in California in 1850 to be between 5,000 and 8,000.

[8] Based on the 1850 census figures (DeBow 1854, pp. 116–17; Wright 1940, 1941), the total migration from the Northeast (Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont) was 28,893; from the border states (Arkansas, Kentucky, Missouri, Tennessee, Texas,

the U.S. argonauts came from the Northeast (particularly New York, Pennsylvania, and Massachusetts), 28% from the border states (particularly Kentucky, Missouri, and Tennessee), 18% from the Midwest (particularly from Ohio), and only 8% from the South. Based on the 1850 census totals, about 0.4% of the total population of the border states had joined the gold rush, 0.31% of that of the Northeast, 0.24% of that of the Midwest, and 0.11% of that of the South.

The vast majority of the argonauts were young rather than old, had middle- to upper- rather than lower-class origins, and (except for American and Chinese argonauts) came from urban rather than rural settings. Most came from the United States, western Europe, Chile, and China. One other characteristic stands out. Those who came left families, friends, and jobs in order to seek their fortune in the gold fields.[9] With the exception of the French and Chilean poor, it would seem difficult to describe these migrants as coming from socially marginal groups. Instead they aspired to or occupied relatively high-status (often professional) positions and were thus, by all indications, well integrated into their respective societies.

This portrait of the argonauts matches the typical description of particular adventurers presented by historians. According to Petersen (1970, p. 62), the early migrants from 19th-century Sweden were "men with a good cultural and social background, mostly young and of a romantic disposition. Since the risks in emigration were great and difficult to calculate, those who left tended to be adventurers or intellectuals motivated by their ideals. . . ." The Spanish adventurers who conquered the Americas, the *conquistadores*, were predominantly young, male, and drawn from the lower aristocracy and the gentry class. "*Hidalgos* in particular were well represented in the conquista—men such as Cortes himself, who came from noble but impoverished families, and were prepared to try their luck in an unknown world" (Elliott 1966, p. 62). Likewise, the first British colonizers in the West Indies and America (e.g., Jamestown) were adventurers—"penniless younger sons of gentility desirous of amassing means sufficient to become landed proprietors in the homeland" (Ragatz 1928, p. 3). The information on the California gold rush, while revealing considerable variation in the social origins of adventurers and with the important exception of American and Chinese argonauts, is consistent with this characterization.

Virginia), 17,738; from the Midwest (Illinois, Indiana, Iowa, Michigan, Ohio, Wisconsin), 11,172; and from the South (Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina), 4,808.

[9] Comments to this effect are frequent, especially in journals of American argonauts. Typical in this regard is a journal entry made in 1849 by Enos Christman (1930, p. 11) on his first day of travel to California. "My feelings and emotions on leaving my friends and my native land on such an expedition, I cannot describe. I have left all that is near and dear and turned my face towards a strange land, expecting to be absent two or three years, hoping in that time to realize a fortune; and then return and be greeted by kind friends."

PUSH AND PULL FACTORS IN THE GOLD RUSH

Was the lure of gold sufficient to cause such a widespread migration? If it were only the promise of easy riches that prompted individuals to join the gold rush, then why did the majority come from only a few widely dispersed societies?

Given the social and geographical origins of the argonauts, one might account for the geographical dispersion by showing the influence of accidental circumstances upon individual decisions to migrate. For instance, in each of the principal contributing societies, circumstantial events that coincided with the discovery of gold might have precipitated the migration to California. In western Europe, massive social movements, economic depressions, and revolutions occurring around 1848 contributed to a feeling of social unrest and insecurity. In China, the Taiping Rebellion and widespread famine paralyzed much of central and south China. Because Valparaiso was the principal port of call for most ships going around the Horn, Chile had a fortuitous location on the world trading routes. And not long before 1848 Chile had had its own political unrest, culminating in its political independence from Spanish rule. In America, the end of the Mexican-American war in 1848 brought the release of a large number of soldiers, some of whom supposedly sought further excitement in California (Caughey 1975, p. 44).

Most historians of the gold rush (as well as sociologists discussing migration more generally) explain the migration in terms of these calamitous or fortuitous events. Closer examination shows, however, that the distribution of these events is far more widespread than the actual geographical origins of the migrants. Revolutions in 1848–49 occurred throughout Europe, but the vast majority of the European migrants came only from western Europe. The rebellion and famine in China were concentrated in a broad area of central China, but Chinese argonauts migrated only from south China. Other Latin American countries besides Chile were well situated on the world trading routes and had had their own revolutions, most more disruptive than the one in Chile. Thus all these "push factors" had only an indirect bearing on the causes for this instance of adventure migration.

For adventure migration, push factors are more complex than simply individual misfortune, and they should be seen in a very different light from push factors motivating people to migrate permanently.[10] When people decide to make a permanent move, they presumably evaluate their native society as less desirable in important respects than their potential destination. By contrast, when adventurers migrate, they do so only to return to their native society at a later date, hopefully with the wherewithal to

[10] The research on "temporary migration," which in theory would include adventure migration, is not large. For a survey of this research see Bovenkerk (1974).

partake more fully in those things they most desire in their homelands. To adventurers the relative desirability of their native society far outweighs that of their destination, or at least initially. Thus the calamitous aspects of revolution in Europe or rebellion in China do not in themselves seem to be sufficient causes to explain gold seeking in California. Something more is at work here, and that something is not misfortune, but dreams of great fortune, regardless of the misery required to obtain it. Thus, whereas most push factors are usually seen as negative, push factors for adventure migration are viewed as positive,[11] a point I will describe in detail below.

Besides disruptive push factors, historians and sociologists often view migration in terms of pull factors. In this sense, one of the most important precipitating causes for the gold rush was something that might be called gold fever—a collective hysteria caused by the discovery of gold (Bieber 1948; Caughey 1975, pp. 38–55). In each of the societies that contributed in a major way to the argonauts, documentary evidence suggests that the gold rush had an important collective component, so much so that the gold rush can be seen as a competitive race to the gold fields. Rumor piled upon rumor, supplemented by eyewitness accounts appearing in newspapers, produced the promise of great wealth for the first ones to arrive at the gold fields. The urge to be first in the race to California created a gold mania.[12]

In the Northeast as well as in areas west of the Appalachian Mountains, the California gold mania was intense. "People of all classes succumbed to the fever" (Bieber 1948, pp. 20–22). People became "California mad." Declared the *Boston Courier*, "adventurers were 'starting off for California by the dozen, the score, and the hundred, hardly allowing themselves time to pull on their boots, and put bread and cheese in their pockets.' " Another newspaper asserted that "the coming of the Messiah, or the dawn of the Millennium would not have excited anything like the interest" caused by the discovery of gold. By 1850, about a year after the discovery of gold became widely known in the eastern half of the United States, almost 70,000 Americans had already arrived in California.

Similar gold manias occurred in all the countries producing large numbers of argonauts. Proclaimed one Liverpool observer (Wright 1941, p. 66) in January 1849, "The gold excitement here and in London exceeds anything

[11] Neil Smelser (1962, p. 171) also makes this point in a slightly different context when he defines "crazes as mobilization for action based on a positive wish-fulfillment belief."

[12] Gold mania can be profitably categorized, according to the typology developed by Smelser (1962), as a craze. It should, however, be noted that, as it is analyzed here, adventurism is a form of action undertaken by individuals who may or may not be responding to a collective movement. Historically, most examples of adventurism have not been associated with collective movements that could be confidently labeled as crazes. Thus crazes do not cause adventurism.

ever before known or heard of. . . . Nothing is heard or talked about but the new El Dorado." According to A. P. Nasatir (1964), the best historian on the French in California, gold mania seized Parisians. French newspapers reported every detail that could be found about California, often erroneously; hysteria sparked a movement of about 20,000 Frenchmen to California in the first two years of the gold rush. In China, where most argonauts were from rural areas, the news of the discovery traveled more slowly. First posted on placards in the main cities, the news spread to outlying marketing towns and from there to villages. Though not as rapid or widespread as in other locations, the response still constituted a collective movement; between 1850 and 1855, as many as 50,000 Chinese sought wealth in California. Another 40,000 set out for Australia, where gold had been discovered in 1851 (Choi 1975). Over 90% of these gold seekers came from Kwangtung Province in south China (Choi 1975; Speer 1856). And in Chile, as one skeptic observed in 1848 (Monaghan 1973, pp. 53–55), "The reports of riches in California which have caused such a stir in Valparaiso have also created much agitation in the habitually sedate Santiago. The spirit of adventure, like the tides on the beaches of Valparaiso, draws [Chileans] away to California. All wish only to be the first in the land of a portentous future. They go arming themselves with only a pick, or shovel, or even a knife to dig gold by handfuls."

Despite widespread excitement, gold fever did not stir all parts of the world equally. Outside of western Europe, Chile, China, and the United States, there were few symptoms. This differential reaction to the news of the discovery is illustrated by the contrast between southern and northeastern United States. According to Bieber (1948, p. 24), "Along the South Atlantic coast the excitement was less intense, though a few ships departed for California from Norfolk, Wilmington, and Charleston. However, no vessel cleared from Savannah for California in 1849. North Carolina was probably the least affected of any of the South Atlantic states; the gold fever, according to the Raleigh *Times*, 'has hardly disturbed the snooze of our quiet old State.' " Although the southern newspapers printed the same stories about the discovery of gold, there was no mania.[13]

The same outcome occurred in Peru, where the absence of a gold mania forced would-be promoters to fill ships bound for California with freight instead of passengers (Monaghan 1973, pp. 94–110). As in the South,

[13] A striking illustration of the difference between the Northeast and the South in response to the discovery of gold is found in the autobiographical account of Howard Gardiner (Morgan 1970, pp. 7–8). Gardiner, a northeastern merchant traveling in North Carolina a short time after the discovery became known, reported that the news of the discovery provided "an interesting topic of conversation" for the local inhabitants. At about the same time he received a letter from his home on Long Island informing him "that the exodus from that vicinity seemed likely to depopulate the village, as almost all the able-bodied men had either gone or were preparing to go" to California.

Peruvian newspapers reported the discovery of gold in full, the first announcement coming one day before it appeared in Chilean newspapers. But no gold fever developed, either among the rich or among the poor. Monaghan (1973, pp. 103–4) puts it mildly: The "gold-rush excitement in Peru . . . lacked the keen aggressiveness displayed in Valparaiso."

Why should people in some societies or a part of a society be so little inspired to join the gold rush? Or, conversely, why should so many people in other areas be so greatly influenced by the discovery? Even though mass movements swept into the rush to California many who would not ordinarily have been influenced by a vision of sudden wealth, these differential patterns of behavior seem symptomatic of the presence of some more fundamental factors influencing individual actions.

STRUCTURAL SOURCES OF ADVENTURISM AMONG THE ARGONAUTS

Although a great many people undoubtedly found the idea of California's wealth attractive, only very few acted on the basis of this idea. To be sure, calamitous events and gold manias were precipitating causes for the gold rush; but to understand this instance of adventurism—the motivation behind it as well as the act—is to uncover those secondary causes, as de Tocqueville (1971, p. 78) once called them, that prepare the ground in advance and shape the course of events without actually causing their occurrence. In other words, motivational factors are not so much ideas in the minds of individuals as structural arrangements in society that give salience to ideas and instill in them a force beyond the content of the ideas themselves. In this sense, ideas are socially grounded in the circumstances of people's lives; individuals act on the basis of the world they see before them; they espouse and manipulate those ideas that make sense to them—the same ideas that, at times, consume them. The secondary causes of the California gold rush have little to do with calamities or gold fever and have much to do with individual interpretations of how one could profit from the wealth that California's gold fields promised. At the risk of considerable oversimplification, the secondary causes of adventurism are to be found in a society's social structure.[14]

The following comparative analysis does little more than outline a hypothesis about what types of structures seem to be conducive to adventurism. Moreover, such comments as I offer result primarily from an ex post facto examination of adventurism in the gold rush and may not apply to adventurism in general. My reason for presenting such a provisional approach, however, is grounded in my belief that risk taking is an important

[14] I should note here that this attempt to specify the stratificational conditions which favor adventurism differs from Smelser's (1962, pp. 175–80) more general attempt to define the structural conduciveness for crazes.

form of behavior in modern as well as historical times. Thus the tentativeness of this effort is to be balanced by the need to open discussion on this neglected feature of social life.

On the surface, the social structures of western Europe, Chile, China, and the United States would seem to be very different from one another, as in fact they are. But they shared in 1848 at least one characteristic: a "particularistic-achievement" orientation, to borrow a concept from Talcott Parsons's discussion of the principal types of stratification.

As Parsons (1951, pp. 195–98) defines it, a particularistic-achievement pattern is a particular type of social structure in which individuals orient their lives toward the attainment of social objects within "the relational system." Universalism is weak; individuals' particularistic bonds are strong. An achievement orientation within this system takes the form of individuals striving for "a *proper* pattern of adaptation . . . which can be maintained only by continuous effort and if not maintained must be re-achieved." Although couched in very general terms, the thrust of Parsons's description depicts the individual actor as having both particularistic affiliations and the obligation to achieve his status within these affiliations.

The concept is suggestive of the conditions that confront would-be adventurers. Consider, for instance, a common sort of adventurer, the impoverished noble. A person may be born noble and feel entitled to practice an aristocratic style of life, but that in itself is insufficient to be of the nobility. To occupy that position is to have enough wealth to live as a noble lives. In this situation the goal of one's orientation is quite specific—to achieve the wealth necessary to practice a style of life to which one already feels entitled. The means to achieve this goal (the mobility strategies) are diverse; any number of different courses of action enable the individual to reach the same goal. The crucial factor is the acquisition of wealth and not the means by which wealth is obtained, though presumably some means would be more honorable than others.

To an individual in this sort of situation, adventurism is a low-risk strategy; the possible gains of an adventure far outweigh the possible losses. One does not lose one's ascribed status (e.g., a noble birth) by engaging in adventurism. One may, however, gain the material goods to play a particular role more fully. A strategy of adventurism holds special gains to such individuals because one need have no commitment to the means by which wealth is obtained. The adventure itself can be undertaken for one's own personal and perhaps secret purposes rather than for society's benefit; and it holds the promise of a rapid, if not an easy, road to achieve one's particularistic goals.

Two components of this structural orientation seem most important. First, one's ambitions are directed toward attaining particularistic goals, which are typically defined in relation to established groups such as the

lineage, community, ethnic group, or more generally status group. Membership in these sodalities can be easily claimed, in the case of the community, or is ascribed, in the case of the lineage. Thus membership itself is not problematic. What is problematic is the individual's standing and esteem within the group to which he claims membership. Intragroup status rankings are ambiguous, informal, but important. Second, one's status within the group is achieved, in part, through the accumulation of objects highly valued by the group. Combining these two components, one can characterize this structural framework as one which favors individuated action taken to satisfy particularistic goals.

One can suggest that this type of orientation is found in many different social settings. In fact, it probably exists to an extent in all societies. The important point and the appropriate focus of analysis is, however, to show that some social settings favor it, while others do not. This hypothesis can be illustrated by comparing societies which did and did not contribute argonauts to the gold rush.

The contrast between Chilean and Peruvian society is a good place to begin. The former supplied California with many adventurers, the latter very few. Two features of Chilean society seem particularly important in explaining this difference: (1) the lack of a system of patronage linking the members of lower and middle classes to the upper class, and (2) the cohesiveness of an upper class into which upward mobility could be achieved by people who were not indigenes, who had wealth, and who could emulate upper-class standards.

In Chile, the absence of an interclass patronage system occurred, in part, because there was no need to bind peasants to the soil. Chile's vast arable terrain, large mestizo population, and concentrated landownership provided a surplus of laborers. Although service tenancy was widespread, landlord/tenant relations lacked the coercive features of debt peonage and the moral features of the *compadrazgo* (patron-client) system (Bauer 1975, pp. 48–52). The decisive characteristic here is that the stability of the landowning elite was largely independent of its ties to the peasantry. In contrast, Peru's mountainous terrain, desert coastal regions, and large Indian population made it difficult for large landowners to maintain a labor force that was not a dependent one. Debt peonage and the *compadrazgo* system, both in widespread use, formed the basis for interclass ties that helped to ensure a stable labor force and to fix a person in a network of interclass relations. Because these ties cut across class lines, they also provided a source of patronage and conflict within the upper class, members of which varied in the amount of resources they could mobilize (Orlove 1977; van den Berghe and Primov 1977).

In Chile, the lack of interclass patronage helped produce a cohesive yet relatively open upper class. In fact, Chile's revolution against Spanish rule

reinforced its cohesiveness as well as its openness. According to Arnold Bauer (1975, p. 18), in an excellent study of Chilean society, "The cohesive and class-conscious creole elite moved smoothly, compared with other former Spanish colonies, to control the machinery of republican government." Although relatively small and primarily composed of titled families, the upper class was not closed. There "was a steady and welcome intrusion of new faces. . . . Money and distinguished military service permitted entry into the best circles" (Bauer 1975, p. 36). And for those who could qualify—people with a European heritage—admittance to the upper class was a goal to be desired. Upper-class status allowed individuals access to secure position in the government as well as prestige and a cultivated style of life. In discussing the most frequently used strategies to enter the upper class, Bauer (1975, p. 40) says that "mining offered the fastest road to wealth and social prominence," a conclusion also reached by Chile's newspapers in 1848 (Monaghan 1973, p. 45). In contrast, Peru's revolution against Spain accentuated cleavages within the upper class (Dobyns and Doughty 1976; Monaghan 1973, pp. 79–93). The elite itself became divided into political factions; an individual's place in the upper class was in large part determined by his ties of patronage to other and more powerful political figures. Access to money-making opportunities, such as the very lucrative guano trade, likewise depended on patronage. Thus one's social mobility in the Peruvian context was a matter requiring constant negotiation, a lapse in which might cause a loss of power, access, and prestige.

Thus, in comparison, Chile's social structure favored adventurism more than did Peru's. In Chile, acceptance in the upper class was based largely on emulation and consumption, both of which required wealth. Hence adventurism was a viable strategy of upward mobility for those who had the pedigree for membership in the upper class but lacked the wealth. In Peru, however, upwardly mobile individuals had to protect their vested interests against factional disputes and had to oversee and, to a certain extent, fulfill their *compadrazgo* obligations, lest they lose their dependent laborers. All these things required constant and personal attention, thus precluding the use of adventurism as a mobility strategy.

Very different conditions facilitated adventurism in western Europe. The revolutions in western Europe, beginning with the French Revolution and continuing through the revolutions of 1848, overturned the old order. Except for Britain, the hereditary aristocracy lost its secure hold over upper-class positions and had to vie with the wealthy from all backgrounds for high social prestige. As de Tocqueville (1969, p. 628) makes clear, however, aristocratic social conventions—the customs, the manners, the accoutrements—did not die out, but rather became the standards for all groups to emulate. Accordingly, observes de Tocqueville, a consequence of revolution, long after the revolution itself is over, is an impression that

"nothing seems impossible" for those having money. "The passions roused by revolution by no means vanished at its close. . . . Longings on a vast scale remain, though the means to satisfy them become daily less. The taste for huge fortunes persists, though such fortunes in fact become rare, and on all sides there are those who eat out their hearts in secret, consumed by inordinate and frustrated ambition." Marx (1969), who on this point agrees with de Tocqueville, chided the adventurism of Parisians in 1848, condemned their lack of commitment, and ridiculed their selection, as president and later emperor, of Louis Bonaparte, "an adventurer blown in from abroad."

In 1848–49, adventurism in western Europe was a strategy of the moment; the gains seemed great, the losses few, because for a time previously established routes to success had vanished and new, more certain routes had not yet become institutionalized. Little by little, predicted de Tocqueville (1969, p. 628), "Longings once more become proportionate to the available means. Wants, ideas, and feelings again learn their limits."

By contrast, the revolutions of 1848–49 in eastern and southern Europe did not cause the social transformations that they did in western Europe. Though shaken, the Austro-Hungarian Empire held firm against the revolutionary tides of 1848–49. Newly emancipated serfs did not immediately migrate, nor did the aristocracy lose its hold over them. In Italy, the revolutions degenerated into wars between competing states; factionalism within the elite was omnipresent. And by mid-1849, throughout the area conservatism and repression gained the upper hand. One observer (Palmer 1961, p. 485) said of the Austrian-Hungaro Empire that the society became "a standing army of soldiers, a sitting army of officials, a kneeling army of priests, and a creeping army of informers." As a consequence, the few adventurers from these areas who did take part in the gold rush tended to be expatriates—the radicals and the exiled aristocrats, such as those from Poland—whose adventurism had become a way of life rather than a means to an end.

In both western Europe and Chile, adventurism was a strategy used as a means to gain entrance into the upper class. These upper classes were national ones. Paris, London, Santiago—all were the central cities in a national culture and the only locations in which upper-class recognition could be gained. The trappings of the upper classes—the proper accent, manners, and material goods—were established at the center. And it was in these cities that gold fever fired the imagination of those whose ambitions were directed toward this particularistic goal. In these locations, virtually by definition, adventurers were not provincials.

In China and the United States, it was otherwise. Both societies lacked a unified upper class and had cohesive, yet highly competitive local communities. Here adventurism stemmed from local concerns, aimed at less

grandiose ends, involved greater numbers, but was no less consuming than adventurism elsewhere. Accordingly, adventurism in these two societies provides a source of variation important in understanding the phenomenon.

Among 19th-century Asian societies, China had no parallel in terms of the openness of its class structure. At the time of the gold rush, Japan's feudalistic social structure circumscribed individual aspirations, despite the influence of widespread commercialization (Rozman 1974). Moreover, as Chie Nakane (1970) makes clear, even in modern Japanese society mobility strategies continue to be plotted through carefully made and constantly maintained ties with one's superior. Thailand's patrimonial state hierarchically arranged and linked everyone, from the peasantry to the nobility, in an extensive system of clientage (Rabibhadana 1969). A similar system existed in parts of Indonesia, a modern-day version of which is reported by Geertz (1963). And in India, castes (*jati*) constrained the visions of their members, resulting, paradoxically, in attention being paid to both the minutiae of daily life and otherworldliness (Schluchter 1976). Of course, social mobility was possible in all these places, but adventurism was hardly a viable strategy in any of them, except China.

Adventurism in China stemmed from a convergence of factors: the centrality of the patrilineal, patrilocal kinship structure; the absence of interclass clientage systems; a bureaucracy recruited through achievement criteria; and a class structure labeled by the eminent authority on social mobility in late Imperial China, Ho Ping-ti (1964), as not only open but "fluid."[15] An understanding of the patrilineage is most important.[16] No group in Chinese society gave the individual a more long-lasting or stable identity than the lineage. Lineages in south China sometimes numbered several thousand living members; they claimed extensive territory and controlled local affairs; their members often lived together in exclusive single-lineage villages. Unlike the clan or super lineage, which had mythical ancestors, the lineage had actual and acknowledged founders who were objects of veneration. Members had their names and accomplishments recorded in the lineage genealogy and plots reserved for them in the lineage cemetery.

Lineages, however, were not peaceful or cooperative communities. Lineage membership was ascribed; but lineage rank, power, and prestige were gained through competition and conspicuous consumption. One of the most important factors in this status competition was the possession of wealth. Through wealth one could become a landowning member of the

[15] This following discussion on China is drawn from a variety of sources, particularly Freedman (1958, 1966), Ahern (1976), Skinner (1976), Chen (1940), and Hamilton (1977). It should also be noted that Chinese society was the prototype for Parsons's discussion of particularistic-achievement orientation.

[16] For a general theory of patriliny that is applicable to Chinese society, see Paige (1974).

local gentry, purchase a literary degree, and obtain the fineries that were so important in revealing to others that a high station had been reached. And finally, wealth allowed its possessor the possibility of becoming a lineage founder—and an object of veneration in his own right—through the establishment of an independent-lineage segment with its own corporate property holdings, ancestral shrine, and burial plot (Freedman 1958, 1966, 1970).

Obtaining wealth in most rural areas was highly problematic. Because of the scarce resources, overpopulation, and intense competition, the ambitious left their native place in search of riches. By the time of the gold rush, temporary migration from rural to urban areas had long been an established strategy to obtain the material means to become influential in local affairs (Skinner 1976). In one of the better analyses of a south China village, Daniel Kulp (1925, p. 48) notes that the "fundamental motivation of emigration" is security—the desire for wealth which would enable the migrant to return to his native village and assume a secure position. Although the Chinese peasant was bound by no tie of patronage to a landowner (tenancy was contractual), he was tied to his immediate family and more generally to his lineage. Though this tie may have been socially and emotionally binding, it was not geographically binding. If anything, it forced him into adventurism (Kulp 1925, p. 49). "Sometimes the young farmer, himself unaffected by the stories [of wealth] becomes a victim of filial piety. The aged parents, lacking the comforts they see others enjoy, urge or sometimes compel him to seek their support where it is more sure. The ideals of forty centuries are the very core of his being; he cannot refuse. He leaves to take up the struggle; and the parents watch for the captain of the ferry to bring them news and money from their son." As Kulp (1925, pp. 48–49) observes, however, the Chinese peasant had relatively little to lose and much to gain by his sojourning. He could expect assistance from other Chinese wherever he traveled; and if he failed, he would return to his native village no worse off than when he left. But if he succeeded, he would "return the object of envy and emulation, and in every respect [would] enjoy superiority over the 'ricepot-keeping-turtles' who lacked the courage to break away." So great was the desire for local success and so strong the structural sources of adventurism that the Chinese peasant is the prototype of the sojourner (Siu 1952). No society has supplied the world with more adventurers.[17]

As in China, adventurism in the United States seems to be a response to

[17] The scale of temporary migration from China exceeds that of any other society. Based on the figures given by Ta Chen (1923), between 1850 and 1925 an estimated 13,500,000 Chinese had migrated to Southeast Asia from two provinces in south China. One of the best descriptions of temporary migration from 19th-century China is found in China Imperial Maritime Customs (1870).

one's involvement in local society. The foremost observer of early 19th-century America, Alexis de Tocqueville (1969, pp. 62–87, 520–38), found the most distinctive feature of American society to be the township. The community encompassed the individual, limited his vision of the world, and instilled in him a restless ambition to possess those things that others around him had. The ambiguousness of rankings in democratic society and the intensity with which Americans invested their ambition and future in local communities created, thought de Tocqueville, a peculiarly American mentality: constant status competition and the use of material goods to signify success. Weber (1968, p. 933) reached the same conclusion about status stratification in the United States, as have most sociologists who study social mobility in the context of local communities. A portion of de Tocqueville's (1969, pp. 536, 548) masterful analysis follows: "It is odd to watch with what feverish ardor the Americans pursue prosperity and how they are ever tormented by the shadowy suspicion that they may not have chosen the shortest route to get it. . . . When everyone is constantly striving to change his position . . . men think in terms of sudden and easy fortunes, of great possessions easily won and lost, and chance in every shape and form. Amid all these perpetual fluctuations of fate the present looms large and hides the future, so that men do not want to think beyond tomorrow."

The discovery of gold in California must have seemed to a great many the shortest route. The letters and journals of American argonauts, in fact, repeat in specific terms what de Tocqueville said in general ones. As Samuel McNeil of Lancaster, Ohio, wrote (Morgan and Scobie 1964, pp. 2–3), "Being a shoemaker, and ambitious to rise somewhat over the bench, it is no wonder that the discovery of gold in California excited my fancy and hopes." To Enos Christman (1930, pp. 225–26), a printer's apprentice from West Chester, Pennsylvania, the promise of gold meant money to marry his fiancée: "At home there is little chance for a mechanic without capital to rise very fast." Both McNeil and Christman joined companies organized within their local communities, as did thousands of other Americans who journeyed to California. From Massachusetts alone, at least 124 companies set out for California in 1849, each company representing a specific locality (Howe 1923, pp. 171–72, 187–213). Thus, in contrast to western European and Chilean argonauts, American migrants were provincials and organized as such.[18]

Rural areas in the South, unlike those in other parts of the United States, lacked autonomous townships and status ambiguity (Cash 1941, p. 35). The plantation rather than the community as the center of social organiza-

[18] Chinese argonauts also organized according to their native locality. See Speer (1856) and Williams (1971) for their discussions on this point.

tion encompassed even those who did not live on them, the poor whites. According to W. J. Cash (1941, pp. 36–39), the plantation system tended to make the division between the successful and unsuccessful rigid. The former thought of society as "a division . . . into Big Men and Little Men, with reference to property, power, and the claim to gentility." The latter were "those whose vague ambition, though it might surge up in dreams now and then, was too weak ever to rise to a consistent lust for plantations and slaves, or anything else requiring an extended exercise of will—those who, sensing their own inadequacy, expected and were content with little." Adventurism in the South was constrained by the framework of its society. Though many adventurers came out of the South, its social structure did not favor adventurism on so widespread a scale as that in the rest of the country.

In summary, an analysis of the gold rush suggests that adventurism is an outcome of a particularistic achievement orientation with a structural component: identity-bestowing collectivities in which one's relative standing must be achieved. In the societies examined above, these collectivities include cohesive upper-class status groups, communities, and kinship organizations. Theoretically, other types of collectivities (e.g., ethnic groups) should provide similar bases for adventurism. It should be noted, however, that the nature of the collectivity, its size and location, is a determining factor in the extensiveness and probably the outcome of adventurism.

By contrast, in those societies examined that did not exhibit adventurism, an individual's social mobility was constrained, channeled, or intertwined with that of others in such a fashion as to exclude alternative courses of action. In each, adventurism would appear to have been an unlikely form of action for any but the most socially marginal individual to take. In these settings, one's personal aspirations as well as less subjective factors limited one's choices and increased the likelihood of one's conformity to a normative order. To violate these norms of conduct in this context, as adventuring would do, would have led to dishonor, failure, or disenfranchisement.

Adventurism is thus most likely in those societies composed of individuals who are ascribed or who claim an identity without having the means to fulfill the particularistic demands of that identity. Adventurism can be seen as a response to particularistic demands and as an attempt to achieve particularistic goals. Traditionalistic loyalties—such as those to a family, an ethnic group, or a community—are the types of particularistic affiliations that compel individuals to seek adventure while at the same time making their adventure temporary and their commitment personal. It is the brevity of their intended sojourn and the intensity of their status involvement that paradoxically lead adventurers to maximize their endeavors in the short run without giving full regard to the social consequences of their actions.

CONCLUSION

The above analysis demonstrates that social structure contributes differentially to the amount and to the kind of risk individuals are willing to take. In other words, some structural conditions nurture attitudes favorable to risk taking, while others favor "playing it safe."

Now, in conclusion, I want to expand my original definition of adventurism by addressing the contrast Weber (1958) makes between types of risk taking: Long-term calculated risks, such as those methodically and routinely taken in the course of modern business, qualitatively differ, says Weber (1958, p. 20), from the "irrational" short-term recklessness associated with adventurism. Although Weber's discussion of adventurism is very brief, he (1958, p. 58) draws a sharp distinction between adventurers and bourgeois capitalists. "The inner attitude of the adventurer, which laughs at all ethical limitations, has been universal. . . . This attitude was one of the strongest inner obstacles which the adaptation of men to the conditions of an ordered bourgeois-capitalistic economy has encountered everywhere." Thus Weber seemingly implies that the motivational posture of the adventurer undermines rational calculation, that the adventurer does not make a capitalist.

This investigation alters Weber's findings. The analysis shows that adventurism, as a form of risk taking, may well be a relatively calculated, even a logical way to achieve individual goals. The important factors determining the use of this strategy are the actors' perceptions of their situation and their recognition of available alternatives to achieve their situationally specific ambitions. Acting on conditions of uncertainty, as does the adventurer, is a reasonable alternative for people who find other options blocked or distasteful or fall short of the gains needed. Although it may be a reasonable response in specific social contexts, adventurism nonetheless differs from capitalist risk taking in terms of the two definitional components given at the beginning of this essay—namely, (1) in the nature of the risks being taken (the act) and (2) in the nature of the goals being sought (the motivation).

With adventurism, the odds for success cannot be calculated as they can in capitalist enterprises. In fact, the thrust of capitalist risk taking is cautiously balancing the minimization of risk with the maximization of profit. Beyond caring for superficial details, adventurers, however, are unable to minimize systematically the uncertainty they will be facing or to maximize their potential gains, simply because both are unknown or uncalculable prior to the venture itself. For this reason, associations of adventurers emphasize not the minimization but rather the equalization of risks; the uncertainties of the venture will be shared more or less equally. This emphasis limits the use of hierarchical forms of group organization, particularly

bureaucratic forms, which Weber saw as an important feature of capitalist enterprise. The kinds of organizations spawned by adventurism do not lend themselves to capitalist adaptation, or for that matter to any complexly organized endeavors.

The actions of the argonauts clearly illustrate this inability to form complex organizations. Of the companies organized to mine gold together, few survived the journey to California; once in California almost none survived the frantic search for gold. Only the Chinese were able to organize their numbers to mine gold cooperatively, and even they did so with difficulty. But once gold deposits that allowed for the systematic balancing of risks and profits were discovered, capitalist enterprises (e.g., hard-rock and hydraulic mining companies) began to form and then to predominate in the gold fields. Thus, it can be suggested that adventurers do act logically, according to the type of risks they are facing, but that the nature of their risks qualitatively differs from that of the risks encountered in routine business.[19]

Second, adventurism differs from capitalism in terms of the "inner attitudes" of the participants. Capitalist ethos, as Weber defines it, subordinates personal desires and happiness to the demands of routine, continuous work, which from the individual's point of view, notes Weber (1958, p. 70), is "so irrational." Adventurism, on the other hand, is a form of action that gives full rein to one's personal ambitions, often at the expense of one's obligation to submit to routine expectations of everyday life. From an individual perspective, adventurism can be seen as a calculated, if not a calculable, strategy to achieve personal goals rapidly by circumventing established modes of upward mobility. From a societal perspective, which Weber takes in this instance, adventurism can be seen as irrational, as a hedonistic response that, if universally practiced, would threaten, if not destroy, the stability of routine economic activity.

But these two ideal-typical forms of action—subordination versus attainment of personal goals—are not neatly separated into discrete categories of behavior. Rather, as Weber clearly recognizes, they are part of an often ambiguous internal tension, an inner struggle between meeting social obligations and attaining personal happiness that is felt by all individuals and that resolves itself differently in different situations. This study indicates that those deciding to pursue personal goals by breaking with established routines are those having the least to lose socially; adventurers gamble their persons—their health, even their lives—more than their social memberships. It is thus not surprising that some social contexts favor adventurism

[19] For an excellent general discussion of different strategies of action in conditions of varying amounts of risk, see Heath (1976). His discussion of the "theory of choice under uncertainty" has some bearing on the choices made by adventurers.

more than others. Nor is it surprising that the entrepreneurial breakthrough that laid the technological foundation for routine bourgeois capitalism should have occurred in many of the same locations from which the argonauts came; for, as Schumpeter notes (1962, p. 132), the entrepreneur, like the adventurer, travels "beyond the range of familiar beacons." Thus, the inner attitudes of the adventurer perhaps do not differ from those of the capitalist as much as Weber would suggest. Nonetheless, I believe the main point of Weber's argument is correct. The individuation inherent in adventurism and the intensely personal goals sought from adventures threaten the orderliness of routine social and economic activity. It is this inner conflict between meeting "the demands of the day" and finding personal satisfaction that "the spirit of capitalism" tries always to subdue by making the two seem to coincide.

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Untangling Structural and Normative Aspects of the Minority Status–Fertility Hypothesis[1]

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Since the residual differential fertility of racial and ethnic minorities can be explained in theoretically opposed ways, effects due to subcultural norms can be established only by direct measurement. Data from a recent survey of Los Angeles Chicano couples indicate that ethnic integration is actually associated with reduced fertility, suggesting that the structural negative minority-status effect on fertility hypothesized by Goldscheider and Uhlenberg operates even within a high fertility minority and contradicting their suggestion that high minority fertility is due to subcultural norms.

A persistent problem in the analysis of fertility variation in the United States has been the nature and extent of ethnic and minority fertility differentials and the degree to which they can be accounted for by socioeconomic factors. The implications of this problem extend beyond the study of differential fertility, since the relative causal importance of structural and cultural factors is relevant to a wide variety of sociological specialities and indeed is one of the enduring fundamental issues in sociology. Residual minority fertility left after the usual socioeconomic correlates of fertility are controlled is usually attributed to "cultural" differences (e.g., Roberts and Lee 1974). But such residuals can be attributed just as well to structural factors. In the analogous status-attainment literature, many sociologists assume (with perhaps no better justification) that residual ethnic differences indicate discrimination, not subcultural variation in values (e.g., Duncan and Duncan 1968). In this paper we employ direct measures of ethnic integration and the logic of internal analysis to test whether the degree of ethnic integration is correlated with family size in one particular high-fertility minority: Chicanos (Americans of Mexican and Southwest hispanic descent). If the Chicano ethnic subculture produces higher fertility, then among Chicanos ethnic integration and family size should be positively associated even when socioeconomic controls are introduced.

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STRUCTURAL AND NORMATIVE EFFECTS

The fundamental weakness of Goldscheider and Uhlenberg's (1969) seminal paper and of most subsequent minority-fertility literature is that they fail to distinguish between what are really two conflicting theories: one explaining reduced minority fertility as a reaction to external structural pressures and another explaining high minority fertility in terms of subcultural norms and ideals (see Lopez and Sabagh [1976] for a critical review of this literature). With such a two-edged theory it is too easy to account for any minority-fertility variation, whether above average or below, without addressing the theoretical question of why minority status sometimes produces high fertility and other times low. Any explanation of minority-status fertility should be able to account for both with one consistent theory, not a pair based on theoretically opposed bases. As long as demographers continue to rely on sophistic explanation and indirect measurement via residuals, they cannot construct a satisfactory theory of ethnic-fertility differentials. Thus a recent paper (Kennedy 1973) can attribute above-average fertility to conditions similar to those outlined by Goldscheider and Uhlenberg as causes of reduced fertility.

Of the two halves of the Goldscheider and Uhlenberg minority status theory the structural pressure-reduced fertility argument was the better supported theoretically and empirically. They argued that awareness of external discrimination produced smaller family size as an adaptive reaction (low Jewish fertility is their best example). We suggest that such awareness should increase with ethnic integration and this structural negative minority status effect should be discernible even within a high-fertility minority. But greater ethnic integration should also mean greater exposure to autochthonous subcultural norms (as distinct from "norms" that only mediate between external pressure and behavior). If there is such a value norm among Chicanos, their high-fertility background suggests that it should favor large families over small, and there is empirical evidence that Chicano women desire larger families than other women at equivalent educational levels (U.S. Bureau of the Census 1974). Explanation in terms of such subcultural norms would be supported if fertility and ethnic integration are positively associated, net of socioeconomic factors. But since the autochthonous norm should be for larger families, a negative relation between ethnic integration and fertility would support our hypothesis that the structural pressure negative minority-status effect operates among Chicanos. A finding of no net relation would support the assimilationist perspective, at least in the sense that fertility and degree of ethnicity are unrelated. But positive or negative relations would supply direct evidence for, respectively, the normative or structural sides of the Goldscheider and Uhlenberg argu-

ment. Whatever one's biases on the particular and general issues involved, it cannot be denied that such a direct test is both rare and significant.

DATA AND METHODS

Since the Census Bureau and national fertility surveys lack indicators of ethnic integration as well as other factors that must be taken into account in any study of Chicano family size (country of upbringing, e.g.), in 1973 the authors carried out a survey of 1,129 Los Angeles area Chicano couples (see Lopez and Sabagh [1976] for a fuller description of this survey and operationalization of ethnicity and other variables). Any measure of ethnicity as a distinct dimension has to be as uncontaminated as possible by indicators of low status or other general traits associated with having large families. We suggest that having mostly other Chicano friends and using Spanish at home ("social ethnicity") along with watching and listening to Spanish TV and radio ("media ethnicity") are the best indicators of sociocultural ethnic integration for Chicanos. Whatever the content of Chicano-Mexican culture, it will be reflected in the Spanish media. Having Chicano friends will tend to reinforce the message of the media and also to supply interpersonal support for the maintenance of any distinct ethnic subculture. Speaking Spanish rather than English at home facilitates both the sociation and media ethnicity and, if language choice and content are associated, is itself an aspect of ethnic cultural maintenance. We have also included ethnic homogeneity of neighborhood and husbands' fellow workers ("context ethnicity"). Like the other four, these indicators are correlated with lower status among Chicanos but unlike those, living and working in high-density Chicano contexts may have intrinsic components of lower status and resource deprivation not taken into account by ordinary SES statistical controls. Living in the barrio is, after all, equivalent to living in a poor racial-ethnic ghetto, and having mostly Chicano co-workers implies occupational ghettoization, working in a marginal industry and job. To test the effects of these varying sorts of ethnicity, the analysis is carried out in terms of the three indexes of ethnicity (social, media, and context) and a summated index of overall ethnicity. Women are divided into four subgroups according to country of upbringing and age. Fertility is measured by the number of live births. Control variables include wife's schooling, husband's schooling, husband's occupational status (Featherman's revised Duncan status scores), and wife's age at marriage. Socioeconomic origins, rural upbringing, income, and age variation within the 10-year cohorts had no additional impact on the pattern of results and so were excluded from the equation. Principal results are reported in the form of standardized regression coefficients (betas).

RESULTS

In all four subgroups, the sets of ethnicity and socioeconomic measures are generally negatively correlated and positively correlated within sets, though social and media ethnicity are consistently more strongly related to each other than to context ethnicity. Age at marriage has the predictable positive association with social status, but its association with ethnicity is not what stereotypes might predict: social and media ethnicity are associated with later marriage, while only context ethnicity (ghettoization) is associated with early marriage (correlation matrices omitted). Including age at marriage in the analysis has, if anything, the effect of making net relations between overall ethnicity and fertility more positive (working against our hypothesis).

Table 1 presents the zero order (r) and net (beta) relations of the independent variables discussed above with fertility (live births). The r's and betas of age at marriage and status are predictably negative. The relationship between ethnicity and fertility is rather more interesting. In both older cohorts and among the younger women raised in the United States, the correlation between overall ethnicity and fertility is only slightly positive ($r = .11$ among younger United States–raised women is the only significant correlation). The net effect of overall ethnicity on fertility, with marriage age and SES controlled, is mildly negative in two groups and essentially zero in the other two.

The separate aspects of ethnicity provide clearer results: among both older cohorts social and media ethnicity are negatively correlated with the number of live births (r's ranging from $-.10$ to $-.16$), while context ethnicity is strongly positively correlated (.32 and .34). In both younger cohorts correlations of live births with social and media ethnicity are insignificant or slightly negative. For context ethnicity the correlation is again strongly positive among younger United States–raised women, but about zero for those brought up in Mexico. In no cohort is there a positive correlation of social or media ethnicity with fertility or a negative one for context ethnicity. The more important net relations are similar. All the net effects from social and media ethnicity to family size are negative, though not always significant in our small samples. Context ethnicity, in contrast, is strongly positively related to fertility in three of the four cohorts, including both older ones.

DISCUSSION AND CONCLUSIONS

We argued that our social and media indicators best tapped involvement in a distinctive ethnic subculture. The results show that, had we limited our index of overall ethnicity to these indicators, the net relations with family size would all have been negative, though to varying degrees. Ethnic integration for Chicanos is associated with reduced fertility, implying that above-average Chicano fertility cannot be accounted for by subcultural norms and

TABLE 1

CORRELATIONS AND STANDARDIZED REGRESSION COEFFICIENTS OF LIVE BIRTHS ON ETHNICITY, STATUS, AND WIFE'S AGE AT MARRIAGE, BY AGE AND COUNTRY OF UPBRINGING, FOR MARRIED CHICANOS IN LOS ANGELES, 1973

	UNITED STATES RAISED				MEXICO RAISED			
	25–34		35–44		25–34		35–44	
	r	beta	r	beta	r	beta	r	beta
Overall ethnicity	.11*	.02	.05	−.10*	−.04	−.09*	.08	.05
Social ethnicity	−.07	−.11*	−.16**	−.31***	−.04	−.05	−.14*	−.17**
Media ethnicity	−.02	−.09*	−.15**	−.07	−.07	−.06	−.10	−.06
Context ethnicity	.26***	.20***	.32***	.20***	.03	−.04	.34***	.30***
Wife's education	−.54***	−.47***	−.23***	−.19***	−.27***	−.13**	−.01	.00
Husband's education	−.18***	.00	−.27***	−.13**	−.18***	−.07	−.10	−.07
Husband's occupation	−.14**	.00	−.34***	−.24***	−.20***	−.14***	−.13*	−.08
Wife's age at marriage	−.29***	−.15***	−.35***	−.15**	−.63***	−.59***	−.19**	−.09

NOTE.—All betas come from complete equation except overall ethnicity, where it is substituted for the three separate measures. Mean live births: U.S. raised 25–34 = 3.2 (N = 184); 35–44 = 3.9 (N = 179); Mexico raised 25–34 = 3.3 (N = 184), 35–44 = 4.7 (N = 114).

* $P \leq .10$.

** $P \leq .05$.

*** $P \leq .01$.

values that should be stronger as ethnic integration increases. Even when context "ethnicity" is included in the overall index, the net relations are about zero or slightly negative. But the pattern of results supports our initial suspicion that these context measures indicate residential and occupational ghettoization, not ethnicity. Subsequent analysis has shown that the social and media items form a Guttman scale, while the context indicators constitute a separate dimension.

The results reported here support the hypothesized operation of a mild negative minority-status effect on fertility even among this high-fertility minority. That occupational and residential segregation (and of course schooling and occupational status), but not sociocultural ethnicity, are positively associated with fertility suggests that structural forces analogous to discrimination and resource deprivation, not subcultural values, best explain high fertility among Chicanos (see Ritchey [1975] for a similar argument about black fertility). Unlike the usual situation (indirect measurement via residuals), where the choice of explanations is ad hoc, here we have definite evidence in favor of the structural explanation and against the normative.

As is often true when theoretical questions are made operational for empirical testing, our methodology is questionable and our conclusions tentative. The generalization of internal analysis to intergroup differences, the aptness of our measures and robustness of our findings, and the assumption that current ethnicity accurately reflects past ethnicity are all open to question, but these sorts of problems are hardly uncommon. On the positive side, we have gotten beyond the atheoretical custom of ad hoc and often sophistic "explanation" of residual differences and shown that direct measurement of that often invoked but little understood ethnicity dimension can at least suggest answers to questions that are logically unanswerable by the usual intergroup comparison approach. Ethnicity is not a bag of norms producing automatic responses (cf. Yancey, Ericksen, and Juliani 1976), nor is it a quality one has or lacks. Like any interesting sociological factor, ethnicity is variable and relative, not reducible to black/white categories. Similar internal analysis must be carried out in a variety of ethnic and minority populations before general conclusions are possible. The results reported here suggest that such a line of inquiry could substantially clarify and revise current ideas about ethnic minority-fertility differentials and possibly other "ethnic" effects as well.

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Commentary and Debate

The comments printed in this section are limited to brief critiques of articles and reviews published in the *AJS*. These comments are expected to address specific errors or flaws. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Authors of articles and reviews are invited to reply to comments, subject to the same length limits. The *AJS* does not publish commenters' rebuttals to authors' replies. Longer or less narrowly focused comments on articles published in the *AJS* should be submitted themselves as articles.

COMMENT ON "DEVELOPMENT TOWNS IN ISRAEL" BY SPILERMAN AND HABIB

The purpose of the paper by Spilerman and Habib (*AJS* 81 [January 1976]: 781–812) was to demonstrate the impact of development towns in Israel on (1) the areal distributions of ethnic groups, (2) the concentration of various ethnic groups in certain industries, and (3) the occupational opportunities available to various ethnic groups within the local industries of development towns. The purpose of this comment is to take issue with the basic direction of that article as reflected in its subtitle, "The Role of Community in Creating Ethnic Disparities in Labor Force Characteristics."

It is true that ethnic groups are distributed unevenly in the urban areas of Israel and concentrated disproportionately in certain industries. It is also true that occupational opportunities for various ethnic groups in the development towns are limited. The existence of these ethnic disparities, however, need not mean that the development towns have created them. Any claim that the towns themselves played a significant role in bringing about such disparities is necessarily exaggerated. Once it is understood that these communities have been "planted" by a government that has determined what industries and what kind of population are to be located in them, the underlying determinants of the phenomena studied by Spilerman and Habib can be clarified. Therefore one must examine the purpose and the ethnic composition of the development towns. Specifically, one should consider (1)

government policies on population dispersal and industrial development and (2) sociodemographic and attitudinal characteristics of the immigrants who were placed in development towns.

First, it is implicit in the government policy that development towns in Israel are planted urban communities. Spilerman and Habib are correct in suggesting that the towns were constructed in order to achieve population dispersal (p. 783, n. 3). It should be added that the policy of dispersing the population according to a plan was aimed not only at the new waves of immigrants but also at the Jewish population at large. The government developed a campaign entitled *Min Ha'ir La-K'var* (Hebrew for "from the city to the village") which was designed to encourage urban populations to migrate to the hinterland. However, the campaign succeeded only with new immigrants mainly of Asian and African ethnic origin.

Cohen (1970), in an important study on development towns (not cited by Spilerman and Habib), concluded that "the planned character of the new towns as well as the framework of their development—an apparatus of governmental and other public, central institutions—attested to the fact that they are 'planted' communities, established 'by decree' for overall social purposes, and have not evolved in accordance with the pressures and demands of local conditions. This means that these communities and their inhabitants are, or are at least considered to be, passive objects of manipulation by central agencies" (Cohen 1970, p. 589).

The fact that development towns were a direct result of government policy formulations leads to the question: "How did this policy affect the areal distribution of various ethnic groups?" From Spilerman and Habib's data (table 2), it can be seen that 66.0% of the foreign-born population of Asian and African origin were in development towns in 1961 as compared with 28.7% of immigrants of European origin. In order to explain the areal distribution of various ethnic groups, one should examine their sociodemographic characteristics. Spilerman and Habib, although they do not dismiss entirely the importance of individuals' characteristics, ignore them in analyzing various sociological dimensions of ethnic disparities. The dramatic differences in the level of education between Asian-African born and European-American born are presented in table 1.

Generally, the Asian-African immigrants were significantly less educated than other segments of the Israeli population, lacked specified occupational skills, and were relatively poor. These factors did not allow them to make a choice as to their location of settlement in Israel. Because of sociodemographic characteristics (low level of education and relatively large family), the Asian-African born were completely dependent on government assistance, such as subsidized housing, low-interest loans, and prospects for employment. In contrast, the American, and especially the European, immigrants were less responsive to the government policy of population dis-

persal; many of them simply refused to be located by government decisio Because of a higher level of education and occupational skills and social an economic support from the veterans (in Hebrew, *Vatikim*) in urban center European immigrants could start their new lives in well-established citie This was not a viable choice for the Asian-African immigrants who did nc have relatives in urban centers to help them in their initial steps of settl ment in Israel.

Spilerman and Habib's assertion that "new immigrants were *encourage* (italics mine) to settle in development towns" (p. 785) is a gross simplific tion of the process of immigrant absorption in Israel. As a matter of fac the new immigrants were not asked where they would prefer to settle bu were transported immediately upon arrival to their place of destination a determined by the Absorption Department of the Jewish Agency. The pla for areal distribution of new arrivals was based on past settlement of im migrants by their country of origin. Cohen (1970) indicated that immigrant were sent to various towns in small groups "comprised of people from th same country of origin and in some instances from the same city" (p. 597

Government policy was probably not alone, however, in bringing abou uneven ethnic concentration. The tendencies of the immigrants themselve appear also to have played a role. As Cohen goes on to say, "Even when a

TABLE 1

PERCENTAGE OF LITERATES IN JEWISH POPULATION (1948, 1954, 1957)

	Total	Males	Females
Jews:			
1948 (aged 15 and over)	93.7	96.8	90.4
1954 (aged 15 and over)*	85.0	91.8	78.3
Old-timers†	92.0	95.9	88.2
Israel born	95.2	98.0	92.7
Asian-African born	62.9	78.2	46.8
European-American born	97.1	99.0	95.2
New Immigrants	78.6	88.0	69.7
Asian-African born	59.3	77.5	42.2
European-American born	95.5	97.4	93.7
1957 (aged 14 and over)	85.6	90.9	80.4
Old-timers†	93.8	96.2	91.3
Israel born	96.1	97.4	94.8
Asian-African born	68.5	81.7	53.3
European-American born	97.4	98.6	96.4
New Immigrants	78.3	86.0	71.0
Asian-African born	60.6	74.8	46.9
European-American born	95.0	97.2	93.0
Others:			
1954 (aged 15 and over)*	42.8	64.1	21.0
1957 (aged 14 and over)	46.9	70.1	23.4

SOURCE.—*Israel Statistical Yearbook* 1957–58, table 21, p. 364.

* Percentage of those who had attended school.

† Israel born and immigrants until 1947.

immigrant group had initially been dispersed, there seems to be a tendency for kinsmen or neighbors from the country of origin to drift together" (1970, p. 597). Pockets of ethnic-group concentration in cities or geographic regions are a well-recognized phenomenon in modern immigration, particularly in North America. The strong ties of kinship among immigrants of Asian-African origin could produce a drift toward ethnic concentration even though those ethnic groups were initially dispersed by being settled in different development towns. However, this hypothesis should be verified empirically. In any event, government policy rather than the development towns themselves was primarily responsible for the areal distribution of ethnic groups.

The second point presented in the Spilerman-Habib article is that development towns influenced the concentration of immigrants in various industries. Again, to understand the problem of occupational opportunities in development towns one must recognize that the development of industry in these towns was an integral part of the government's policy on population dispersal. If Spilerman and Habib mean to argue that the occupational opportunities of the new immigrants were restricted by the type of industry that the government established, they are perfectly correct, but what they say needs to be supplemented.

Berler (1970) develops this point: "In dealing with the problem of industrialization of development towns we must therefore emphasize the role of the Government in the establishment of local plants" (p. 111). The bulk of funding of these industries came directly from government budgets, and the constructed plants received various governmental tax concessions. The new industries faced various problems, the most important being that the industries failed to become economically successful because of high production costs. Berler (1970) mentions, among other factors, the high production cost which resulted from expensive professional staffing; professionals had to be transported from other areas simply because they were not available among the population of the development towns.

In sum, new immigrants placed in industries that were built for them did not have an occupational choice because of their lack of professional skills. Weller (1974) indicated that the initial low standard of education was an important factor resulting in constant unemployment among the African and Asian immigrants: 90% of those seeking employment were unskilled workers (p. 103). Moreover, the restricted occupational opportunities for the new immigrants should also be examined in terms of aspiration for upward mobility for various ethnic groups. Don, Hovav, and Weller (1970) indicated that Yemenites and Europeans showed an interest in white-collar jobs, whereas the Asian and African immigrants were mainly interested in construction, factory work, small retail businesses, and commercial jobs. In order to demonstrate that new immigrants faced limited opportunities for

upward mobility in development towns, one would have to demonstrate that despite an upgrading of the educational and occupational skills of the population involved no corresponding changes took place in the structure of occupational opportunities. Studies of second-generation educational achievements, however, have shown that the gap between children of African-Asian origin and those of European origin was not substantially reduced (Lissak 1969).

In conclusion, given that the government made no substantial progress in upgrading the educational and occupational levels and the occupational aspirations of the African-Asian immigrants who form the majority of the population in development towns, it is questionable whether development towns themselves played any specific role in restricting occupational opportunities of their inhabitants. The concluding remark by Spilerman and Habib that community characteristics have been neglected in research on status attainment is of relevance to such research mainly in areas where significant discrepancies exist between the educational and occupational attainment of the population and its occupational opportunities. In Israel such a discrepancy is generally not observed.

It is therefore clear that the factors primarily responsible for various sociological dimensions of ethnic disparities in development towns are (1) the government policy of population dispersal and industrial development and (2) the sociodemographic characteristics of the population that inhabits the towns.

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REPLY TO VIGDERHOUS

Vigderhous states that his intent is to take issue with our assessment that development towns in Israel have contributed to ethnic disparities. He writes (paragraph 2), "The existence of these ethnic disparities . . . [in areal distribution, industry concentration, and occupational opportunity]

need not mean that the development towns have created them. Any claim that the towns themselves played a significant role in bringing about such disparities is necessarily exaggerated." Despite this indication of a major disagreement with our paper, Vigderhous's argument is largely a restatement of our own thesis. Moreover, in the few places where he adopts a contrary position, he *ignores* our analysis and results, rather than taking issue with them. We will consider his contentions item by item.

His first point (paragraphs 2–4) is that development towns "have been 'planted' by a government that has determined what industries and what kind of population are to be located in them." He further argues that the government's policy was to disperse all urban Jewish settlers, not just immigrants, but that the policy succeeded only with new immigrants. What do we (Spilerman and Habib 1976) say on these matters? "[Development towns] signify urban settlements . . . established by design, since 1948, and with considerable governmental assistance" (p. 784). ". . . the founding of a new town was preceded by comprehensive planning of physical facilities, industrial composition, and population growth" (ibid., n. 4). "The national government has followed a policy of extending incentives for certain kinds of industries to locate in development towns" (p. 794). On the matter of population policy we wrote: "[Population redistribution] was easier to achieve through encouraging new immigrants . . . to settle in the towns, than through stimulating the migration of veteran Israelis from the country's metropolitan centers" (p. 804). "The dependence of immigrants on public agencies offered a unique opportunity for altering the settlement pattern existing at the creation of the state" (p. 783, n. 3).[1] In short, we fail to see that Vigderhous is suggesting something different from our own remarks.

Vigderhous next inquires, "How did this policy affect the areal distribution of various ethnic groups?" (paragraphs 5–8). This same question is addressed in a four-page section of our paper (pp. 790–793), though he takes no note of our discussion. There is, incidentally, a serious confusion on Vigderhous's part over which population groups are being considered. Although he uses the term "various ethnic groups," it is evident that he is really concerned with two broad population categories—Asian-Africans and European-Americans. Thus he stresses the "dramatic differences" in education level between these categories (table 1) in regard to understanding their settlement locations.

[1] Sources for the statements above, as cited in our paper, are Berler (1970); Lichfield (1971); Matras (1973); and Brutzkus (1964). Vigderhous chides us for not referencing the Cohen (1970) article. That paper is an excellent one though it does not deal with our principal topic, the effect of development towns on ethnic disparities in national-level labor force characteristics. As a source of background information about immigration to Israel and the establishment of development towns, the information in Cohen (1970) is not different from reports in the sources we have used.

However, we investigated the areal distributions of 10 ethnic groups, five from each of the above continent-of-origin categories. Our concern was to uncover processes which explain residence location patterns at this disaggregated level. Education and degree of dependence on governmental assistance are relevant considerations and were hardly slighted in our study, but they do not account for the very different settlement patterns *among* Asian-African ethnic groups or *among* European ethnic groups. We argued that the various ethnics arrived in Israel at different points in time and were absorbed, to a disproportionate extent, in the towns under construction or expansion at the times of their arrival. We reported evidence for this thesis (pp. 790–92) in which, incidentally, it is shown that several *European* ethnic groups (Bulgarians, Poles, Rumanians) are also overrepresented in towns that were expanding at the time they immigrated to Israel. It is by virtue of such processes that the establishment of development towns has contributed to ethnic differences in residence location, the first point in our paper.

In paragraph 7, Vigderhous takes issue with our statement to the effect that new immigrants were "encouraged" to settle in development towns. He desires stronger phrasing: "As a matter of fact, the new immigrants were not asked where they would prefer to settle but were transported immediately upon arrival to their place of destination as determined by the Absorption Department of the Jewish Agency." Yet, in his preceding paragraph, Vigderhous states that there was some choice: "[T]he American, and especially the European, immigrants were less responsive to the government policy of population dispersal; many of them simply refused to be located by government decision." Vigderhous can't have it both ways. In point of fact, governmental policy vis-à-vis locating immigrants varied with time and other considerations, but the basic theme was one of providing incentives for settling in outlying areas, which is what we had written: "[N]ew immigrants were encouraged to settle in development towns, with subsidized housing, low-interest loans, and the promise of employment serving as inducements" (p. 785).[2]

In paragraph 8, Vigderhous describes mechanisms which function to keep kinsmen together, whether in Israel or in the United States. He writes, "Pockets of ethnic-group concentration in cities or geographic regions are a well-recognized phenomenon in modern immigration, particularly in North America." Quite so. He might find our remarks on this matter to be of interest. On page 808 we begin a lengthy discussion with the sentence, "The sorts of mechanisms that have generated ethnic concentration by region and community in America concern time of arrival, route of travel.

[2] For details on governmental policies toward new immigrants in relation to recruiting settlers for development towns, consult Matras (1973).

and degree of affinity of a group for its own kind." In short, Vigderhous is parroting our statements.

A second issue addressed in our paper concerns the impact of development towns on the distribution of ethnic groups among industries. Vigderhous raises this matter (paragraph 9) but actually elaborates upon a different topic, the impact of the industry composition of development towns on the occupational opportunities of their residents. Before turning to that topic, we summarize our argument (pp. 793–98) on the former issue: Because the Israeli government encouraged certain kinds of industry, in which large plants are common, to locate in development towns, most are unbalanced, one-industry communities;[3] they specialize in textiles, food processing, or mining. At the same time, for the reasons we presented, the various ethnic groups tend to be concentrated in different towns. This has created a situation in which each has available different industry options and has resulted in a corresponding pattern of industry affiliations by the groups.

Turning to the occupational character of development towns, Vigderhous comments (paragraph 9): "If Spilerman and Habib mean to argue that the occupational opportunities of the new immigrants were restricted by the type of industry that the government established, they are perfectly correct, but what they say needs to be supplemented." We appreciate the agreement but can find no cause for ambiguity in our argument. We wrote (p. 800): "We wish to make clear the structural underpinnings of the occupational differences among settlement types. The differences derive principally from the kinds of industries located in the communities, and relate only indirectly to the skill levels of the inhabitants." Further, "we have characterized development towns as locales in which few moderate-status positions are available, this limitation deriving from a concentration of low-skill industries in the settlements" (p. 803). With respect to "supplementing" our argument, Vigderhous's elaboration is again a restatement of our own points, as the following comparison illustrates.

Vigderhous elaborates (paragraphs 9–10): "[T]o understand the problem of occupational opportunities in development towns one must recognize that the development of industry in these towns was an integral part of the government's policy on population dispersal. . . . The bulk of funding of these industries came directly from governmental budgets, and the constructed plants received various governmental tax concessions." In comparison, we wrote (p. 794): "The national government has followed a policy of extending incentives for certain kinds of industries to locate in development

[3] Our characterization refers to the organization of the towns in 1961, the year of the census information available to us. For comments on future prospects of the towns, see Spilerman and Habib (1976, pp. 807–8).

towns. . . . For locating in a development town, firms are granted tax reductions and low-interest loans. . . . The kinds of industries that have been given preference are ones which either exploit the resources of a region . . . or are labor intensive and provide many jobs at a low initial capital cost."

In his final paragraphs, Vigderhous comments on the third of our principal contentions, that development towns, by virtue of containing limited occupational opportunity (as a result of industry composition), have contributed to ethnic disparities in occupational standing. Here we cannot claim that he is parroting our findings, only that he did not read them. He contends that "it is questionable whether development towns themselves played any specific role in restricting occupational opportunities of their inhabitants." He adds that community characteristics are relevant to status attainment mainly "where significant discrepancies exist between the educational and occupational attainment of the population and its occupational opportunities. In Israel such a discrepancy is generally not observed." Yet we report precisely such a disparity! Controlling for education, age, and length of residence in Israel,[4] 11% of the gap in occupational status between Asian-African and European immigrants is attributable to their different community locations (pp. 802–3). Moreover, this is an estimate of the lower bound to the effect of community; if we view educational attainment as a consequence of settlement location (such as would arise from poor schools being located disproportionately in development towns), the estimate of the community effect rises to 21%.[5]

A final bit of evidence concerning the impact of limited occupational opportunity in development towns may be garnered from an examination of who leaves the towns. We reported (pp. 804–5) that migrants to other types of communities tend to be better educated than either nonmigrants or migrants to different development towns. This is the pattern one would expect to find if the towns do constrain occupational advancement. Other capable residents surely choose to remain in the towns, sacrificing career prospects in order to reside close to family and friends. This is one sort of mechanism which gives rise to the community effect noted in the preceding paragraph.

In summary, Vigderhous's comment is a restatement of our own argu-

[4] That is, for the individual-level variables which Vigderhous (paragraph 5) believes we "ignore" in analyzing ethnic disparities.

[5] Our argument about the costs in occupational status from residing in a development town has been replicated. Using data from the 1974 Israeli National Mobility Survey, Kraus and Weintraub (1977, p. 25) report that 10% of the gap in status between residents in "central" and "peripheral" settlements is due to the community effect. Their specification of peripheral settlement corresponds closely with our definition of development town.

ments. Where he differs from our views he tends to ignore our analytic results rather than address them.

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COMMENT ON GUSFIELD'S REVIEW ESSAY ON *BECOMING MODERN*

Joseph Gusfield's review essay on Inkeles and Smith's *Becoming Modern* (*AJS* 82 [September 1976]: 443–48) speaks eloquently of the "feeling of emptiness" which the study produced in him. He attributes this primarily to the authors' stress on a polarity between the "ideal types of 'tradition' and 'modernity' " (p. 443) which neglects the interaction between them so typical of rapid social change. Although I have found the work of Alex Inkeles and his colleagues consistently stimulating, my own research on the industrial entrepreneurs of Pakistan leads me to agree with Gusfield. Indeed, it was the traditional social organization, based on ethnic and kinship bonds, which made major innovations in business organization possible for the new Muslim industrialists.

But *Becoming Modern* also leads me to feelings of emptiness of another sort. The title promises more than the book delivers: the authors deal only with the process of making *men* modern (Inkeles 1969). What are the impli-

cations for social science of a major study of social and individual tran formations that totally omits women, not only as subjects of study, but al in its basic assumptions about women and men in society? As long as inte action between women and men is characteristic of a social process—as surely true of individual modernization—social theory and methods mu include both women and men. But *Becoming Modern* is based on a visio of women's place in society which makes their omission from the stud population possible. Though not explicit, this view can be inferred fro statements about methods and findings. Recent research extending Inkeles methods to the study of a few samples of women (see Inkeles 1976, p. 11 hardly alters the basic assumptions and social vision of the larger study. Th assumptions grow out of prevailing stereotypes about middle-class wome in industrial societies. They do not accurately reflect even this limite segment of women. They are hardly universal.

To be sure, when Inkeles and his colleagues began their study in th early 1960s (see Inkeles 1969, pp 208–29), they did not need to fear criticis of a traditional approach which assumed that women were as marginal t most major social processes as they were to the academy. But the cons quences of this assumption pervade the study and raise serious question about many of its aspects. I suspect that if the implications had been clea from the start, the "practical considerations arising from the limits on ou budget" (p. 311) which are stated to have caused the omission of wome from the sample might have accommodated a different research strategy.

A few examples will have to serve. To begin with, the omission of wome from the sample of 6,000 persons in six countries was justified by the author because there was a "concentration of men in the industrial jobs in whic we were especially interested" (p. 311). In other words, there was a hig degree of occupational segregation by gender (Blaxall and Reagan 1976) Actually in some of the countries studied, substantial numbers of wome are in the paid labor force, usually in segregated "women's occupations."

Occupational segregation by gender produces a dilemma in the interpreta tion of *Becoming Modern.* Having decided to "classify as modern thos personal qualities which are likely to be inculcated by participation i large modern productive enterprises such as the factory" (p. 19), Inkele and Smith also expect that "the pattern [of modernization] which wil eventually emerge for women will be broadly similar to that we observe in the case of men" (p. 311). This expectation is hardly consistent with a methodology which selected only men because the labor force was highly segregated. Nor does it follow from Inkeles's stated belief that "what we find in individuals does, in good part, reflect the nature of the social system they live in and their particular statuses within that system" (1976, p. 107). If women, in fact, do not participate in "large modern productive enter-

prises" to the same degree as men, what are the mechanisms of their modernization? The available evidence suggests that technology often displaces women from paid employment in newly industrializing countries. Affluence, and a growing middle class, often produce the withdrawal of women from paid employment, at least for a time, in spite of longer schooling. What are the implications of the modernization of women for the modernization of men who are not in large modern productive enterprises? How do they affect theory?

Women appear only indirectly in *Becoming Modern*—in the process of measuring male attitudes. The authors conclude that the "more modern man . . . *permits* his wife and daughter to leave the home for *more active participation* in economic life" (p. 313, italics mine). One could hardly ask for a clearer statement of the basic misperception of women in society. The notion of "permitting" the women of the family to seek paid employment implies a distribution of power and control within the family which varies among classes and between societies, as well as individuals. In many societies, the existing division of labor precludes even the option of "permitting" paid employment, especially where the active participation of women in economic life is indispensable to family survival, whether this work is carried out within the home or outside it. Inkeles and Smith also state that one aspect of a man's modernity is reflected in his attitudes toward "a woman's right to work and to equal pay, to hold public office and to freely choose her marriage partner" (p. 26). These attitudes are often highly differentiated and refer to several quite distinct issues. Class and ethnic differences affect them profoundly, even within a single occupational grouping. This makes it difficult to see them as good indicators of men's modernity. Moreover, although Inkeles and Smith do not say so, we can only assume that they are speaking of women's right to paid employment, not of their right to work as such. Here again, the authors follow current social science tradition, in which the inappropriate term "working women" (meaning employed women) is as hard to eradicate as crabgrass. Conceptually, the term prevents an accurate analysis of the varieties of women's work and its role in society and economy.

A major consequence of the omission of women is the neglect of the family as an arena of modernization. Historians of the family have recently raised crucial issues affecting studies of modernization (see review article by Hareven 1976). Even within the restricted confines of *Becoming Modern*, however, the omission of women and family interaction raises major questions. For example, what were the consequences *for the men* of being involved in close daily interaction with people usually less "modern" than themselves? Were the most modern men, in fact, those who lived in factory dormitories

away from their families? What is the role of families in the process of on-going attitude change and how do families alter as individuals become modernized? Inkeles and Smith recognize the role of families, often in terms of the men's background, but once again women are omitted. For example, father's education is used as "an objective index of a potentially more modern home atmosphere" (p. 240). Mother's education is not. This omission is oddly at variance with the consistent references to maternal influence found in so many biographies, notably those of ambitious men of achievement. Do the authors assume a high correlation between mother's and father's education, or assume that the education of mothers is irrelevant in the home atmosphere, or that they influence their sons in ways unaffected by formal education? Or was it simply easier to concentrate on the fathers?

Following Inkeles's own belief that individuals reflect their social system and their status in it, I would suggest that modernization may be a very different process for women than for men and that the attributes of modern women may be rather different from those of men. This is contrary to the conclusion claimed by Inkeles (1976) for several follow-up studies. The work of Jean Baker Miller (1976), on the other hand, suggests some of these qualities, particularly the ability to provide emotional support to others under stress, as a key element of adaptation by women to modern life. This suggests many different patterns of modernization and a different process from that suggested by *Becoming Modern* and its predecessors. This is an important issue, particularly if social science affects social policy in newly industrial societies.

Women may also be better equipped than men to respond to rapid change in and through their work. Much recent research on women in poor countries, and on poor women in affluent countries, deals with women's small-scale enterprises, exchange of services among women, barter systems in slums, informal labor markets, and the work of women migrants. These studies suggest great flexibility among women in conditions which often defeat men—surely a very modern attribute. It may well be that women can cope so readily with rapid social change because they are *less* involved than men with large-scale productive enterprises and retain a degree of autonomy which men in factories have lost.

Whatever differences and similarities may be found between women and men, however, it is indispensable that both be studied in research on major social structures and processes. Women and men interact constantly and profoundly. What happens to one affects the other. Social theory must reflect this interaction without assuming the marginality of one group and the centrality of the other. For the future, I take comfort in the paradox that in social science, as in the rest of human affairs, the most important areas of knowledge are often those about which we keep quiet the longest.

But while silence may be the best indicator of significance, in the long run it must be broken.

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WOMEN AND MODERNITY: A RESPONSE TO PAPANEK

I understand that Professor Inkeles is preparing a reply to Professor Papanek. As an author of several papers using the six-nation data, including work in progress analyzing male attitudes toward women's rights, and as someone deeply interested in understanding the antecedents and consequences of women's place in sexist societies, I am led to respond to several major points she raises. I do not believe her questions can be dismissed lightly, but at the same time I view them with a perspective that differs somewhat from hers.

Papanek's critique attacks both the ideological basis of the six-nation study and the value of its findings. Regarding the first, I believe that her imputation of a highly sexist view of women's place in society to the study and its originators is unwarranted. The main purpose of the study was to analyze how experiences with industrial institutions, particularly the factory system, impinge on individuals in rapidly industrializing societies. This theoretical interest plus practical considerations dictated the choice of men over women as subjects and the choice of data about experiences with specifically industrial institutions over data about past and present family experiences. I do not believe it is necessary or correct to attribute a vision of women as marginal social beings to those conceiving the study in order to explain the decisions made.

The fact remains that, in this study, data on women are lacking in im-

portant areas. What implications does this have for what the study can tell us?

I think the most serious challenge raised by Papanek is her point that an understanding of family dynamics such as mother-child and husband-wife interactions is critical for a full understanding of how the modernization process impinges on individuals. The family is a cause as well as an effect of modernization, as shown by Goode (1970), Hareven (1976), and others. Further research is needed to specify the situations in which various family relationships impede or facilitate the modernization of individuals. In the case of the six-nation study, data on the mothers and wives of the male respondents would add greatly to the explication of the process of individual modernization. I think it is unlikely that such data would change the basic conclusion that particular social experiences such as factory work affect individual modernity, but testable hypotheses along this line remain for other studies.

The process of how women themselves become modern, apart from how they affect the modernity of men, is a question for which I believe the six-nation study can provide at least a starting point. *Becoming Modern* offers a paradigm for how particular institutions such as the factory affect individual attitudes. The factory is viewed as a "school" which teaches particular attitudes and values, and its effects are shown to be additive to those of other social structures such as schools and the mass media. Women are also affected by their experiences within social structures, but men's and women's experiences with social structures in sexist societies are different in many ways, some obvious, some subtle. This would mean that women might generally emerge as modern in ways somewhat different from men. In other words, women generally "learn" modernity in different "schools" from men. Having learned what conditions appear to affect the modernity of men, we can ask how conditions experienced by women are different or similar and what the outcomes might be. For example, given occupational segregation by gender, we can ask what conditions are associated with "women's work" and how these conditions affect attitudes. I think it is still likely that, given comparable structural conditions, men and women react similarly. Olsen (1974) provides evidence that this is so in a study of the conditions impinging on mothers in Taiwan, in which she compares her findings with others on how comparable conditions affect fathers. J. Miller et al. (1977) have also found basic similarities between the sexes in a study of occupational effects on the psychological functioning of women and men. Other evidence is provided by Suzman (1974), who found that among black respondents in Boston the modernity of employed women and men was predicted similarly, with occupational experience a major predictor, whereas for nonemployed women early experience was the major predictor of modernity. Just how women react to their work and other aspects of their situation,

across a range of social structural conditions, is a question about which we really know little, and I agree with Papanek and others such as Hareven (1976) that further research in this area is needed.

An area in which the six-nation study does have data is men's attitudes toward women's rights. Contrary to what Papanek implies, the study is measuring *not* actual power and control in relationships but an egalitarian set of *attitudes* which can be taken to form a personal ideology. I think that the study of sexist ideologies provides valuable information so long as we keep in mind that ideology does not have a one-to-one relation to behavior. What we can gain from an analysis of the six-nation data on men's attitudes toward women's rights is some sense of how sex equality as an ideology among men is affected by the modernization process. What this tells us about actual progress in women's rights is another question. Certainly male attitudes do not completely determine women's place, but they do affect it. The degree is an empirical question, and it presumably varies by culture, social class, and issue. The data of the six-nation study cannot be used to address the question of the relationship between attitudes and behavior, but they can show variations in the attitudes themselves in relation to other variables.

In sum, the six-nation study provides us some theoretical ideas and some limited data relevant to the interrelationships of women and men in industrializing societies. There are many important questions left unanswered by *Becoming Modern* and the other work coming out of the six-nation study. This is true for any study. I have tried here to delineate briefly what I see as its enduring value to us as we address the important questions concerning the place of women and men in societies they share.[1]

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[1] ED. NOTE.—This rejoinder was solicited by the editors at the suggestion of Dr. Alex Inkeles, who is preparing a related research note for possible publication at a future time.

Review Essay: Outsiders' Politics

The Strategy of Social Protest. By William A. Gamson. Homewood, Ill.: Dorsey Press, 1975. Pp. xiv+217. $16.50 (cloth); $6.95 (paper).

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In the 145 years between 1800 and 1945 over 500 organized protests challenged the existing structure of American politics. The diversity of their constituencies, targets, and objectives was considerable: Workers claimed the right to bargain collectively or protested wage labor and private property. Tobacco farmers rode at night to break the tobacco trust. Women claimed suffrage, fought for birth control and against slavery. Native-born Protestants challenged the rising power of immigrant Catholics. Urban blacks marched on Washington to protest job discrimination. Middle-class reformers tried to abolish slavery, reorganize prisons, make war illegal, and undermine urban political machines. Violent and nonviolent conflicts were normal incidents of such protests: riots, arrests, boycotts, strikes, sit-ins, demonstrations, guns, clubs, and tear gas were advocated or used by or against three out of every five social movement organizations. But in the end these protests were surprisingly successful: three out of every five movement organizations were accepted as a legitimate interest or accomplished half or more of their program objectives within something like 20 years. Of these, two out of every five were both accepted as legitimate representatives of a legitimate interest and accomplished at least half of their programs. One in 10 failed to accomplish even half of what it had set out to do but was nevertheless accepted as a legitimate interest. Another one in 10 was not accepted as legitimate at all but nevertheless accomplished at least half of its concrete objectives.

What was the nature of these movements? Why were violence and coercion so common? What were the conditions of their success? In *The Strategy of Social Protest* Gamson, like Tilly, argues that riots are merely politics by other means. Movements are violent and coercive because they are the politics of the disprivileged, the unrepresented. Except for their coercive tactics, the conditions of their success are the conditions of any other political organization's success: selective incentives, bureaucratization, centralization, specific and limited goals. One effect of Gamson's analysis is to blur the boundary between movement politics and normal politics; another is to define more clearly the boundary between the politics of the represented and the unrepresented.

Gamson's method is brilliantly simple: he has consulted standard works of reference and listed every collective episode and every organization in

America between 1800 and 1945. An 11% sample of the 4,500 organizations discovered by this means was combed for social movement organizations mobilizing a previously unmobilized constituency (ruling out established pressure groups) to influence a target outside themselves (ruling out revivalist movements). About the 64 movement organizations surviving this filter he was able to find sufficient information, either in primary or secondary sources, to answer a fairly large number of questions about the issues, tactics, and organization of 53 of them.

Were they irrational protests of the uprooted and disoriented or rational instruments of political struggle? In *Strategy*, Gamson argues that the politics of social movements are neither more nor less rational than the politics of parties and pressure groups. The central purpose of the book is to break through the distinction between the two kinds of politics according to which movements are caused by social disorganization, are nonrational or irrational in their purposes, conditions, and consequences, and should be interpreted in largely symbolic terms because the connection between their interests and actions is so indirect; while "normal" politics, that of parties and pressure groups, is organized, rational bargaining. *Strategy*'s approach unifies the riot and the lobby. Both are the outcomes of rational decision making in which movements, like pressure groups, calculate consequences, weigh alternatives, and rationally allocate means to ends. Not that their observable behavior is the same. But they do not differ because the underlying laws of their behavior are different or because one is the social pathology of "collective behavior" while the other is the utilitarian bargaining of organized society. The underlying laws of their behavior are the same. Their behavior is different only because their political situations are different.

How different? Normal politics is the politics of recognized, established interests, the politics of "members." Movements are the politics of "nonmembers." Membership is a matter of legitimacy: "members" are those whose interests are recognized by others as valid and whose views are taken into account when decisions are made that affect their interests. "Nonmembers" are those whose interests and participation are not recognized as valid by others. Even the most democratic polity has its nonmembers: all polities make rules about children, none of them consult the opinions of children about the rules. Nonmembership may of course be more subtle: the political nonmembership of the young or the poor consists not so much in disenfranchisement as in their lack of organized pressure groups that routinely protect their interests. The boundary between member and nonmember is for Gamson the boundary that divides the represented, those whose interests are protected by pressure groups, from the unrepresented. The argument in *Strategy* turns on the boundary-maintaining behavior of members. When nonmembers have some interest at stake in the outcome of collective decisions and sufficient resources to mobilize and organize, they are frequently found struggling for citizenship rights. But members do not readily concede these rights to nonmembers. The boundary that divides them is privileged. Nonmembers are therefore forced by their illegitimacy

as political interests to make use of illegitimate political tactics. And illegitimacy licenses the legitimate use of the force of the state against who violate its rules. Demonstrations, sit-ins, riots, and arrests are ther normal politics at the boundary.

What are the conditions of success in crossing such a well-defe boundary? Gamson gives some careful thought to the problems of asse the fate of movement organizations. They are "accepted," that is, tr as legitimate interests, if members are consulted, included, given stat authority when decisions are made. They are effective (although that i Gamson's term) if they accomplish at least half of their concrete pro objectives. Almost no movement succeeds in displacing established men of the polity; hence movements with revolutionary agendas (in the Ame setting) routinely fail (chap. 4). But otherwise it makes surprisingly difference how radical a movement is (chap. 4). Multiple issues turn o be less, not more, successful in attracting a following, making a move acceptable, or increasing its effectiveness (chap. 4). Because they of public good, movements are more accepted and more effective if they su ment pure devotion to the collective goal with more selective incen (chap. 5). They are more likely to survive if bureaucratized, are more e tive if centralized; and bureaucratization and centralization have inde dent, additive effects on the accomplishment of program objectives (chap Wars increase the likelihood that a movement will be accepted but dec the likelihood that it will be effective (chap. 8). Historical period has or no effect on any of these relations (chap. 8).

But the heart of the book is its analysis of tactics (chap. 6). This ana is the foundation at once of the argument that politics at the boundary unruly and that the unruliness of movements is merely politics by o means. Gamson establishes with care and deliberation the correlatio violence with effectiveness (though not acceptance), and violent and violent coercion taken together are correlated with both the acceptance effectiveness of social movements. He does not argue that they are suffic to guarantee a movement's success; in fact, he concludes that their cor tion with success is spurious. Violence and coercion are a consequenc the same underlying factors (such as widespread support for the moven outside the movement itself) that make success likely. But the succes the unruly makes it implausible that violence and coercion are coun productive or the desperate measures of failing movements.

It is on the unruliness of success that Gamson rests his case against pluralist description of the structure of power in America. Gamson's brilli concise "Stable Unrepresentation in American Politics" (unfortuna published in the obscure *American Behavioral Scientist*) made the per ability of American political boundaries a central issue in the disp between pluralists and neo-elitists over the nature of power in America. wasted no time arguing that the elite are coordinated or cohesive. L Lowi, Bachrach, and Baratz, he conceded the pluralist description American politics as the politics of multiple interests, organized in press groups, each with some power to influence the outcome of collective d

sions. But again like Lowi et al., he argued that this was only the view from the inside. Outside the pressure system, Gamson claimed, there is a large underclass of the systematically disprivileged, the "stably unrepresented," doing little about it and essentially invisible to both the active participants in the system and the pluralists who study them. The boundary dividing the unrepresented from the represented is not inert; it actively prevents the unmobilized, unorganized unpeople of a polity from mobilizing and organizing. It prevents them from pursuing the kinds of legitimate tactics of influence open to mobilized, organized, established interests. Gamson questioned how readily even a pluralist polity would accept the disprivileged if they did mobilize. From the evidence in *Strategy*, it has to be said on the side of the pluralists that the barrier is not insurmountable. But on the side of the neo-elitists it has to be said that the costs of mobilization are great and the struggle protracted and seldom peaceful.

This book, then, has three conclusions to support: that movements are merely politics by other means, that established centers of power may be plural but are privileged, that unruliness is normal politics at the boundary which divides the represented and the unrepresented. How well does the evidence of the book support these conclusions? This is a bold, exciting, important book. But it does have some problems, and four of these seem particularly serious.

First, the 53 cases are not independent. Almost a third involve unions. Was the fate of the Steel Worker's Organizing Committee (SWOC) really independent of the fate of the early AFL? Some of these unions shared a common past; some of them were more (or less) successful because of the fate of others also in the sample. I have some doubts, therefore, about just how many cases Gamson really has and about just how much of his story is merely the union history of America.

Second, the effectiveness of a particular movement organization usually cannot be uniquely attributed to its own incentives, organization, issues, or tactics. What Gamson measures is whether half or more of a movement organization's objectives had been accomplished, either in its own view or that of historians, within 15 years after its challenge ended. But more than half of the movement organizations in Gamson's sample had competitors. Even without that, there were obviously other forces besides the movement organization at work to accomplish the same ends. And Gamson's task is not simply to discover whether particular ends were accomplished; it matters how that accomplishment was brought about. If no unique causal imputation is possible, half of the book (the half about effectiveness) is inconclusive. If the behavior of other forces is uncorrelated with the characteristics of sampled organizations, the only effect is an increased noise level. But how seriously would we take Gamson's conclusions about violence if one of the effects of a movement organization's violence were to increase the acceptability and effectiveness of its less violent competitors? The error would in that case be correlated with independent variables in Gamson's model. The broad picture would be much the same: politics at the boundary

would still be unruly. But its details about conditions of effectiveness would become blurred.

Third, there is a theoretically important gap in the results. There is no distinction between mobilization of the unpeople of a polity and mobilization of those who are members for other purposes. The League of American Wheelmen satisfies Gamson's definition of a "challenge group." The Wheelmen mobilized a mostly white, male, middle-class constituency to remove restrictions on the use of the bicycle. Gamson defines a "challenge group" in terms of the mobilization of new commitments and commitments in terms of specific interests. Hence the constituents of the Wheelmen are wheelmen, not the white, male middle class. The Wheelmen are "outsiders" because wheelmen are outside the established pressure system of American society. It makes no difference that the white, male middle class satisfies preconditions of membership that the constituencies of the Young People's Socialist League, the March on Washington, the Federal (female) Suffrage League, or SWOC do not. But the League of American Wheelmen and the March on Washington make strange bedfellows. This is a fact of some importance, because legitimacy is what the argument of the book is all about. The Wheelmen claimed new rights, but for a constituency already represented for other purposes in the pressure system. The Federal Suffrage League, the March on Washington, and SWOC claimed citizenship rights for a new interest that was also a more systematically disprivileged class. Are there no differences between the barrier that defends the pressure system from the unrepresented who are "members for other purposes" and that which defends it from the unrepresented who are not? It is of course consistent with the argument made in "Stable Unrepresentation" to equate legitimacy (membership) with mobilization. And Gamson may be right that citizenship qualifications, preconditions of membership, make no important difference in how the process of acquiring membership behaves. But *Strategy* does not try to offer empirical support for this assumption, and it seems to me a question of importance.

This objection may be too strong. Gamson does report (in a candid and useful appendix on negative results) that he analyzed constituency effects without finding much. But the fourth serious problem with the book is its tabular method of analysis. It is a very readable work, accessible to anyone who can read Zeisel. But it cannot control more than one conditional or confounding factor at a time. (The analysis also omits age and sex as conditions of legitimacy, but I suspect that the more serious difficulty is methodological.) Measurement is primitive; in fact it deals mostly with dichotomies. It therefore cannot take any of the advantages of distance or order that more precise measurement would provide. The *N* is small: although 53 cases is a sizable sample of historical organizations, it does not leave much room for analysis. Crucial analyses, like that of violence, depend on such small cell sizes that a shift in three cases would substantially alter the conclusions of the book (see the χ^2 computed, on cells of seven and eight cases, in table 5-2 on the success of violence). If constituency had no effect, I would be very suspicious of the whole Tilly-Gamson theory of political

violence. But its effect may be conditioned or canceled by some combination of organization, incentives, tactics, and issues ($r = 0$ is just as likely to be spurious as a positive or negative correlation, and for the same reasons). The only control Gamson can make is for issue (whether the movement sought to displace anyone). I therefore suspect his method more than his theory.

But the difficulties in the way of completing this investigation are so formidable that it is amazing the book was done at all. How do you list such an evanescent universe? What are its units? How do you randomly sample them while ensuring that adequate information is available about the sampled units? How do you measure the success of a movement, compare issues between movements, or determine how fundamental a challenge they make (without depending tautologically on how the established order reacts)? No one can accuse Gamson of faint heart or weak liver. This is a bold book. He is sometimes ready to settle for rough-and-ready methods, if they are necessary, in order to get on with it; but he faces most of the risks with brilliance, imagination, and good common sense. It is a book that makes the unfeasible suddenly look feasible. But it is important also because it advances significantly what we know about collective violence, power in America, and the relations between them. Compare it with other recent, outstanding contributions to these three subjects: one natural comparison is with Shorter and Tilly's *Strikes in France*. *Strategy* is neither as good nor as important a book, but it is in some ways more daring. It disaggregates collective violence, a methodological shift that makes it possible to study, directly instead of indirectly, the organizations that participate in it. *Strategy* therefore significantly extends the argument of *Strikes*. A second natural comparison is with Bachrach and Baratz's *Power and Poverty* or Lowi's *End of Liberalism*. Together, the three will constitute the trinity of neo-elitist thought about power in America. *Strategy* is less important; it breaks fewer new paths. But it is empirically compelling in a way that neither of the other two is. *Power and Poverty* is theoretically penetrating, but its case studies are thin, superficial, and underanalyzed. *The End of Liberalism* is much denser and more literate, and its case studies are superior; but they are case studies of the devolution of power, not the defended boundary. *Strategy* is flawed, but its flaws are largely of detail: it requires refinement and replication to establish how violence is related to politics at the boundary, what precisely is the place of legitimacy in the theory of politics at the boundary, and exactly how it is that movements are effective. But the basic facts of the defended nature of the boundary, the (political) nature of movements, and the unruliness of boundary politics are not blurred by these flaws. It therefore provides a (missing) empirical foundation for neo-elitist thought. A third natural comparison is with Lowi's *Politics of Disorder*. This is by far the closest comparison: both books are about the relations between normal and movement politics; they grow out of similar critiques of pluralism; they deal with many of the same ideas (the inertia of the pluralist pressure system, the defended boundary, the unruliness of mass politics). Both ask fundamental questions about some fundamental

things and are therefore important. *Strategy* is not as elegant, does not have the moral uplift, is not as close to the major political and social (as distinct from theoretical) issues of our time. But it faces the empirical difficulties of the subject while *Disorder* evades them. *Strategy* is therefore a more stimulating point of departure. Whatever conceptual refinement or empirical replication it requires, its shortcomings are more fruitful than *Disorder*'s virtues.

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Review Essay: The Almost Compleat Social Scientist

Collected Papers. By Kenneth E. Boulding. Edited by Larry D. Singell. Boulder: Colorado Associated University Press. Vol. 3: *Political Economy*. 1973. Pp. 614. $12.50. Vol. 4: *Toward a General Social Science*. 1974. Pp. viii+623. $12.50. Vol. 5: *International Systems: Peace, Conflict Resolution, and Politics*. 1975. Pp. x+497. $12.50.

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I first came across Kenneth Boulding while I was a graduate student at the University of Michigan. On a spring day in the mid-1950s a group advocating the cessation of atomic testing invited members of the academic community to a silent vigil on the quad. I had expected a small crowd and was surprised to see only one solitary figure. "Who's the guy by the flag pole?" I irreverently asked a friend. "That's Professor Boulding," he replied. Then, and on numerous later occasions, this man was part of Ann Arbor's conscience in a world awash in realpolitik. Two years later I went to ask him for permission to take a seminar in economic development. He was pleased to have me join, for he believed that sociologists have much to learn from economists—and from other disciplines generally. His office in the ancient economics building was reminiscent of an Albrecht Dürer engraving—books piled high, a crowded desk, sunlight filtering through small-paned windows. Only the sleeping lion on the floor was missing.

These two images of Boulding—the active believer in peace and the scholar with wide-ranging interests—pervade his three massive volumes of papers written during the last 30 years. Within each book, the previously published articles, lectures, reviews, book chapters, and contributions to reports and symposia are arranged chronologically, giving a sense of development and time. For those who have not read Boulding's books, these collections provide a good overview of his lifelong interests and work.

One is immediately struck by Boulding's lucid writing, his elegant way of putting across complex ideas, and his often personal and sometimes whimsical style. It is pleasant indeed to encounter the "wallpaper principle" (prediction based on pattern), the "pinocchio principle" (a front organization takes on a life of its own), the "mesa principle" (some decisions don't matter much), and the "bathtub theorem" (the relation of production, consumption, and stock), among other gems.

In these papers Boulding appears mainly as a practitioner of integrated social science in the best sense of the phrase: bringing specialized propositions and esoteric ideas from various sources to the study of the diverse phenomena that bedevil modern man. The method includes good, clear

writing, striking metaphors, retention of scientific integrity, little jargon, but unfortunately, few references. The subjects include not only economics but also agriculture, technology, ecology, education, armaments, war, conflict, peace, and international relations. The apparent audience is equally diverse: decision makers, specialists, students, and, for the most part, a generally educated and concerned public.

Only a few papers seem dated, others would require little revision to be as relevant as when they were written, and many are as interesting and timely today as when they first appeared (such as "The Menace of Methuselah: Possible Consequences of Increased Life Expectancy"). Furthermore, the reader gains a good perspective of recent history, change, and time, by comparing early statements with the actual later events—for example, the limitations of policies and planned social change (4:249 ff., 347 ff.). One can easily become enchanted by the intellectual adventures charted on these pages—kaleidoscopic trips through the several social sciences, their methods and perspectives, diverse human problems and their far-flung causes and possible solutions.

The papers in these volumes are not primarily concerned with economic topics (those are collected in vols. 1 and 2), and they are clearly not the foundation of Boulding's reputation as a highly competent economist. To appreciate his contributions to economics one must read his books, which are listed in the general bibliography at the end of volume 5.

The great variety of topics covered in these 1,600 pages makes it difficult to say much about any one subject. Hence I will sketch only the general theoretical framework, with a few illustrations from specific papers. There are four principal themes which reflect Boulding's abiding interest in human beings, significant social processes, and major social concerns. Each theme includes a set of concepts and propositions with which a great variety of events can be explained or at least illuminated.

Major Themes

Image.—The first and major theme is that of "the image" as a major determinant of individuals' activities and organizations' modes of operation. An "image is the cognitive structure or 'subjective knowledge' possessed by an organism or organization . . . the 'view of the universe' " which, while present in many living things, is most highly developed in man (4:87). The various components of an image are rank ordered on the basis of values, reflect a person's past experiences and future hopes, and vary among societies, subcultures, and individuals. Boulding postulates that "the behavior of organisms or organizations consists not in a mechanical stimulus-response pattern, but rather in a response to the image. A *decision* involves the selection of part of the field of the image representing the possible future which is highest valued (first in the ordering) of the dominant value ordering" (4:89). Thus, when we know a person's or firm's image of the future, including its value scales, we should be able to predict later activities.

The various components of an image may be inconsistent, but this does not matter. What is significant is the individual's perception of the world, not an observer's assessment of the image's components, logic, or validity. In fact, images often are self-validating in that they lead to actions whose effects further strengthen them.

The raw material of images is information, and the major process of their development and change is learning. When new information clarifies or modifies an image we should expect changes in subsequent behavior. This conceptualization leads Boulding to the study of communication, both on the abstract level (e.g., channel capacity, information overload) and in the real world of business and government.

I will give only one example of the way in which Boulding uses the concept of image—and blends it with sociological factors to provide some insight into perplexing phenomena. He assumes that "our present behavior is determined very much by our image of our own future, our image of what might be called the 'probable total life pattern'" (4:555). In earlier decades, when the age distribution of a population was pyramidal, increasing age brought higher prestige and power. Hence people's image of the future included an increasingly valuable self-image (assuming that they did not dwell on the fact that half of them would be dead by age 40). Today, with fewer babies being born and most people living longer, the age distribution is becoming more rectangular. As young people look ahead they are aware, perhaps only half-consciously, that there will be few opportunities for them to gain status with age because the power and prestige structures of organizations are still pyramidal. In fact, as they grow older, their value as persons (a composite of income, status, self-esteem, and respect) can be expected to decline. Under these circumstances, Boulding suggests, "we have a pathological youth culture, devoted to the sensate world and the pleasures of the moment in sex and drugs. The problem, furthermore, is an intractable one, for we are certainly not going back to the triangular age distribution. It is hard to construct rectangular hierarchical distributions. The idea of having just as many people at the top as there are at the bottom would be somewhat threatening to most organizations, and the whole concept of the 'span of control' implies a triangular hierarchy" (4: 557-58).

Boulding's conceptions of image, its nature, sources, and effects, make considerable intuitive sense and contribute greatly to a better understanding of various aspects of social life. Yet one cannot help but wish that there were more data, and more explicit links to the relevant work of psychologists. Even his book on images (Boulding 1956) is quite general, with few ties to empirical studies. If "image" is to be more than an amorphous descriptive concept with some heuristic value we must know more about its structure and operation, including the ways in which learning actually affects it, and its relation to such well-known phenomena as expectation must be clarified. Boulding relies on "image" in almost every article, yet a reader cannot help thinking about the methodological difficulties related

to its specification and operation, not to mention the wide variety of images Boulding postulates (e.g., "national images" in vol. 5, chap. 5).

Boulding speaks about people testing their images of the world (e.g., 5:240) and the ways in which some images lead to self-validating behavior (e.g., 4:95–99), but he says little if anything about how one would ascertain an individual's, organization's, and a society's images, or how one would study the determinants, structure, operation, and effects of such images. In these papers there is scant reference to the relevant work of psychologists on cognitive dissonance, to the relationships among experience, cognitive structures, behavior, and learning, or to sociologists concerned with the nature and dangers of self-fulfilling hypotheses. Perhaps the character of the audiences for which these papers were originally intended precluded such linkages, yet such ties are essential for Boulding's goal of achieving an integrated social science.

Systems.—The second theme revolves around the idea of "system." Boulding employs this concept in several ways and endows it with several meanings. On the one hand he uses the general systems perspective of social phenomena, along with its technical terms. Positive and negative feedback loops, information flow and channel capacity, entropy and dynamic equilibrium, for example, are among the concepts that provide readers with new insights into old problems (but not necessarily more practical solutions).

On the other hand, the word "system" is frequently used much more loosely, as a label for a bewildering variety of phenomena whose systemic characteristics (in the technical sense) are difficult to ascertain. But Boulding does not think that the usual distinctions among the components of social systems (e.g., economic, political), are as significant as five other types of systems (clearly in the nontechnical sense) which cut across the usual divisions (e.g., 4:153 ff.). The first of these is *population* systems, conceived rather broadly to include not only human beings but also capital goods, commodities, ideas, and images. Changes in the magnitudes and composition of these systems (or perhaps one should say aggregates) are viewed as the roots of social dynamics, such as the process of economic development. Second, there are *exchange* systems, the basic form of human interaction and indeed of social life. Here Boulding includes not only conventional economic relations but also such broader aspects as are studied by exchange theorists in sociology.

The "*threat* systems" are quite similar to exchange, differing mainly in what is exchanged: "An exchange system is based on a transfer of goods, a threat system on the transfer of 'bads.' . . . A threat system is based on the proposition, 'If you do not do something good for me I will do something bad to you.' Threat systems are the basis of politics as exchange systems are the basis of economics" (4:156–57). But since there is a tendency for similarly negative reactions among the threat targets, relations often deteriorate. "Threat systems, therefore, constantly decline into war systems or deterrence systems which seem to have an inherent instability in them. This is the main reason for the rise and fall of civilizations, and the long

cycles which have characterized human development for the last five thousand years" (4:157–58).

Fourth is the *learning* systems, considered as those social processes which lead to changes in individuals' and societies' images of the world. Social integration, affection, and altruism are the major components of the fifth system: "Love systems are those in which the individual comes to identify his own desires with those of another. These are important in the explanation of the institution of the family, of the church, and of nationalism, of the phenomena of philanthropy and self-sacrifice, and of all those areas of life where we do not merely exchange or threaten but in which we *identify*" (4:160). Later he called these the "*integration* systems."

Boulding points out that each of these five systems is subject to varying degrees of necessity, chance, and freedom, which affect its operation and possibilities of change. A system's knowledge of itself introduces a "human variable," which influences a system's future structure and operation and introduces a source of internal dynamics, particularly in social systems (e.g., vol. 3, chap. 29).

Boulding uses the three major systems (exchange, threat, and integration) in his descriptions of conflict and peace (vol. 5), and both the systems and their resulting types of power (e.g., exchange power) in volume 4 to discuss a variety of social phenomena. His discussion of ecosystems, finally, anticipates much of what concerns us today (e.g., 3:119, 299, 313, 478, 482).

Boulding believes that any understanding of social phenomena and processes, and indeed of any human event, depends on insights from all five systems. A focus on only one, for instance, by narrowly oriented economists or political scientists, will not lead to true understanding which requires at least that the other systems be part of one's peripheral vision. Hence Boulding urges sociologists to have a broad perspective (e.g., 4:161).

Dynamics.—Boulding's third theme, social change, draws heavily on his conceptions of "image" and "system." Several papers are devoted to the examination of change ranging from large-scale processes like economic development through alterations in organizations to modifications in individuals' activities.

On the level of individuals, behavior change is viewed primarily as the result of new or altered images, which in turn arise from new information or one's own novel experiences. Societal dynamics are conceived in terms of alterations in the systems discussed above. While one or another may predominate—as population systems do in economic development—all are involved to some extent. Generally speaking, "society moves and changes by an enormous interaction of images, behaviours, disappointments, role fulfilments or unfulfilments, and the constant interaction of images on society and society on the images" (4:158). Many of the papers, especially on "political economy," in volume 3, can be viewed as an examination of these interaction processes and their implications for social life.

Conflict and peace.—Conflicts among individuals, groups, and nations, arms races and their economic aspects, war, and peace with its implications

constitute the fourth major theme. Boulding considers peace not simply as the absence of war but as a phenomenon itself, worthy of and indeed requiring careful study. The theoretical framework is provided by the first two themes: war, peace, and international relations generally are viewed in terms of individuals' and societies' images of the world and of the future, which operate within the context of national systems dominated by threat power and inadequate exchange and integration.

The major problem of these papers is their high degree of abstraction. All too many discussions are about a disembodied, not to say ethereal world. For example, it is one thing to view conflict in terms of images or welfare functions (e.g., vol. 5, chaps. 17, 30) or to assess the economic costs of arms races and conflict (e.g., vol. 5, chaps. 1, 3). It is quite another to move from diagrams and hypothetical persons "Able" and "Baker" to social historical realities, to people with heart and brain, driven by passions, memories, hopes, and fears. For example, Boulding outlines five stages for preventing World War III. In the second, involving formal agreements, he says, "We could begin, perhaps, with an organization for the prevention of accidental war. This will be a joint organization of the major armed forces of the world. Once this has been accomplished, a major system change is under way" (5:116). A noble vision. But that little word "once" covers a large number of complex processes, and when Boulding blithely skips over the great obstacles to implementation the whole intellectual enterprise becomes suspect. Again, in his discussion of Gandhi's success in India, Boulding fails to consider the fact that England did not treat dissidents as Russia did and does. Throughout these papers Boulding is much less realistic and uses a less hard-nosed approach to data than he did in the other two volumes.

The noble vision detailed in volume 5 suffers from an acute empirical myopia; it is disconcerting that the papers in volumes 3 and 4 contain a wealth of illustrations and data from the real world, while the last volume contains far fewer ties to real events and human beings. Perhaps this is due to the larger and more complex issues under discussion, but one has a right to expect that relevant events of recent history should find a prominent place, even when they do not support the author's major theses. The peace papers in particular focus on a curiously ethereal and lopsided world, where we hear much about the U.S. State and Defense Departments and Vietnam, but where there is little if anything about the violence of the Berlin Wall, the guns of Hungary, Czechoslovakia, Korea, and Tibet, or the implications of the Gulag Archipelago. Even an abstract discussion of great ideas and noble visions should recognize or at least be applicable to actual events that have occurred, else it is in danger of being disregarded. Here it is interesting to consider what would happen if Boulding were to include the major conflict-related events of the last 30 years in his discussion of international relations, disarmament, and peace, if he included political realities and looked at the East as closely as he scrutinizes the West. The papers would then be more realistic—if indeed they retained their content and thrust.

In fairness we must recognize that international systems and peace are more difficult to analyze than smaller and less significant topics. Boulding's outline of required research in this area (e.g., vol. 5, chaps. 13, 32) is staggering indeed. And the fact that there are three schools of research—those who concentrate on broad issues, on narrow topics, and radicals (who war with the others)—cannot help but reduce one's confidence in future results.

Data and Verification

These volumes abound in provocative ideas, interesting hypotheses, and potentially fruitful concepts. However, most of the papers appear to have been prepared for nonprofessional audiences and are therefore necessarily superficial; their limited length, moreover, frequently restricts the depth in which the issues are explored and the amount of empirical evidence which is brought to support propositions. Many of the concepts lack specificity and empirical referents, many variables will be difficult to measure, and many of the hypotheses will be hard to verify or even test.

Consider the concepts "image" and "personal value" in the foregoing discussion of youth. Boulding's general idea here seems reasonable enough, but what happens when we ask: How valid is it? The specification of even the major variables involved, their measurement, and the testing of the several hypotheses which constitute the "general idea" are yet to be accomplished. Certainly it will not be an easy task. As one reads the relevant paragraphs (4:554–58), one might nod repeatedly and exclaim "Aha" more than once; but along with the feeling that one now has a better understanding of some of the problems of modern youth, there nevertheless arises a feeling of uneasiness because one cannot be sure that one's understanding reflects reality. Here, as elsewhere, others must step in to formulate and test specific hypotheses.

The problems of verification, and of the inadequacy of underlying data in general, arise time and again. The books give readers a host of "aha" experiences, and practically every paper provides new insights. But upon sober reflection one cannot help but ask: This is what Boulding thinks, but how can we test this idea—and is it true? It is quite disconcerting to realize that, were one to add the necessary specifics, measures, and data, the papers would lose much of the sense of adventure that now pervades them—and all of their charm. They would look—and read—like a scholarly journal, and while this would satisfy the intellect it might not please our sensibilities.

Furthermore, some crucial processes receive inadequate attention. According to Boulding, not only is violence learned, but organizations learn and images are modified through learning. This is intuitively true, but we are left in the dark as to just how the process operates and what factors influence it. The reader is told that learning occurs, but the papers do not indicate the complexity of the process or the complicated ways in which

past and present external events affect learning and its outcome. Similarly, to say that behavior is determined by images is true only in a superficial sense. As psychologists have shown, consequences affect future actions, and changes in consequences eventually lead to behavior change (e.g., see Bandura 1969). The introduction of a mediating factor—the image—does not contribute much to the analysis, except to make the description of events easier to understand and more vivid. Since image refers to the summation of one's own and others' experiences and other environmental events, why not study these in relation to behavior? If one wants to emphasize learning and behavior, sociological analysis can be done without reliance on such hypothetical constructs (e.g., Homans 1974; Kunkel 1975).

Toward a General Social Science

The subtitle of volume 4—"Toward a General Science"—describes one of Boulding's major aims. In many papers he succeeds admirably in drawing on concepts and propositions from several fields, in synthesizing diverse views, and in using the resulting insights for the analysis of various phenomena. But while such efforts are commendable and are basic to the success of Boulding's approach, too much is left unsaid.

Consider this statement: "We see the world the way we do, instead of in some possible alternative way, because in some sense it 'pays' us to do so. In the theory of signal detection, for instance, it has been shown that the chance of a person detecting a signal, failing to detect it, or giving a false alarm, depends on the 'payoffs' (rewards or punishments, or value orderings) associated with each contingency." This could have been written by one of several people, sociologists and psychologists alike, including Walter Buckley, George Homans, and even B. F. Skinner. But it is part of the economist Boulding's discussion of image (4:95), and part of the inescapable conclusion of these volumes, that the development of a "general social science" may be close at hand. He repeatedly states his belief that academic divisions are irrelevant, counterproductive, and a reflection of history rather than of reality. It is evident from these books that all we need do is look behind the words and concepts that presently keep the several disciplines apart. Boulding has done this (e.g., vol. 4, chap. 21), but too many connections remain implicit. Time and again I was struck by the similarities of statements in various papers to the work of anthropologists (e.g., George Foster's [1965] discussion of world view and Charles Erasmus's [1961] analysis of development programs), psychologists' work on the roles of past experience and future expectations—either with (Bandura 1969) or without mediating processes (e.g., Skinner 1953)—and sociologists' writings too numerous to mention. It is unfortunate that one has to read between the lines to see the linkages and similarities among the several disciplines.

I do not suggest or mean to imply, by any means, that Boulding relies on the work of others without proper acknowledgment. In fact, he fre-

quently pays tribute to writers in several disciplines, and the publication dates of many papers indicate that the author could not have known of the works of others whose ideas he shares. Rather, it is abundantly clear that the similarity of conceptualizations and analyses arises largely from the fact that various researchers are studying the same reality: human beings enmeshed in social systems. Perspectives and tools may differ, but the descriptions and conclusions are amazingly similar.

These volumes provide sociologists with a bountiful harvest of provocative ideas that are relevant for theory, analysis, and various practical concerns. When these books are placed in every university library—as they should be—students and their professors will no longer be able to say that they have trouble finding interesting and significant topics for research.

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Book Reviews

A Measure of Justice: An Empirical Study of Changes in the Californ Penal Code, 1955–1971. By Richard A. Berk, Harold Brackman, and Seln Lesser. New York: Academic Press, 1977. Pp. xix+312. $17.50.

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A Measure of Justice is an important, path-breaking analysis of change law. It makes major contributions to the study of law and politics at thr levels. First, and most generally, Richard Berk, Harold Brackman, aı Selma Lesser develop an approach to the study of legislative change th enables them to gauge precisely the impact of public opinion, lobbyin media coverage, and other factors on legislative change and to assess tl consequences of such change. Although Berk, Brackman, and Lesser app the approach to only one area of law, it should be possible to use tl approach to answer questions critical to those interested in democrat political change in all issue areas—questions about the responsiveness the government to public opinion, about the power of the press, about tl influence of lobbyists, etc.

Second, in order to carry out their analysis, the authors had to deal wi a problem which has proven quite intractable in past work on legislati change: measuring the critical variable, legislative change itself. The tecl nique they develop is a significant methodological advance over previousl published work.

Third, and most concretely, the book tells us a great deal about tl causes and consequences of changes in the California penal code froı 1955 to 1971.

Berk, Brackman, and Lesser begin by outlining their approach (chap. 1 and providing a long and detailed qualitative history of changes in th penal code (chap. 2). The historical chapter plays an important role i the book. The quantitative study of legislative change (except for expendi tures) is an undeveloped area, and the authors expect much of thei audience to be skeptical about their results. Consequently, they continuall compare the results from their rather innovative quantitative analysis wit notions derived from more traditional qualitative work, using each type o analysis as a check on the other in a way which clearly shows the reade the strengths and weaknesses of each approach.

Next, the authors describe the way they have developed to quantify legislative change (chap. 3). This part of their work is extremely im portant because the lack of a satisfactory way to measure legislative chang has hindered progress in the analysis of political change over time. Th

authors' approach is a variant of content analysis. They had expert coders estimate the magnitude of changes in the law from legislative session to legislative session along a large number of preestablished dimensions according to detailed instructions which are presented in an appendix. The chapter is a thought-provoking guide to the content analysis of complex legal materials.

After quantifying legislative change, the authors use their data to describe trends in the penal code affecting actors in the criminal justice system (defendants, policy, prosecutors, etc.; chap. 4) and trends affecting criminalization and the severity of penalties (considering crimes against persons, property, and the public interest, and crimes without victims in chap. 5). Next, they attempt to explain the changes in legislation by relating them to changes in the party balance in the legislature, partisanship, lobby influence, the legislative enactments of previous sessions, crime rates, crime coverage in newspapers, and public opinion (chaps. 6 and 7). Finally, they try to determine whether changes in the penal code affect crime statistics, public opinion about crime, and newspaper coverage (chap. 8). The data analysis is careful and judicious.

Among the findings are the following: the law enforcement and civil liberties lobbies were often opposed to each other, but, over the entire period, both gained significant victories as the resources available to both police and defendants increased (chap. 4). Both "liberals" and "conservatives" seemed committed to controlling undesirable behavior by criminalizing it (though they often disagreed on what conduct should be criminalized; chap. 5). Newspaper coverage of crime is seen as affecting the legislature, but public opinion is described as having little impact (chaps. 6 and 7). Laws increasing criminalization lead (perhaps not surprisingly) to an increase in crime statistics (chap. 8).

Many of the findings are interesting, some are unexpected, and some will be controversial. It is important to emphasize a crucial fact: whatever the specific findings might be, none of them could have been discovered without the development of an approach permitting the statistical time-series analysis of legislative change.

There are a number of potentially significant flaws in the book, however. The method of content analysis employed seems excessively subjective, and it detaches the measures of legislative change from the laws themselves, producing change scores with no concrete referent. The measures of several other variables are also problematic: the measure of lobbying activity seems unduly subjective, and the sole available measure of public opinion (gauging whether people thought crime was one of the most serious problems facing the country) is extremely general and does not imply any particular policy preferences. Finally, the work is weak theoretically (as work on policy change is in general), so the authors are left with a large number of findings they are at a loss to explain (see pp. 260, 204, 213, 263, among others).

The faults of the book, however, are largely ones associated with ambitious undertakings. Berk, Brackman, and Lesser have developed a very

promising way to analyze legislative change. They have presented their methods and results in a clear, straightforward manner which invites close scrutiny by others. I think they have pointed out a path worth pursuing.

The Federal Machine: Beginnings of Bureaucracy in Jacksonian America. By Matthew A. Crenson. Baltimore: Johns Hopkins Press, 1975. Pp. xii+186. $10.00.

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It has been too long assumed in sociology that the best way to test social theory is to study contemporary society—or more accurately a sample of its living members—through the survey. To be sure, many sociological problems can best be illuminated by this conventional approach. There are, however, many important questions that cannot be so answered. The long-standing interest in the origins and development of social institutions means that sociologists—or others—must develop methods that do not depend on the cooperation of live respondents. Some such methods already exist. For example, Phillippe Aries, the brilliant French social historian, devised ingenious measures for discovering the origins of childhood in a study that has become a classic and one which gave birth to a new specialty among historians, the history of the family.

Other central concerns in the sociological tradition will surely be appropriated as topics for historical study. Matthew A. Crenson, a political scientist, has taken one as the topic of an impressive study which examines another institutional process that has been at the heart of sociological analysis, the bureaucratic organization. Crenson asks what conditions led Americans to abandon nonbureaucratic administration and to adopt bureaucratic forms in the national government. He discovers the first major departure from the prebureaucratic condition under President Andrew Jackson who had, indeed, promised reform in government, but for whom that simply meant a return to the republican virtue and honesty of an earlier America. Crenson's study concentrates on the important episodes of organizational change and the factors which appeared to trigger them. He asserts that reorganization efforts of the Jacksonians were pervasive, that "hardly an agency escaped their attention," and that "almost every federal department was overhauled at least once" (p. 3).

That reorganization required an ability to deal with administrative arrangements in formal and abstract terms and to make explicit the resulting arrangements. Stated briefly, the administrative legacy of the Jacksonians was bureaucracy with the following characteristics: "What was informal became formal. Administrative jurisdictions and responsibilities were explicitly defined . . . and official duties were separated from private

activities. . . . Generalists became specialists . . . functions of government agencies were sifted out from one another and assigned to different internal divisions or offices. . . . Administrative hierarchies became more formal and elaborate. The division of agencies into specialized bureaus and offices created additional levels of authority" (p. 4). Personal supervision, supplemented by accounting and information systems, was designed to assure honesty and diligence.

In accounting for this revolution in organization, Crenson rules out the presence of a coherent theory of management because the Jacksonians had none. Nor did bureaucratization result from the fact that Jacksonians were from strata of society which had been excluded from the civil service: Old Hickory's department heads resembled the upper-class persons who had previously dominated the higher reaches of government. Nor did Jacksonians have a distinctive political philosophy; their uniqueness was not founded on political principles or a program of action but on an emotional bond between themselves and the masses.

For Crenson, the source of bureaucratization is to be found, not in Jackson's political or administrative philosophy, but in the nature of Jackson's America. By 1830 (and even before) social institutions that had once imposed right standards on Americans—the church, the legal profession, the business community—were no longer effective in doing so. In law, for example, a great increase in the number of lawyers so strained the profession that its associations could no longer discipline its practitioners. The frontier lawyer especially was left free to pursue "private gain without the hindrance of professional oversight and supervision" (p. 36). Similar expansions in business led to an entrepreneurial fever that had everybody speculating and willing to do almost anything to get rich. In terms reminiscent of Robert Merton's conception of anomie, Crenson writes: "Any means for taking hold of opportunity was acceptable—or so it seemed" (p. 37).

Jackson's top appointees were aware of the absence of standards in American society. It is Crenson's contention that they realized that this condition would require new mechanisms to ensure compliance with government directives from both the civil servants who matured under these anomic conditions and the grasping citizens with whom they would have to deal. At first, however, the department heads continued the administrative style which they had inherited, one which was general in America and which Crenson calls the "individual entrepreneur organization" in which the personal character of the owner determined the conduct of the firm. That administration was based on the premise that good men made good organizations. In the beginning, the reputation of Jackson's government depended in large part on the bureau head's regard for his reputation rather than on that form of rational behavior which Weber called bureaucracy.

It soon became evident, however, that the personal administrative style which had, in a simpler and purer America, guaranteed efficiency and honesty in government, could no longer do so in a society whose regulatory

institutions had weakened. A number of sensational scandals in Jackson's first administration convinced officials that a thoroughgoing reorganization of their agencies was required to restore the efficiency and honesty that had been felt to characterize an earlier America.

The beginning of the bureaucratic form in Jackson's administration, according to Crenson, was the spoils system under which "individuals could be placed or replaced without upsetting the integrity of the whole" (p. 56). As an instrument of reform, rotation in office increased efficiency by ignoring "pre-existing social criteria like 'character' and 'respectability' and defining office impersonally, entirely by rules and regulations" (p. 56). While this is certainly an inventive interpretation of the function of the spoils system—one which Crenson attributes to Lynn Marshall—no evidence is provided to support the contention that Jackson did in fact ignore character and respectability in making appointments. It is plausible, however, that one of the unintended consequences of the spoils system was that it contributed to the impersonality of federal administration by depriving placemen of tenure in office.

The main part of the analysis, case studies of the administrative history of the post office and the land office, the fastest-growing agencies of the Jackson age, is far more convincing and compelling. Crenson ably reconstructs the history of each agency's organization, the number of employees and the tasks assigned to each, and how these changed as surveillance, formal rules, and careful record keeping were introduced. In Amos Kendall's post office, for example, "the administrative separation of powers doctrine . . . was extended to the country at large in a grand system of bureaucratic checks and balances . . ." (p. 109). What a contrast this was to the administration of the mails in John Quincy Adams's term, during which official transactions had been recorded on scraps of paper or not recorded at all.

Crenson also uncovered a number of plans to reorganize the land office and shows how difficult it was to create an "organic system" in the face of congressional resistance. Thus the administrative changes that took place were modest, their beginnings were evident in previous administrations, and the tradition of personal organization did not completely die out under Jackson since the department heads, whose own functions remained unspecified, still exerted the force of personality upon their subordinates.

In a final substantive chapter, Crenson raises some questions about the relationship between the private and public spheres of action. In order to speed the delivery of the mails, Kendall hoped to use the railroads. When it appeared, however, that their operators were eager to receive the same kind of government largess that the old corrupt contractors had received, Kendall demurred. On the other hand, he willingly provided western newspaper editors a subsidy in the form of free carriage of news exchange slips, the forerunner of the wire services. While Crenson sees in this the courting of political favor, he also offers the interesting possibility that Kendall hoped the government "could draw from local communities the kind of regulatory power which had once been provided by institutions

like the legal profession or the business community" (p. 149). Thus the public-private relationship which increased government morality would be encouraged but not one that reached into the government till.

Crenson's contributions to the theory and history of administration are substantial. For one thing, he is one of the first to locate the origins of bureaucracy among the Jacksonians. Even so brilliant a scholar as Leonard White did not discern a break with the past in Jackson's bureaucracy, and what changes he saw he attributed largely to the increase in the size and scope of government. Another contribution is the notion that bigness alone did not produce bureaucracy but that formal rules and careful auditing and bookkeeping procedures were designed principally to guarantee the good behavior of civil servants. Also, the relationship between the absence of morality among civil servants and the deterioration of standards in law and in business is intriguing. From the methodological point of view, Crenson has shown sociologists how to measure origins and functions in an institutional area that has been especially important to them.

I have very little fault to find with this work. Given the importance to his thesis of the breakdown in morality in America during the 1820s and 1830s, it would have been more convincing if Crenson had clearly demonstrated that the law, the church, and the business community were more corrupt then than previously. It may be unreasonable for me to raise that question since to answer it requires a number of careful studies and Crenson had his hands full in the work of tracing the changes in the administration of a number of government bureaus. This criticism should not, however, detract from what is bound to be the main source of our insights as to the origins of bureaucracy in the federal government.

Radical Protest and Social Structure: The Southern Farmers' Alliance and Cotton Tenancy, 1880–1890. By Michael Schwartz. New York: Academic Press, 1976. Pp. xi+302. $17.50.

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In this monograph, Michael Schwartz provides an interpretation of the Southern Farmers' Alliance which successfully argues that the activities of those involved in protest groups are fundamentally rational. In opposition to much of the current theoretical literature on collective behavior, Schwartz maintains that the failures of protest movements are much more likely to be accounted for by internal oligarchies and lack of adequate information than through investigations of irrational or emotional group behavior.

Schwartz begins his analysis with a relatively straightforward exposition of how the one-crop cotton tenancy system in the American South provided a social basis for the emergence of the Southern Farmers' Alliance. He first

outlines basic tenancy relationships, then places them in the context continuing landlord-merchant struggles for control of agricultural profi The characterization of this economic system is very thoroughly done, w particularly good analysis of the stakes involved in intra- and intergro conflict. One difficulty in this section is the relative weakness—or perha simply the lack—of the early quantitative data employed to substantia claims regarding developmental trends in the system.

In addition, questions of racial stratification do not receive adequa attention. Schwartz argues that the operations of the ". . . tenancy syst would have maintained and amplified racial stratification" (p. 10) in t absence of racial discrimination, but does not incorporate systematical in his analysis the "intense racial prejudice" (p. 10) of the period. Lat discussion of relatively positive alliance orientations toward black membe ship (p. 101) appears to underplay the discriminatory aspects of allian behavior, and the subsequent but undeveloped utilization of complex co cepts like "superexploitation" and "immiseration" (p. 285) in the concl sion does little to remedy this difficulty.

The ensuing description of the organizational structure of the alliance ar the subsequent theoretical discussion of the relevance of major collectiv behavior theories for understanding it constitute the core of the study. I these sections, Schwartz analyzes the two major impediments to allianc success—internal oligarchy and lack of adequate information among th membership—and demonstrates that, in the light of these difficulties, actu membership involvement and activity were quite rational.

Schwartz bases his analysis of alliance oligarchy in part on Michels' work but moves well beyond it through his demonstration of the input tha the existing class structure had in promoting the emergence of oligarch within the alliance. He first shows that at least two-thirds of the leader were recruited from social elites in terms of education, wealth, or previou political officeholding. He argues convincingly that, given social interest which diverged from those of the alliance membership, the leadershij operated as an oligarchy which blunted the more radical aims of the general membership. The leadership first attempted to deflect radical economic proposals from the rank and file into more diffuse attacks on commercial interests external to the agricultural system, and then launched electoral activities which absorbed resources previously employed in the economic conflict.

The discussion of information breakdowns and failures within the alliance in terms of "structured ignorance" (p. 150) is the most innovative aspect of this study. In Schwartz's view, political actors are not thinking or behaving irrationally when they engage in disruptive activities which fail, but instead are rational individuals operating under conditions in which relevant information often does not exist, or, if it does, is not readily available to them. Further, ignorance exhibited by participants in social movements is not a random phenomenon. It is structured by the social

order in which they live. "Social systems function to obscure and distort the sources of problems and to make individuals with similar grievances see them as different" (p. 142). Under these circumstances, it is possible for the widely differing experiences of cotton farmers to stimulate many different solutions which could be plausibly defended.

Schwartz's treatment of the problem of rationality in protest movements is, on the whole, quite convincing. One reservation which remains concerns the range of his theory. The most persuasive analyses which focus on the irrational dimensions of protest activity have been developed in response to 20th-century totalitarian systems, particularly fascist ones, and it is unclear whether Schwartz's scheme can deal with such phenomena in a completely satisfactory fashion. Also, in terms of style, the presentation occasionally lapses into the abstract formalism of organization theory. Questionable distinctions between state power as "institutional power" and other power as "noninstitutional power" (p. 130), or propositions like *"any system contains within itself the possibility of a power strong enough to alter it"* (p. 173, emphasis his) contribute little to our understanding.

In the third section of the monograph, which deals with patterns of conflict, Schwartz suggests that the alliance initially operated with two different tactical orientations, alternatively pressuring the system and developing counterinstitutions. The basic process which linked the varying tactics was that of escalation. When the alliance assault on the prevailing supply system succeeded, pressure was transmitted upward in the system, and merchants and manufacturers escalated the conflict by boycotting cooperatives, or individuals involved in trade agreements.

The process of conflict and escalation culminated in the formation of alliance exchanges, organizations designed to circumvent the market system by handling all market transactions for its members. These statewide counterinstitutions, "the most ambitious . . . ever undertaken by an American protest movement . . ." (p. 217), had a devastating short-run effect on existing business arrangements but failed because they could not cope with the subsequent escalation in which sources of credit were cut off. Throughout this analysis, Schwartz displays exceptional facility in linking structural considerations to associated social processes. In particular, he employs materials gathered at a variety of levels, ranging from activities in a specific county in North Carolina to an account of the jute boycott, to elaborate his basic themes of internal oligarchy and structured ignorance as major reasons for alliance failures.

In sum, Schwartz has made a major contribution to the collective-behavior literature. He has provided an excellent critique of the dominant approaches in the field, and, through his analysis of the alliance, done much to create an alternative theoretical perspective. In addition, his ability to link his primary concerns to related problems, such as rural stratification systems and political conflict, makes the work deserve a wider audience.

Networks of Collective Action: A Perspective on Community Influence Systems. By Edward O. Laumann and Franz U. Pappi. New York: Academic Press, 1976. Pp. xx+329. $18.00.

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Networks of Collective Action sets itself an ambitious task: to develop theoretical and research strategies useful "not only in studying community decision making" but also in answering "the central integrative question: How does a social system as a whole establish priorities among competing sets of ends (or goals) requiring the expenditure of its scarce resources (means) given the existence of competing standards for evaluating these alternatives among its component actors?" (p. 1) Studying community decision making in a small German town (of 20,000, here named "Altneustadt") was simply the particular empirical choice the authors made of a way to investigate this basic question. Why this somewhat atypical small-town setting? It is "a case of the drosophila fruit-fly" (p. 15)—that is, a unit of analysis sufficiently manageable to allow the testing of new methods and the refinement of hypotheses, but not one of great intrinsic interest.

Given the large ambition and the narrow data base, Laumann and Pappi succeed remarkably well in a study which will be an indispensable point of reference for future analyses of community decision making and of their "central integrative question." What is most intriguing is not their specific results but rather, as they intended, their presentation and critical discussion of comparatively new methods, procedures, and hypotheses. The 268 pages of text are jam-packed with multidimensional scaling analyses and complex sociometric techniques. The first effect is to dizzy the reader; the more lasting impact is a much clearer idea about the strengths and limitations of these approaches to the data.

Unlike traditional students of community power, Laumann and Pappi give equal billing to three sides of the matter: the social structure of the community, the social structure of the elite, and the structure of the "interface" between the two. The book is organized as a sequence of analyses of these three aspects. After an introductory chapter, part 1 makes use of a random sample of the town's eligible-voter population. Each respondent was asked the occupations of his three best friends and a series of value questions. Using smallest-space analysis, the authors investigate the "proximities" of various occupational groups as indexed by similarity of occupational friendship choices made by group members. They then use hierarchical clustering to classify together sets of occupations which are relatively close by this measure. Similiar methods show that proximity in the "value space" is closely related to that in the "social space."

Part 2 identifies the "elite" as the incumbents of the "highest positions of authority in organized collectivities whose primary functional responsibilities are in one of the four [Parsonian] functionally specialized institutional subsectors at the community level of analysis" (p. 96). Reputational

methods were used to add elite members who did not hold such positions. Of 51 elite members so designated, 46 were interviewed and named their three closest or most frequent business, social, and community-affairs associates; they also rank ordered one another on level of influence and indicated the kinds of influence resources used by each elite member. Clique-finding methods indicated the presence of two main cliques: a large Christian Democratic party–based clique and a smaller Socialist party–connected clique. The distance of elite members from one another is displayed by putting minimum network path distances between members of each pair into a smallest-space analysis; two-dimensional solutions are produced for each of the three types of tie. It is asserted that centrality in the smallest space diagrams should indicate level of influence, but reputed influence explains only between 3% and 16% of the variance in distance from the centroid (p. 142). The diagrams would nevertheless be of considerable heuristic value if the individual elite members were identifiable; but no key identifies them uniquely for the reader. In a chapter prepared with Lois Verbrugge, interpoint distances in the elite smallest-space analysis of the community-affairs network are made the dependent variable in a causal model; the main explanatory variable turns out to be distances in the social relations network. Five issues are chosen for decision-making analysis, of which two are hypothetical. Elite members are designated as proponents or opponents on these five by means of the judgment of other elite members.

In part 3 Laumann and Pappi describe the interaction or "interface" between elite and public. They develop a typology of eight bases of elite influence, or "resources." Each member of the elite was asked to indicate which resource each other one "primarily" controlled. The voter sample was asked to say which of the 27 most influential elite members they would call on for various purposes, or which would most likely influence their opinion. The choices of elite members by members of various groups were then put into a smallest-space analysis to display in one three-dimensional space the proximities of groups to particular elite members. Multiple-regression equations analyze, for each group, what explains which elite members they "approach." One of the most interesting arguments in the book is that to the extent groups approach elite members who are actually out of tune with their value or interest positions, or distant socially from them, this alliance is vulnerable and a source of future strain—a "fault line" in the political system (pp. 234–43).

The substantial achievements of this volume are accompanied by some problems. The use of Parsonian theory throughout, even if only as a "sensitizing framework" (p. 2), generates turgid prose and excessive attention to unilluminating issues and hypotheses. For example, the statement that people may be divided by both interests and values becomes the announcement that "large-scale systems are usually differentiated around at least two axes or dimensions of potential cleavage and/or social differentiation. The first, which we shall call the *adaptive axis*, refers to the extent and character of the division of labor. . . . The second, which we shall call

the *pattern-maintenance axis*, refers to the differentiation of the population into subgroups holding distinctive social values . . ." (p. 4). The reader must wade through discussions of which of the four Parsonian sectors (A-G-I-L) various actors and organizations should be coded into; these coding decisions then lead to tendentious assertions—for instance, that the SPD (Socialist) clique is "very narrowly recruited from leadership elements in only one institutional sector, pattern-maintenance, including religion, education and science" (p. 116). The hypothesis that "various resources should be differentially distributed among the institutional sectors according to their functional relevance for each sector" turns out to be verified by such findings as these: money and credit are controlled mainly by bankers and small businessmen; land and jobs by professionals, managers, and owners of large businesses and farms (p. 200).

Methodological problems stem mainly from reifying the results of the multidimensional scalings rather than treating them as heuristically useful geometric representations of rank orderings. The relative arbitrariness of smallest-space pictures is indicated by the occasional need to distort or oversimplify the data in order to achieve an acceptable solution. Thus, in analyzing the proximities of occupational groups, students had to be eliminated because their level of self-selection was so high that it distorted the solution (p. 63); to construct a proximity space for population groups and elite members together, the level of a group's choice of particular elite members had to be dichotomized to "1" if above average or "0" if below, discarding valuable information (p. 244). Proximity pictures can be made even when groups differ very little from one another. Thus, even though most variance in value orientations (74%–91%) was within rather than between occupational groups, a multidimensional scalogram routine produced a good two-dimensional fit (p. 81). One may suspect, as Laumann and Pappi do explicitly, that "our analytic procedures may be especially conducive to finding an order or structure when there may, in fact, not be one at all or when it might be of quite a different sort. Given sufficient ingenuity and massaging of the data, we can almost guarantee that these procedures will produce a structural representation of a data matrix—regardless of the meaningfulness or justifiability of using the original data set to provide even ordinal estimates of the proximities among a set of points" (p. 252). Despite this salutary note of caution, the authors occasionally slip into treating the spatial representations as ultimate reality. For example, in chapter 8, where a causal model explicates the elite's community-affairs network, the dependent variable used is not the actual network path distance among pairs of elite members—a quantity with a straightforward interpretation—but the interpoint distances derived by putting these path distances into a smallest-space analysis—a derivative quantity, with no direct sociometric interpretation.

There are also flaws in the sociometric analyses. All the sociometric data are derived from the forced-choice technique of asking respondents to name their *three* best or most frequent contacts. There are two main objections to this. One is that forced-choice questions result in serious distortions of

the actual network situation, since people are likely to be naming either more or fewer people than they actually think appropriate. One account of the distortions introduced is given by Paul Holland and Samuel Leinhardt in "The Structural Implications of Measurement Error in Sociometry" (*Journal of Mathematical Sociology* 3 [1973]: 85–111). A more substantive objection is that, as I argued in some detail in "The Strength of Weak Ties" (*American Journal of Sociology* 78 [May 1973]: 1360–80), weak interpersonal ties may play crucial roles in instrumental and political contexts but are missed by "three-best-friend" questions. This consideration is particularly important since Laumann and Pappi make frequent arguments about connectedness and reachability. They assert, for example, that the SPD clique is vulnerable because the removal of one liaison person would disconnect its two segments (p. 118), and that it is "striking" that there is no causal path from distance in the business-professional network to that in the community-affairs network (p. 159). But both findings may be artifacts of a procedure which neglects all but strong ties.

It also seems to me a mistake to symmetrize interpersonal ties as the authors do in several analyses—discarding the information that A chose B but B did not choose A. Several writers have argued convincingly that asymmetry of choice is a good indicator of hierarchy in a relationship, the chooser occupying a lower position (see, e.g., James Davis and Samuel Leinhardt, "The Structure of Positive Interpersonal Relations in Small Groups," in *Sociological Theories in Progress*, ed. J. Berger [Boston: Houghton-Mifflin, 1972]).

The authors fail to integrate their three separate sociometric analyses of elite members, treating them separately and sometimes inconsistently. Chapter 6, on the elite's coalition structure, is based wholly on the network of informal social relations, since these "would be especially likely to reveal informally based alliances and cliques within the elite" (p. 102). But chapter 8, a causal model of the community's "influence structure," takes as its main dependent variable the network based on discussion of community affairs. Laumann and Pappi do point out that sociometric methods for handling several different types of social relations simultaneously, mainly "blockmodeling," were "developed . . . only after the bulk of our data analysis . . . was complete" (p. 22) (re "blockmodeling," see Harrison C. White, Scott A. Boorman, and Ronald L. Breiger, "Social Structure from Multiple Networks. I," *American Journal of Sociology* 81 [January 1976]: 730–80).

The most serious overall problem is the inability of the data, gathered at one point in time, to speak to the question of how issues and decisions develop—the traditional focus of community power studies. The authors admit that "the least satisfactory aspect of our work . . . has been the relative neglect of process and structural transformations of systems over time. . . . Only data gathered on the same system at different points in time can begin to resolve this question satisfactorily" (p. 268). But a series of snapshots of the kind provided by the present data might not in itself

be sufficient; some reorientation in the kind of information sought would be needed.

There seems to me, for example, far too little ethnographic reporting of the historical development of the issues whose resolutions are treated. Even from interviews at one point in time one could have elicited fairly detailed accounts of how issues came to be posed and by what process they came to decision. The neglect of how issues are posed leaves the authors open to the objection that much of community power lies in control over what is perceived as the agenda for decision rather than in decision itself, an objection raised especially against "pluralist" writers but relevant to the present discussion. Nor do the authors explain in detail how they chose the three actual issues treated except to say that they wanted to distribute issues across the sectors of the A-G-I-L paradigm.

The sociometric question asked could be focused more sharply in order to shed more light on political processes. Instead of asking elite members to whom they most often talk about community affairs, it might make more sense to ask them with whom they had actually discussed the issues which were to be decided. Instead of asking voters which elite members they "would" approach under various circumstances, more behavioral questions —about which elite members they actually have approached in the past or interacted with—would give a more realistic picture of "proximity" between population groups and elites. This procedure would avoid such peculiar results as a high "influence rank" for students, because they "happened to select disproportionately high-status Altbürger influentials" whom they probably could not actually mobilize (p. 248). I would have liked, in short, more of the blow-by-blow account of the traditional power studies: who said what and did what to whom, why, and with what result? With more behaviorally based closed-ended questions, such considerations would not necessarily be divorced from the sophisticated methods used here, but could be integrated with them in a more satisfying analysis. In combination with more open-ended questioning and data collection over time, such changes would help realize the potential displayed in the present study.

Friends of Friends: Networks, Manipulators and Coalitions. By Jeremy Boissevain. New York: St. Martin's Press, 1974. Pp. xv+285. $16.95.

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This is an uneven book. It contains an outline of a typology of coalitions and of social entrepreneurs, a reinterpretation of several social situations well-known from the literature of social science, numerous generalizations about social life, and an account of social network analysis. The merits and demerits of the book vary from part to part.

The best sections are those in which the author sets out a scheme for describing different kinds of coalition, "a temporary alliance of distinct parties for a limited purpose" (p. 171). Differentiating these in terms of internal structure, mode of recruitment, content of interaction, objectives and type of external environment, Boissevain distinguishes gangs, action sets, factions, and, somewhat idiosyncratically, cliques. The social activists or entrepreneurs who, along with their followers and clients, make up coalitions are defined as patrons and brokers. The author discusses what sorts of people become entrepreneurs and how coalitions change from one type to another, and how they are metamorphosed into enduring corporate groups. These social processes are illustrated from his own inquiries in Malta and Sicily and his experience in the U.S. Army, and by glossing William Foote Whyte's "The Nortons," Bruce Kapferer's Cell Room dispute from Mitchell's symposium *Social Networks*, and other accounts of the activities of coalitions by sociologists and social anthropologists. This analytical scheme is clear and useful, and the examples are straightforward and often insightful. The purely corporate model of society (applied deductively in social analysis) against which Boissevain directs sustained criticism throughout the book may be largely a straw target, but nevertheless his own partial model, highlighting the significance of dyadic relations and indirect contacts, is an essential complement to a model based on corporate groups. But I am baffled about the analytic status of the assertion that "in contrast to the clique, . . . the activities of the members of a gang more often take place out of doors" (p. 181). While rejecting culture as an explanation for social behavior (p. 121), Boissevain stresses the causal effects of climate (pp. 74–77); but the causal nexus between gangs and fresh air eludes me.

His exposition of social network analysis is less successful. Two of the three densities and one centrality index in diagram 2.6 (p. 39) are wrong, as are two indices in diagram 2.7 (p. 41). Diagram 7.1 (p. 176) looks like a two-dimensional network diagram but in fact seems to be a one-dimensional social distance scale. In chapter 5, in which the concepts of degree and density are used to analyze the first-order zones of two men in Malta, there are inconsistencies between tables. Pietru ignores 349 of his fellow villagers according to table 5.7 (41.2% of 846) but 370 of them according to table 5.8 (67% of 552) (pp. 114 and 116). There are similar inconsistencies in the number of villagers he engages in conversation and visits. These inconsistencies have been repeated uncorrected or unexplained from Boissevain's contribution to the symposium entitled *Network Analysis* which he edited with J. Clyde Mitchell (The Hague: Mouton, 1973). Since Boissevain makes only imprecise use of the numerous statistics he provides, the reader can ignore almost all of them without loss to the argument, particularly since some of them are based on calculations involving an unspecified number of persons "for whom no data are available" (p. 122).

Scattered throughout the book are many sweeping generalizations unsupported by any evidence. For example, according to Boissevain the individual had been banished from sociological analysis since Durkheim until this

book appeared (p. 9); the simultaneous rise of fascism and structural-functionalism was a reaction to the shift in the balance of power away from the industrial, agrarian, bureaucratic, and military bourgeoisie in favor of the workers, native peoples, and other subjugated persons (p. 13), structural-functionalism being a scientific belief system which was most congenial to colonialism (p. 19); the number of bachelors who have been successful in politics is striking (p. 157); the personality of sexually attractive women differs from that of unattractive women (p. 69); and so on. Some of these unsupported propositions are in principle testable and are also worth testing; for example, the idea that coalitions characterize southern Europe and corporate groups northern and western Europe because security cannot be guaranteed by the community at large in the former area but can in the latter (pp. 170, 203).

If we ignore the *obiter dicta*, the main weakness of the book seems to me to be the notion of politics used by Boissevain. He asserts that politics is a zero-sum game (p. 165), and most of the entrepreneurs in his illustrations seem to agree with him. But although he rightly emphasizes the structural asymmetry of competing coalitions, in these illustrations he overrates the extent to which rivals disagree about the rules of the game. Boissevain seems to envisage not the possibility of radical conflict and irreversible change, but merely the replacement of one dominant coalition by another and by the transformation of coalitions into groups. The "seeds of change" that "are present in all societies" (p. 231) appear to have been well domesticated even before they start to germinate.

University Communication Networks: The Small World Method. By R. Lance Shotland. New York: John Wiley & Sons, 1976. Pp. x+179. $6.95.

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University Communication Networks has an interesting empirical core—a careful and well-presented application of Milgram's small-world method to communications among university students, faculty, and administrators—but the book as a whole lacks a clear focus because the author seems confused about what he is trying to do.

Milgram originally developed his small-world method in order to study connections among people in large-scale social systems. The method was designed partly in response to a common experience: the exclamation of amazement at what a "small world" we live in when we discover that someone we meet in Hong Kong knows one's college roommate, or that a new acquaintance is related to an old friend. Milgram wanted to know how many intermediaries would be needed to connect any two people in a large-scale social system, like the United States, and whether the chains of acquaintances would form discernible patterns. He and his collaborators

sought answers essentially by finding a cooperative "target person" and a number of subjects who would try to reach the target through someone who knew him on a first-name (or some other) basis. If the subject knew the target, he or she could mail the packet provided by Milgram to him directly; if not he was instructed to mail the packet to someone known on a first-name basis who would have a greater chance of knowing the target. Results concerning the number of intermediaries required to connect people and the patterns of connections were fascinating, but Milgram's work has not, for the most part, been followed up.

In chapters 4–6 of *University Communication Networks* Lance Shotland replicates Milgram's method in order to study patterns of communication among students, faculty, and administrators at Michigan State University. His application of the method is well explained, straightforward, and careful; he documents his findings thoroughly and clearly; and he uses some imagination in treating possible problems concerning the method. The findings themselves—about the isolation of students on a large campus, about the central position of administrators in communications networks, about the role of rank and department boundaries in influencing patterns of contact—are not very surprising but well worth documenting.

Unfortunately, the book is seriously flawed. Probably the most serious problem is that the author never makes his purpose clear. He seems to have been motivated initially by a concern with the student unrest of the late 1960s and early 1970s and an intuitive belief that the unrest was rooted in student feelings of isolation and powerlessness (chap. 1). After trying to provide some evidence to support this belief by examining the literature by and about the participants in several student movements, he reports the results of a number of classic social psychological experiments showing individual satisfaction in groups to be related to one's position in group communication patterns. While there may be some sort of vague relationship between the literature drawn on and the results Shotland finds, it is not clear what the relationship is. He is not finding that isolates are rebellious, because he is not directly studying rebelliousness. He is not merely concerned with isolation or communications patterns, because he begins the book with long discussions on other topics. I couldn't quite figure out what the point of the whole enterprise was.

There are other problems. The review of work on the college environment (chap. 2) could be used as an example of how not to write a review of literature. It is essentially a narrative review of past studies, summarized one after the other. Shotland develops no particular point of view or interesting synthesis, and the chapter is boring. A three-page chapter telling the reader that a university can be viewed as a stratified social system (chap. 3) is pointless. Finally, Shotland's thinking about a critical point is extremely fuzzy: he confuses communications with power. He frequently seems to take the naive attitude that if only everyone in the university community talked to everyone else problems would disappear. He does not seriously entertain the possibility that interests may differ and that no amount of communication will prevent the powerful from getting what

they want disproportionately often, frequently to the dismay of others. Students may rebel because they have little or no power, and want some; studying communications patterns in a university alone will not provide answers to question about power.

The book is best seen as a report on the results of an experiment; the reader must provide the theoretical context.

School Politics Chicago Style. By Paul Peterson. Chicago: University of Chicago Press, 1976. Pp. xvi+304. $15.00.

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This is an exceedingly interesting and useful volume for those interested in a comprehensive study of policymaking in urban school systems. Peterson considers policy formation from both bargaining and unitary (organizational) perspectives. His bargaining model is distinguished conceptually into "pluralist" and ideological types. *Pluralist bargaining,* according to Peterson, "occurs when participants are primarily concerned about preserving or enhancing their immediate electoral or organizational interests" (p. xi). *Ideological bargaining* "occurs when participants are motivated by broader, more diffuse interests, such as those of a racial or class faction or of a political regime which are regarded as of such an enduring significance that the participants become deeply, ideologicaly, committed to them" (p. xii).

The core of the book is the author's attempt to use the bargaining and organizational models to explain the outcomes of three important issues faced by the Chicago school system—desegregation, collective bargaining, and decentralization. And it is here that the author is at his best. The case studies describe how the Daley machine attempted to deal with reform opposition, teachers' demands for collective bargaining, and the demands of blacks for integration and social equity. He uses the pluralist model to explain Daley's strategies and the ideological model to describe and analyze bargaining within the school board.

The utility of all three models in explaining or predicting outcomes is questionable. As the author himself points out, "In the end the relative significance of the three models depends on the observer's opinion concerning what outcomes were most important" (p. 242). One of the major problems here is the distinction made between pluralist and ideological bargaining models. One could well argue that the ideological model is not so much an alternative to the pluralist model as an extension of it. Indeed, if the machine politicians follow the institutional bargaining process as the author suggests (p. 5), bargaining takes place "among brothers," not among those not in the institutional order (p. 18). Using this analysis, bargaining outside the fraternity occurs only when the demands made by outsiders

reach unacceptable levels. Ideological bargaining in its earlier stages, then, is likely to focus on the use of "voice" to elevate the conflict; and if the demands are not satisfactorily met there will be a threat to "exit." In other words, unacceptable levels of confrontation and conflict must ensue before compromise is possible. Other problems include too much repetition in the case materials and some incongruent data totals in tables 8 and 14.

The policy prescriptions offered by the author in his concluding chapter should be of considerable interest to students of urban politics. Peterson points out how external forces constrain alternative choices of local policy-makers. Yet he adds in a curious note: ". . . central cities must struggle as best they can to entice both industry and desirable labor into their political jurisdiction. To do this, they must provide quality amenities uncontaminated by the presence of undesirable groups, pursue fiscally responsible public policies, and maintain social order" (p. 251). The reader can draw his own conclusion about what the author means here.

On balance, however, this book is worth reading because it contributes to our understanding about the utility of using decision-making models to explain and predict urban outcomes. Case studies might be used by others using different analytic conceptions to check the utility of these ideas. In the final analysis, the most important feature of this book is likely to be the understanding gained about the political process within Chicago—perhaps the single most influential urban political structure in the United States.

Power and Control: Social Structures and Their Transformations. Edited by Tom R. Burns and Walter Buckley. Sage Studies in International Sociology, vol. 6. Beverly Hills, Calif.: Sage Publications, 1976. Pp. 290. $12.00 (cloth); $6.00 (paper).

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This book of essays has all the flaws of the field it is surveying, and as an ideal type should become required reading among students of "structural analysis." Regrettably it has too few of the redeeming qualities of that field, and so it is unlikely to make any new converts. I shall start with a discussion of the common flaws of this type of analysis and this book and then discuss some of the bright spots.

What I am calling structural analysis stands at the meeting place of network theory, systems analysis, and Marxism. *Power and Control* devotes a section to each of these three diverse but complementary styles of analysis. Advocates of all three share a love of macrosociological modeling; a low estimate of the individual as against the aggregate, and hence relatively little interest in values, sentiments, or other micro-level traits; and a sense that social systems are *sui generis*, complete with their own laws,

equilibrating processes, emergent properties, and coercive impact upo individuals. Individual action does not so much make up the system a express it; conscious reflection upon actions taken is little more tha rationalization.

Because of their different traditions, these three approaches place dif ferent emphases upon their common elements. They sometimes give simila concepts different names, and they have different concerns in addition t the common ones. For example, Marxist theory has a humanistic side albeit not a side examined in this collection of essays. Empirical work b network theorists often includes an ethnographic detailing of microstruc tures, savoring the peculiar, indirect, and unanticipated outcomes of socia processes. By contrast, systems theory is given to ambitious comparison between human and nonhuman systems, plundering the realm of universa knowledge for analogues that will promote sociological understanding.

Each tradition is attractive and has immense potential as long as th operating strategies can be thought of as metaphorical. But practitioner in these schools often mistake metaphor for science, and the result is a dehumanized science fiction in which the means of analysis—the devising of assumptions, concepts, methods, and indicators—become ends in them selves. Such misanthropic fantasizing exhorts more than informs and creates more jargon than is needed for the empirical analysis that finally gets done. Like the worst in this field, this book is full of ritualistic model making, overgeneralization, and pseudo-scientific verbosity.

Happily there are a few valuable papers for which this book ought to be read. The outstanding example is by Frank Parkin, reprinted from an earlier publication. Whether or not Parkin's conclusions about power struggles in the Soviet bloc are correct—and a subsequent paper by Russell Hardin calls Parkin to account on a number of issues—his analysis is forceful, fluent in the logic of Marxist analysis, and gracefully written. Indeed the "Marxist section," comprising three papers examining system stability under different economic orders, has the greatest coherence of any part of the book.

The section on network theory contains an eccentric but interesting discussion of network thinking by Bo Anderson and Manuel L. Carlos. Unfortunately this summary confines itself mainly to the anthropological approach, which has been treated better elsewhere. A paper analyzing interlocking directorships, by John A. Sonquist and Tom Koenig, is well executed and to the point; but one is starting to tire of descriptive, methodology-creating forays through the data on directorships. A higher ratio of ideas to numbers would be appreciated.

I found the papers growing out of systems theory, in section 3, the least appealing of all, though I recognize I am less competent and less interested in this area than in the others. A nice anthropological analysis by Ronald Cohen attempts to specify the conditions giving rise to hierarchies in a hitherto somewhat undifferentiated community; the hypotheses Cohen develops are quite intriguing. Regrettably this paper fails to articulate with the other papers in this section, ostensibly on related topics.

To close, this book is about important things, but it fails, for the most part, to reach its potential. The field is still waiting, and ripe, for a synthesis in structural analysis.

The Domestic Revolution: The Modernization of Household Service in England and France, 1820–1920. By Theresa M. McBride. New York: Holmes & Meier, 1976. Pp. 160. $18.00.

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In a well-known passage in *Democracy in America* Alexis de Tocqueville pointed out that master-servant relationships in England, France, and the United States reflected the overall patterns of class relationships in those countries. Using a wide variety of historical data, Theresa McBride elaborates on this observation by showing not only how changes in 19th century domestic service reflected changes in the larger social and economic structures of England and France but also how domestic service itself played a key role in producing those changes. The study begins in 1820, when the first domestic-economy manuals were published, and ends in 1920, shortly after the close of World War I. During this time domestic service experienced a dramatic rise and fall, was feminized, and became a modern rationalized occupation. On a larger social scale the 19th century saw rapid growth of the middle classes, widespread urbanization, and industrialization.

That changes in domestic service were intimately connected to changes in the middle classes is one of McBride's major propositions. The middle classes became the new employers of servants, and for them employing servants was both a symbolic measure of their newly acquired status and a virtual necessity for maintaining an appropriate standard of domestic life. Paternalism and the expectation of deference remained important parts of master-servant relationships, but these traditional values interacted with new middle-class work and family values to produce the ambivalence about employing servants which we see even today. McBride particularly points out the importance of middle-class interest in rationalization of the household and in the nuclear family for the eventual change from live-in to daily service.

It is well known that economic pressures on the rural areas increased the supply of migrants to the city throughout most of the 19th century. McBride analyzes why, despite the harsh working conditions of service, so many of these migrants went into domestic service rather than other work. Noting that in both France and England the rise and fall of the servant group paralleled the peak and decline of rates of urbanization, she finds that domestic work provided an attractive channel for the rural-urban transition, especially for women. Temporary local domestic work had been a traditional peasant pattern, and sex-role attitudes made the protected

family setting of domestic service particularly appealing for young women. The harsh conditions of service were also offset for many years by its earnings. Although wages themselves were small, not having to pay room and board allowed many servants to save almost their entire wages, so that women could have a dowry with which to contract an attractive marriage and men might leave service to go into business.

Service thus provided a means both for integration into city life and for upward mobility. The exposure to middle-class values and to a certain amount of education by their employers added to servants' chances. McBride estimates that about one third of servants in the period experienced such upward movement. But another third experienced downward mobility, and McBridge determines that employers' complaints of illegitimacy, theft, drunkenness, prostitution, infanticide, and suicide among servants were valid though probably exaggerated.

The decline of servants occurred after 1880 in France and 1890 in England because of servanthood's declining economic advantage for both workers and employers, the growing influence of middle-class family values which resented the live-in presence of outsiders, and the growing interest of urban migrants in increasingly available alternative jobs. Teaching and clerical work were especially attractive since they were other work situations which supported traditional values about the roles of women.

A weakness in the book is the uneven use of the comparison between France and England. McBride uses the comparison to support the generalizability of her model of the role of domestic service in the process of modernization. It is disconcerting, therefore, to find places in the text where it is not immediately clear whether a statement refers to England, to France, or to both. It is also disappointing that the implications of differences between the two countries (such as the relative scarcity of land in England) are not always explored in the same detail as are the similarities. This in effect minimizes the importance of differences in pattern between the countries without adequate explanation. The result is that for me the argument for generalizability of the relationship of domestic work to the modernization process is seriously weakened. I suspect that the model is generalizable, but McBride's presentation of the argument would be much stronger if the comparisons between countries were more systematic throughout.

Despite that qualification this historical study is of great interest to more sociologists than those specifically interested in social change and modernization. It is a rich contribution to the sociology of occupations, not only for its analysis of a neglected, low-status occupation, but also for its success in showing how that occupation interacted with the larger social structure in a process of change. Finally, this study of the complex 19th-century "domestic revolution" provides valuable historical insight for the sociology of modern sex roles and family life.

Revolutionary Jews from Marx to Trotsky. By Robert S. Wistrich. New York: Barnes & Noble, 1976. Pp. vii+254. $16.00.

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Despite its interesting title, this book is neither principally about revolutionary Jews nor about the way in which the Jews relate to revolution. It is about 10 *radical* Jews or ex-Jews and how each dealt with his or her Jewishness, with Judaism as a doctrine, with anti-Semitism (an important force throughout the period in which the 10 lived), with Zionism, and with Jewish organization, particularly attitudes toward the Bund, the Jewish workers' organization in central Europe formed at the end of the 19th century.

The 10 dealt with are Karl Marx, Ferdinand Lasalle, Eduard Bernstein, Rosa Luxemburg, Victor Adler, Otto Bauer, Bernard Lazare, Léon Blum, Julius Martov, and Leon Trotsky. Wistrich divides the book by geographical focus, treating the first four subjects in a section on Germany and successive pairs in sections on Austria-Hungary, France, and Russia.

Part of the problem with the book is that, as will quickly be noted by revolutionary afficionados, few of the 10 were revolutionary. Bernstein, for example, was the founder of evolutionary socialism; Adler and Bauer were radical within the context of Austria-Hungary and latterly of Austria but hardly revolutionary; Bernard Lazare was a passionate Dreyfusard but not revolutionary; Léon Blum fell within the evolutionary orientations of the Second International. Martov represents more of a problem: he was a revolutionary in the context of czarist Russia but hardly went beyond Menshevik concerns for a transition to bourgeois democracy.

This is not simply an idle issue. A careful examination of the three who were revolutionary shows them to be the most uncompromising internationalists. This category includes Marx, Luxemburg, and Trotsky. (Lasalle is difficult to categorize: hardly a revolutionary because of support of the Prussian state, his position came close to being *very* radical; he was a German nationalist rather than an internationalist.) Thus revolutionary orientation and internationalism appear to be strongly correlated.

But this correlation tells little about how each of the 10 related to his or her Jewishness. Marx, Lasalle, Luxemburg, Adler, Bauer, Martov, and Trotsky were most clearly assimilationist. Bernstein, Lazare, and Blum maintained or developed an orientation toward their own Jewishness. Of the eight to whom the issue of Zionism applied (Marx and Lasalle lived too early), all but Bernstein, Lazare, and Blum were anti-Zionist. Thus there appears to be a negative correlation between the association of Jewishness with Zionism and that of Jewishness with revolutionary orientations and internationalism.

One might have hoped for more systematic treatment of how each *personally* related to Jewishness. While Wistrich provides some information on parentage, he does not provide systematic information about whether his subjects married Jewish wives or how their children were raised. This is somewhat unfortunate since such personal data often provide insights into the degree to which ethnic identification operates.

Each section opens with a general discussion of the country under examination. This is followed by chapters devoted to individuals. Wistrich focuses on the writings of each of the 10 with respect to Jewish issues. One interesting point which emerges is the degree to which some radicals were associated with anti-Semitic movements and individuals in the burgeoning working-class movements of Europe. Thus, Bernstein identified with Dühring for several years before Dühring became openly anti-Semitic. Luxemburg looked the other way when issues of anti-Semitism arose. The Austrian socialists struggled for many years with social Christian movements that became increasingly anti-Semitic. Because many bankers and manufacturers were Jewish, the inability to separate the bourgeoisie from the Jews became an important issue for Europe's working-class movement. Wistrich's treatment is interesting and worth reviewing if only to clarify, in the light of history, some of the weaknesses inherent in the early developments of the working-class movement.

A major problem that I encountered with this book developed from Wistrich's identification with Judaism, Jewishness, and Zionism. The author is fundamentally unsympathetic to those individuals who were apostate, rejected Jewish identification, or refused to concern themselves with anti-Semitism. The term "self-hate" is used continually by Wistrich to characterize these persons. Wistrich thus regards Jews who are assimilationist or who are anti- or non-Zionist to be manifesting a psychological process, self-hatred. He sees assimilationism as *hatred* of *self* rather than a reaction against a limiting background or a rejection of a social group. I remain unconvinced that Marx, Lasalle, Luxemburg, et al. hated themselves. That each had reservations about Jews and their own Jewish antecedents is clear. Wistrich's reasoning is that a rejection of any socialization process through which one goes constitutes self-hatred. If so, how shall we characterize some Germans (who knows how many?) raised under Nazi conditions but rejecting them? Surely self-hatred is an incorrect designation. Wistrich's labeling represents simple name calling and is inadequate and unsatisfactory.

Despite being irritating, this book is interesting. The subject of revolutionary or radical Jews has long been discussed, but little literature has appeared on the subject. In this sense, the book has been long overdue. Might one suggest, or might this seem to be catering to the very anti-Semitism that Wistrich discusses, that a companion volume—*Capitalist Jews*—in which a similar analysis is undertaken of Jews on the "other side" would be useful?

The Pursuit of Knowledge in the Early American Republic: American Scientific and Learned Societies from Colonial Times to the Civil War. Edited by Alexandra Oleson and Sanborn C. Brown. Baltimore: Johns Hopkins Press, 1976. Pp. xxv+372. $16.50.

Donald deB. Beaver
Williams College

This collection of 16 articles redresses an oversight concerning the social and institutional history of knowledge by providing the first general picture of the historical development and sociocultural role of learned societies in America before the Civil War. Although 12 of the contributions are by historians of science, technology, or medicine, the variety of subject societies is surprisingly great. Included are agricultural (M. W. Rossiter), medical (J. H. Cassedy), technological (B. Sinclair, B. Hindle), natural historical (P. A. Gerstner), humanistic (M. A. McCorison), western (H. D. Shapiro), southern (J. Ewan), and Canadian societies (P. J. Bowler). A matching diversity of approaches yields an abundance of valuable information, often of such contrast that it may prove difficult to establish commonalities.

The opening selections are concerned in general with early influences and patterns of development (J. C. Greene, A. H. Dupree, G. F. Frick). They are also the most explicitly theoretical, even though in later, more narrowly focused studies, both Sinclair and Shapiro critically discuss methodological and historiographical concerns. Of the early essays, Dupree's adopts a viewpoint which might profitably be adopted more often: the treatment of learned societies as dynamic components of an evolving information system, itself subject to a variety of developmental pressures.

A relatively commonly held perspective views developments in terms of professionalization, a dominant and conventional frame of reference for the social history of science in 19th-century America. Many of the articles refer to the transition from "amateur" to professional; refreshingly, however, several do so in challenge to the convention. Rossiter's essay illustrates its inapplicability to institutional developments in the technical subject of agriculture. N. Reingold, in an illuminating discussion of the distinct types of "members" of the scientific community, explicitly sets out to rethink professionalization as a model, pointing out pertinent shortcomings and qualifications. The most thought-provoking challenge, however, is given by Bowler's valuable comparative study, exemplifying professionalization's inadequacy in explaining the development of scientific societies in neighboring Canada.

Related to the theme of professionalization is common agreement on the existence of a significant difference in the characteristics of learned societies before and after 1840. This appears in Gerstner's and Rossiter's choice of time periods for analysis, and in Cassedy's pattern of the separation of

medicine from scientific and other learned societies. Shapiro indicates a similar demarcation, claiming that the crucial event in pre–Civil War science in America was the transformation of the content and structure of science.

The treatment of the learned societies' social role and function commonly presents themes of ambivalence, conflict, and competition, not surprising features of a struggle on the part of individuals of diverse social backgrounds to create lasting and functional learned institutions outside university walls (cf. J. M. Hobbins, Sinclair, Shapiro). Illustrations of ambivalence appear most clearly in discussions of the problems of evaluating the different claims of theory and practice (Rossiter), the gulf between the expectations and attitudes of the public and of the experts (Sinclair), and between the conflicting needs for popular support and professional autonomy (S. G. Kohlstedt). Fundamentally, these issues derived from the thorny and largely unresolved problem of establishing universally agreed upon criteria for evaluation of ideas and of professional qualifications. Further clarification requires the provision of an adequate, complementary picture of the public's image of and response to the learned societies and their "knowledge."

The great variety of approaches tempts one to search for some unifying perspective, yet simultaneously prohibits easy generalization. So many different developments are reported that they highlight the difficulty of comprehending the special nature of what passes for learned knowledge, over time, in a particular culture, and how that nature may differentially affect its institutional representation. How substantial a change occurred can be seen by comparing Frick's study of the Royal Society's influence on colonials with Kohlstedt's study of the members of the American Association for the Advancement of Science.

As with any collection of articles produced by a number of authors, criticisms of uneven quality, divergent approaches, and lack of integration might be leveled. But in this case the divergent concerns are of positive value, and design does underlie the order of presentation. Furthermore, prefatory essays by I. B. Cohen and J. Voss, an introduction by A. Oleson, and two commentaries by R. J. Storr and B. G. Rosencrantz serve to prepare, outline, and synthesize the approaches and analyses of the individual works.

This collection has a number of special merits. It presents an abundance of new data about the members of the early learned societies, pertaining to their sociocultural backgrounds, their purposes and ideals, and the factors which aided or frustrated efforts to realize them (cf. Hindle, J. M. Hobbins, W. M. Whitehill). A considerable body of data is also presented in quantitative form (Reingold, Hobbins, Kohlstedt). Further, the range and breadth of the studies is so extensive that they provide foundations for the start of a useful and illuminating comparative analysis. Finally, the volume is truly interdisciplinary, assembling in one convenient location what hitherto has been widely scattered among different specialties and therefore obscured.

There can be no question that this collection of essays constitutes a valuable and stimulating contribution, a landmark volume for those interested in the historical sociology of knowledge, the sociology of professions, and the social history of scientific institutions.

Public Health and the Medical Profession in the Renaissance. By Carlo Cipolla. New York and London: Cambridge University Press, 1976. Pp. viii+136. $14.50.

Mervyn Susser
Columbia University

This is a small book but a valuable one. It will be of interest to sociologists concerned with social institutions and professions as well as to historians and scholars in medicine and public health. It deals separately with two interrelated themes and brings new data and a new understanding to both. The first theme is the development of public health in Northern Italy from the 14th to the 17th century. Quarantine (40 days of isolation) is a venerable response to the threat of epidemics, and although its origins are always attributed to Venice conceptions of the institutions that gave rise to it have been fragmentary. The second main theme of the book is the development of medicine as a profession over the same period, particularly in the early 17th century in Tuscany.

With regard to public health, Cipolla's account reveals a continuity of ideas that has long been unsuspected because of discontinuity in institutions between Renaissance Italy and Victorian Britain. The late 18th-century work of Johann Peter Frank and his concept of medical police was less the harbinger of what was to come, as it is usually seen, than the codification of practice developed in Italy over the previous four centuries. His work reflects the fact that he had been director general of public health in Lombardy and professor of clinical medicine at the University of Pavia. Modern public health ideas must now be traced much further back than 19th-century Britain.

The story begins with the first major onset of plague in Europe in 1347. The states of Northern Italy were wealthy and advanced societies, and their responses were more effective than those of other places and countries in Europe. These states, and Venice in particular, set up temporary boards of health with powers to control movement and take other steps against contagion. As the plague harassed Europe in one calamitous episode after another over the next 150 years, the boards became institutionalized and permanent. By the middle of the 16th century the health board of Venice had acquired considerable jurisdiction. The following matters came under its control: the marketing of meat, fish, shellfish, game, fruit, grain, sausages, oil, wine, and water; the sewage system; the activity of the hospitals; beggars and prostitutes; burials, cemeteries, and pesthouses; the profes-

sional activity of physicians, surgeons, and apothecaries; the preparation and sale of drugs; the activity of hostelries, and the Jewish community (*sic*). Epizootics were another main concern of the board. The epidemics of plague to which these powers were owed were devastating. Sometimes as many as 75% of the population of large cities like Genoa died.

Cipolla outlines the conflicts of interest that rose in these cities, particularly between the health boards and the merchants and workers. Health officers, direct representatives of the governing bodies, were usually members of the nobility. In their difficult task they frequently had to balance the "morality" of duty against its effects on the daily life and trade of the community. Quarantine, the disinfection and fumigation of goods, the regulation of all traffic in and out of cities, had an immense cost both in direct outlay and in the depression of trade. Merchants and workers were often ready to rise up in protest. For workers who had no reserves, loss of their daily work because trade stopped often meant starvation. They preferred the chance ravages of plague to the certainty of starvation for themselves and their families. Priests and prelates too had their reasons for opposing the measures of the health boards.

The actual cause of the plague was unknown, of course, and it is doubtful whether health board intervention accomplished much. It focused on isolation and disinfection, and might have affected the flea population which carried the plague from rats to man, but the rats which carried the plague nonetheless multiplied. The health boards of several cities did maintain an effective epidemic intelligence service and warned each other of the onset of plague wherever it occurred. They were less likely to announce the outbreak of plague in their own cities. Yet Cipolla finds the health boards in general efficient, earnest, and not corrupt.

With regard to Cipolla's second theme, the development of the state of the medical profession in Tuscany in the 17th century, we again learn new things. He unearthed a unique census of physicians and surgeons conducted by the health board of Florence throughout its jurisdiction in 1630. The health board of Florence, faced by a plague outbreak, wished to know what the supply of physicians and surgeons might be. Here again we find discontinuities in medical professional development between Italy and northern Europe. University-trained physicians were not the elite they have commonly been held to be, to be found only at the courts of the popes and the dukes and rulers. By the time of the Tuscan census, university-trained physicians, it appears, were almost as common as any others. They were distributed widely across the Grand Duchy and many small towns. Many communities supported physicians and provided them with a stipend and a place to live. Ratios of physicians to population were as high as 4.1 per 10,000 in Florence, 9.2 per 10,000 in Pisa, and 5.5 per 10,000 in Pistoia. In Venice at that time the publicly supported physicians were in the ratio of 3.0 per 10,000 population.

The preeminence of Italian Renaissance medicine has been well recognized by historians. Less well recognized has been the extent to which medicine was institutionalized as a profession and dispersed across the

North Italian city-states. The licensing of physicians required examination by a college of physicians, an upper-class institution that assumed the regulating and controlling functions of the old guilds. The college represented more than an administrative expedient. It was the result of profound social change in the course of which class distinctions had become well marked and ossified. By the seventeenth century physicians had been assimilated into the upper class. Surgeons, barbers, and apothecaries ranked with the lower orders and treated the lower orders.

Cipolla goes on to discuss what the costs of medical ignorance might have been with regard to the use of worthless and sometimes harmful remedies. As Ivan Illich does in writing about modern times, he is able to draw such evidence as there is from the writings of enlightened physicians of the time. The costs of medicine to societies, both economically and in the dangers they risk from it, are inherent in well-off and developed societies rather than others. So it was that at that time Northern Italy more than other places in Europe suffered the costs as well as the meager benefits of medicine. This was especially true for public health. Some public health interventions may indeed have been devastating.

To have illumination together with brevity is a rare pleasure. To be sure, one would have welcomed an elaboration of the social class differentiation among healers within the stratifying society in which they functioned. One could have wished also for a better understanding of the economic and social interests represented by the health boards. One may hope for these things from Cipolla's subsequent work.

Doctors Wanted: No Women Need Apply: Sexual Barriers in the Medical Profession, 1835–1975. By Mary Roth Walsh. New Haven, Conn.: Yale University Press, 1977. Pp. xix+303. $15.00.

Barrie Thorne
Michigan State University

Many have hailed the recent increase in the number of women medical students (from 9% of total U.S. medical school enrollment in 1969 to 20% in 1976) as a historic breakthrough. Such optimism, however, should be tempered by this book, which documents and analyzes a similar breakthrough for women physicians in the late 19th century, followed by a series of reversals which nearly erased the earlier gains (for example, there were fewer women physicians in Boston in 1950 than in 1890).

Exploring this earlier rise and fall of women in the U.S. medical profession, Mary Roth Walsh focuses primarily on the "regulars," those physicians who eventually gained control of medicine. (This focus, she acknowledges, neglects the experience of women in the "irregular" sects, such as homeopaths; a more integrated historical analysis of the full array of women healers would be useful.) To illuminate national trends, she uses Boston

as a case study, noting that the struggle of women physicians in that city presaged events elsewhere. Some of the earliest challenges to the male monopoly of medical institutions were launched in Boston by pioneers like Harriet Hunt, a practicing physician who in 1847 became the first of many women to petition Harvard Medical College for admission (women were not admitted until 1945), and Marie Zakrzewska, who founded the first hospital in the country staffed by women physicians. Boston was also the seat of prominent opponents of women's entry into medicine, including physicians like Horatio Storer, who argued that because of "menstrual difficulties" women were unfit for medical practice.

Walsh traces the sequence of struggle: repeated and often well-organized efforts by women to gain access to medical schools, hospitals, and societies; and strong male resistance, partly inspired by economic fears (Victorian morality, sugesting that women might best be treated by physicians of their own sex, gave women practitioners a potentially large clientele). Denied access to mainstream institutions, women, drawing support from the broader feminist movement, formed their own medical schools, hospitals, and professional societies. Eventually several prestigious medical schools admitted women, although women practitioners were kept at the margins of the profession. The period from 1850 to 1900 was one of progress for women physicians; by 1893 women were 10% or more of the students enrolled in 18 regular medical colleges around the country.

This period was heralded as a breakthrough, but it was followed by a series of reversals. After the 1890s the percentage of women students declined; coed medical schools began to restrict sharply the admission of women, and by 1903 women's medical colleges had almost disappeared. The 20th century became (until very recently) a period of stagnation, with women limited to token representation in the medical profession.

Earlier accounts have attributed the decline of women physicians either to decreasing inclination of women to enter medicine (on the contrary, Walsh demonstrates that there was a continuous supply of women who were denied access to medical training) or to the process of professionalization. According to the latter explanation, as medical training became more formalized and licensing laws took hold women were less able to compete, and their numbers diminished. Walsh disputes this with evidence that women did well on licensing examinations and by tracing historical patterns: there were few women doctors in the early 19th century, and women made their greatest gains in the late 19th century, the time of rapid professionalization. The professionalization of medicine may, in some respects, have worked to the advantage of women. More formal modes of entry, such as licensing tests, made barriers visible, and formal credentials enhanced the credibility of women physicians.

Walsh argues that the decline of women physicians in the early 20th century was not a by-product of the professionalization of medicine but the result of deliberate sex discrimination by those who controlled medical schools, hospitals, and societies. Furthermore, many of the seeming break-

throughs were made because women were able to take advantage of problems in the medical world. Medical schools were willing to admit women when they lacked competent male applicants (e.g., during both world wars, or in a school's early years) or when they were financially hard pressed (e.g., Johns Hopkins became coed to qualify for a large donation raised by women). But the lowering of barriers was never founded on substantial commitment to the medical education of women, and once a particular problem was solved barriers were raised once again.

Walsh neglects the importance of professionalization in implementing deliberate discrimination against women. In the long run, professionalization eliminated alternative modes of access to medical work; it restricted the numbers of practitioners and erected new, institutionalized hurdles such as longer, school-based periods of training, and internships. The tightened monopoly which was an outcome of professionalization gave the white male establishment more power to exclude women and minorities from medical practice.

When she turns to the present, Walsh discusses discouragements to medical careers which infuse the socialization of female children and the structuring of family and work in the lives of women. Examining the way these factors affected women's entry into and practice of medicine in the 19th century would have given a more rounded view of the past.

Overall, the book is beautifully written, well documented, and argued with sociological sophistication. The Boston case study makes especially good reading. It is a book with a message for the present, especially in a time when a backlash against affirmative action is under way in medical schools. And the historical materials and analysis are of special relevance to those interested in professions, in medicine, and in sexual inequality.

The News People: A Sociological Portrait of American Journalists and Their Work. By John W. C. Johnstone, Edward J. Slawski, and William W. Bowman. Urbana: University of Illinois Press, 1976. Pp. ix+257. $9.95.

Phillip J. Tichenor
University of Minnesota

Although the study of gatekeepers has become a major area in mass media research, the field has lacked a comprehensive analysis of gatekeeper occupations and the organizational context in which they exist. The void has now been amply filled by this impressive monograph based on a sample of 1,313 journalists in daily and weekly newspapers, news services, news magazines, and broadcast stations. The book stands as a landmark contribution to the field of mass communication and as a major addition to the literature on sociology of occupations.

The authors proceed on the sociologically sound premise that characteris tics of working journalists and their news organizations bear heavily o the question of why American citizens read, see, and hear some news item and not others. Nearly 90% of the journalists sampled work in communitie of more than 50,000 population and nearly two-thirds are from white collar families. Such findings illustrate the urban, middle-class environmen of news gathering and distribution and help explain why urban events values, and perspectives dominate media content.

Have American journalists established themselves as a professional com munity? Here, using traditional sociological measures of professional status the authors arrive at mixed results. In their values, journalists as a whole are much like other professionals, ranking public service and autonomy high, well above job security and financial rewards. But in organizational behavior journalists differ sharply from members of some of the other professions: no single professional organization accounts for more than a small minority of the sample, with a great deal of heterogeneity among organizations that do exist. There is also serious question about the existence of a unique body of abstract knowledge in journalism. Furthermore, about two-fifths of the journalists in the study had not completed college.

A section of the book that may intrigue journalists, media students, and critics alike examines professional autonomy. To what extent do journalists perform their tasks according to their own best professional judgment? About three journalists in four claimed to have a free hand in writing their stories, but fewer than half felt they had full control over what they wrote about. The authors also find that autonomy varies predictably according to size of news organizations and levels of bureaucratization. Autonomy (as perceived by the journalists) is higher in small news organizations than in large ones and higher among journalists with managerial jobs than among full-time reporters. In this profession, then, the management-oriented career pattern may be favored not only for its greater financial rewards but also because it increases the likelihood of realizing one of the most cherished journalistic ideas.

An analysis highlighted in a separate chapter deals with the "underground" or "alternative" press, which included upward of 1,000 newspapers and magazines in 1970 when the study was conducted. The authors found "alternative" journalists characterized by a high degree of idealism, low pay, a great deal of informality, and high individual autonomy. The last attribute appeared to be as much a result of organizational size as of values.

While data on the organizations for which journalists work were gathered independently, the study otherwise rests on self-reports of journalists gathered by telephone interview. Neither print content nor broadcast messages were measured; the analysis of organizational individual characteristics, as they relate to media content and performance, must await another investigation. Meanwhile, the authors have produced a major baseline work which is unique in the mass media literature. The only comparable work anywhere is Jeremy Tunstall's *Journalists at Work,* which presents an analysis of specialized British reporters.

Of Woman Born. By Adrienne Rich. New York: W. W. Norton & Co., 1976. Pp. 318. $8.95.

Kristin Luker
University of California, San Diego

Looking over the broad expanse of women's experience in the past 300 years, two events stand out in clarity. The first, the Industrial Revolution, led to the separation of home and work as two physically and emotionally different experiences and intensified the role segregation of men and women, each in their own separate spheres of influence. The second, less heralded revolution (probably because it is so historically close) is the separation of sexual activity from reproduction. Like any revolution, one can quibble about when it actually began (rubber was vulcanized in the 1840s and rubber pessaries were developed not long after), but it seems clear that the rapid distribution of the potentially 100% effective birth control pill in the early 1960s and the change in attitudes about abortion in the late 1960s have been a watershed in this revolution.

Of Woman Born is both a product of this latter revolution and an analysis of it. As Karen and Jeffery Paige have pointed out, in all societies until recent times children have represented the largest single capital asset available, either as workers or as members of a kinship lineage. As a result, the production of children is as encrusted with myth, norms, values, and regulations, in short, as "mystified" as is the production of capital in contemporary society. Precisely because the production of children is so fundamental to the social order, an ideology exists which asserts that motherhood is natural, intrinsic, and, most important, *voluntary,* thus hiding from view the extensive and pervasive social pressures which control and regulate fertility. As Adrienne Rich argues: "In the being of a woman sold as a bride, or rejected because she is 'barren' and cannot produce sons to enhance a man's status, economics and sexuality, legalism and magic, caste structure and individual fear, barter and desire co-exist inextricably . . ." (p. 81).

Rich has assigned herself a delicate and daring task in *Of Woman Born.* She attempts to pin down the rapidly shifting boundaries between internal, subjective realities ("motherhood as experience") and external, sociological realities ("motherhood as institution"). Because of her profession as poet and her skill as creative thinker, she realizes that this is a complex, interwoven, overlapping job: "We are talking at one and the same time about the physical realm of human biological reproduction and nurture, the cultural/historical realm of what human beings have invented, prescribed, designed in their efforts to live together and the realm that exists within the individual psyche" (p. 95). What she seems less aware of (as befits an intellectual pioneer) is the revoluntary aspect of even taking on such a job. As Diane Horowitz has noted, social scientists until recently have assumed that women became mothers because they wanted to and because it was

in the natural order of things; such an individual, deterministic view would have been instantly rejected by social scientists as tautological had it been applied to any other social role, such as doctor, lawyer, or even criminal.

What Rich has done in *Of Woman Born* is to make the invisible visible. She shows us how motherhood is everywhere so totally and thoroughly socially structured, and its rules so effectively internalized, that social ordering becomes taken for granted, and the internalized social rules come to be experienced as a part of the self. Using a poet's insights and impressively wide-ranging reading, she explores the social/personal boundaries of motherhood, all the time reminding us that in motherhood "social" and "personal," "external" and "internal," are problematic and intertwined categories. (In a related context, she uses the metaphor of pregnancy to demonstrate that for women "inner" and "outer" are not the clearly bounded categories they are, for example, for Erik Erikson et al.

Thus Rich brings such a clear eye to the institution and experience of motherhood that she constantly confronts the reader with "aha!" experiences. She reminds us, for example, that, linguistically, to "father" a child connotes the brief, biological act of fertilization, while to "mother" a child connotes an ongoing, long-term, emotional relationship. Or she parallels "mother-work" with wage work: "The worker can unionize, go out on strike; mothers are divided from each other in homes, tied to their children by compassionate bonds; our wildcat strikes have most often taken the form of physical and mental breakdown" (p. 53).

Perhaps the most pleasurable part of Rich's exploration of the "taken-for-granted" reality about motherhood is her ability to work phenomenological sandings-down-to-the-bare-wood in spare, graceful, inventive language. She coins the term "patrivincialism," for example, to describe the tendency in intellectual thought to assume "that 'man,' 'humankind,' 'blacks,' 'the working' holds for mothers, daughters, sisters, wet-nurses, infant girls." (In other parts of the book she talks of the "torsion of a piece of music" and the "drenching light of a painting.")

This book by one of America's leading poets is one which no social scientist interested in either women or fertility can ignore. While it is not science in the usual sense of "proof," it transcends the usual categories of art, to our benefit. W. H. Auden's advice notwithstanding, we are fortunate that Adrienne Rich has chosen to "sit with statisticians" and, in a way that enriches both art and science, to illuminate both the personal experience and the social structuring of motherhood.

Woman's Body, Woman's Right. By Linda Gordon. New York: Grossman Publications, 1976. Pp. xviii+479. $12.50.

Constance Lindemann
University of Oklahoma

This book contributes to the understanding of birth control as a women's liberation issue. Perhaps of greater importance, it provides new and pro-

found insight into the controversy between revolution and reform, that is, between the total program approach which seeks changes in social power relationships and the single issue approach which seeks changes on single social issues within existing societal power relationships.

Defining birth control as "efforts to increase individual control over reproduction" (pp. xiv–xv), Linda Gordon distinguishes between birth control as a social movement and birth control as contraceptive and abortifacient technology. She shows that to be understood as a social movement birth control must be considered in the context of sexual repression and the oppression of women, since the goals of the birth control movement change under the impact of these two social factors.

Gordon carefully documents the birth control movement as it passes through both total program and single issue phases. She shows that in unequal situations gains on single issues are manipulated against the less powerful and turn into losses. For example, in Victorian times sexual repression was used to oppress women in all other social areas. Today women have gained sexual freedom, but it is used to coerce them into being readily available sex objects. Before, a woman was forced to say yes to sexual intercourse because conjugal rights demanded it; now she is coerced into saying yes because a sexually free society expects it. In either case the woman is not sexually free, and reproductive self-determination is at the least questionable and at the worst impossible under these circumstances. When class and sex relationships are basically unequal, gains in a single area become counterproductive and contribute further to oppression. As the author puts it, "the pretense of equality in the face of basic inequality merely contributes to the inequality" (p. 413).

The value of viewing the birth control issue in the context of social and political history extends also to people who played major roles in the movement, in particular, Margaret Sanger. Reviewers of books about Sanger have noted that she is either idealized or severely criticized and given total responsibility for the problems of the birth control movement. Gordon recognizes Sanger's personal foibles and the part she played in directing the movement away from a class and feminist context, but at the same time she shows clearly that class movements and feminism were as responsible as Sanger for that direction, because they did not recognize the radical potential of the birth control movement and give it full support. She shows that Sanger was forced to seek the support of professionals, thereby changing the character of the movement. This dialectical analysis of Sanger's relationship to the birth control movement provides a much more understandable view of her role in the movement than appears in other books about her.

An important topic in this book is the analysis of the population control movement which, Gordon shows, is distinct and separate from the birth control movement. Gordon traces the organizational and theoretical background of the population control movement and shows that it is the direct successor to the elitist and racist eugenics movement of the early 1900s. That movement sought to contain and limit breeding among the "undesir-

able" masses (immigrants) who bred "feebleminded" children and to promote breeding along elitist lines. It attributed all social ills to overbreeding rather than to inadequacies of the social system. Today the population control movement seeks to control births particularly among Third World peoples abroad and poor people in the United States. It, too, blames overbreeding rather than the social system for social ills. Both, in essence, blame the victim. This movement has been confused with the birth control movement for reproductive self-determination. Gordon recognizes the necessity for considering the question of overpopulation, but points out that the population control movement always considers birth rate reduction before women's right to reproductive self-determination, often to the detriment and harm of women.

The author states that this book is "a documented plea for the importance of a total program of female liberation, for a never-ending vigilance to view birth control and sexual activity in the context of the over-all power relations of the society, especially sex and class relations" (p. 415). As such it illustrates the use of social science scholarship in the service of a particular perspective of human liberation. Undoubtedly many readers of the book and of this review will take issue with this use of scholarship. To paraphrase the author: The pretense of taking no sides in the face of unequal sides contributes to the more powerful side.

Quantitative Applications in the Social Sciences. Edited by Eric M. Uslaner. Sage University Paper Series. Beverly Hills, Calif.: Sage Publications, 1976. Vol. 1: *Analysis of Variance.* By Gudmund R. Iversen and Helmut Norpoth. Pp. 95. $3.00. Vol. 2: *Operations Research Methods.* By Stuart S. Nagel with Marian Neef. Pp. 76. $3.00. Vol. 3: *Causal Modeling.* By Herbert B. Asher. Pp. 80. $3.00. Vol. 4: *Tests of Significance.* By Ramon E. Henkel. Pp. 92. $3.00.

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Under the editorship of Eric M. Uslaner, Sage Publications has performed the commendable service of providing introductory explanations of several quantitative methods enjoying wide use in the social sciences. The first four volumes, under review here, deal with analysis of variance, operations research, "causal" modeling or path analysis, and classical tests of statistical significance. Designed for those whose methodological background is not extensive, they present a well-organized guide to the voluminous literature accumulating in these areas. Distinctive to the series is its emphasis upon underlying assumptions, substantive interpretation of results, the proper place of these methods in theory construction, and criticism of "cookbook" applications. While the presentations are uniformly economical

and cogent, some of the essays raise controversial issues which might best have been excluded from introductory expositions.

Analysis of Variance by Gudmund R. Iversen and Helmut Norpoth introduces one-way, two-way, and three-way ANOVA in the usual ways but discusses residual analysis and other special topics essential to the reasoned application of this sturdy statistical method. The writers discuss not only "fixed-effects" models common in nonexperimental research but also "random-effects" models, and provide a convincing social science example of the latter.

For the sociological community, the most valuable essay in this collection will be *Operations Research Methods* by Stuart S. Nagel with Marian Neef. These authors present linear programming and optimum-mix analysis (chap. 2), inventory modeling and optimum-level analysis (chap. 3), and some elements of decision theory (chap. 4). Operations research methods—particularly linear programming—have not been widely used in sociological research, and this informative essay, drawing upon the authors' own innovative work in the study of political and legal process, shows through example many potential applications for sociological studies of the allocation of resources in rational social systems. Sociologists are not likely to have a formal grounding in these methods (with the possible exception of decision theory), and so the effort of Nagel and Neef seems quite worthwhile.

Causal Modeling by Herbert Asher presents elementary techniques of "causal inference" for systems of variables in which the endogenous variables are quantitative. The Simon-Blalock approach, whereby intervening and antecedent variables are treated as controls in partial correlation, is presented and criticized. Fully recursive models are thoroughly discussed, and nonrecursive models (including identification problems) are briefly introduced. Asher's treatment of recursive models is somewhat misleading, because he argues that fully recursive models require a "large number of stringent, unrealistic assumptions" rarely satisfied in reality (p. 26). Part of the fault of recursive models, Asher claims, lies in the assumption of recursiveness itself. He believes that nonrecursiveness is in fact the rule for interesting social data and that with recent developments in econometrics and psychometrics "there is no need to confine oneself to recursive models out of fear of the complexity of nonrecursive analysis" (p. 68). The problems of finding suitable instruments to identify reciprocal "causal" effects, as well as understanding the very *meaning* of reciprocal causation in many static, cross-sectional path models, make the recursive-nonrecursive choice an extremely complex one. The issue is certainly more subtle than Asher indicates, and he would have done well to include, say, Wold's criticisms of nonrecursiveness in his introduction. The assumptions of recursive causation are not so unrealistic; if our present "theorizing" always leads us to reciprocal causation, perhaps our theory or our data need overhauling, not the recursive causal model.

Asher finds other assumptions of linear "causal" (recursive and nonrecursive) models unrealistic too, and sometimes presents an unduly cautious

exposition of the power of these methods. For example, he finds the assumption of linearity itself "a matter of convenience rather than a well justified claim" (p. 28), whereas this fundamental assumption is usually justified by the fact that we can almost always approximate any nonlinear relationship by a linear one (e.g., by Taylor's Theorem in calculus). Some other statements of misplaced emphasis also occur. On pages 26 and 63 Asher tells us that partial regression coefficients are always attenuated toward zero when the independent variables are subject to random measurement error, whereas this statement is necessarily true only for the simple regression case. Asher argues throughout for a "more causal approach" to social data analysis, but in this argument he is both too pessimistic about required assumptions in linear structural models and too optimistic about the theory-building capacities of complicated (e.g., nonrecursive) models. With respect to the latter point, it is telling that the recent book by Otis Dudley Duncan (*Introduction to Structural Equation Models* [New York: Academic Press, 1975]) did not include a single sociological example of a theory built out of path analysis. Because of the fascination these days with "causal inference" as the primary activity of empirical social science, these criticisms of Asher's otherwise lucid introduction to the subject are not trivial.

The most controversial essay in this series is Ramon E. Henkel's *Tests of Significance*. After leading the reader through the logic and technique of several standard statistical tests, in the end this essay questions the "utility and sensibility of employing significance tests in social research" (p. 76). Henkel even goes so far as to claim that significance tests are "detrimental" to social science (p. 91, n. 32), leaving the reader in a quandary over the very purpose of the essay. Henkel's intent is apparently to dissuade the social science researcher from using significance tests at all. It would take up too much space here to present all of Henkel's arguments and their counterarguments (he devotes only a small paragraph to the merits of significance tests). It seems clear, however, that he has presented only one side of a very complicated controversy that should not be the subject of an expository essay of this type. Tests of significance only play a small part (though an *important* part) in the statistical analysis of data, and Henkel overstates the degree of abuse of significance tests in social research today. While one must concur with the author's criticisms of the unthinking application of statistical tests, it is not so difficult to find arguments in their favor as he claims. Social science theory and sampling theory in statistics are developed enough to justify classical hypothesis testing in many instances: a social theory which does not admit tests with sample data is not really a theory at all. If for a given sample from a given population alternative theories are suggested on a posteriori basis, these alternatives can be tested with new samples from the same or different populations, or the researcher can employ simultaneous inference procedures to protect him from certain errors which can arise from the routine tests. A telling criticism of Henkel's point of view—as well as that of others who scorn significance tests—is that no convincing alternative to significance testing is put forth. While the exposition is very clear, Henkel's advice on

the use of significance tests should be tempered by arguments from the alternative point of view.

These four volumes could prove valuable for those who are seeking nontechnical introductions to these essential methods. In the mid-1960s such essays would have been widely received by the social science audience. One can only hope that in the late 1970s students will find more technical introductions in statistics, econometrics, and linear programming of greater use.

Juvenile Victimization: The Institutional Paradox. By Clemens Bartollas, Stuart J. Miller, and Simon Dinitz. New York: John Wiley & Sons, Halsted Press, 1976. Pp. xv+324. $17.50.

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The sociological literature on juvenile institutions has been lagging behind the literature on other types of total institutions and other aspects of the juvenile system. Much of what purports to reveal the dynamics of life inside these institutions is actually based on research conducted in adult prisons. Some studies, such as that by Howard Polsky on Cottage Six, provide only a partial picture; others are old and outdated. The research done in England cannot be generalized to the United States because of cultural differences and differences in the compositions of institution populations (mainly racial composition). Nor does the future of research in this area look promising. The emerging rights of offenders in general, and juvenile delinquents in particular, already make research very difficult. New laws about confidentiality of records, coupled with a refusal by inmates and administrators alike to permit research, present roadblocks for the conduct of independent studies within institutions. With this in mind, we should welcome every new addition to the limited number of books on life inside juvenile institutions.

However, it is with mixed feelings that I report on this new book. It concentrates mainly on victimization within one Ohio institution—victimization of inmates by inmates, inmates by guards, guards by inmates, and everybody by the administration. The major emphasis, however, is on victimization of inmates by other inmates. The authors attempt to present a critical view of the juvenile-justice system as one which creates victimizations instead of reducing them. This is a noble attempt, and it is too bad that it has resulted in an unconvincing and, at times, confusing study.

The institution in question is a custody-oriented one. It has a capacity of 192 boys, but at the time of the study the inmate population was below capacity. It houses mostly hard-core delinquents, boys with disciplinary problems, and runaways. Built in 1961, it utilizes the cottage system. At

the time of the study, slightly more than 50% of the inmates were blacks and slightly fewer than 50% were whites.

In pursuing the subject of victimization, the authors collected information from a variety of sources, yet the core of the study is based on information obtained through questionnaires distributed among staff members who were directly involved with inmates. These questionnaires were supplemented by interviews with some staff members. The ratio of response by staff members was very low (only 36 of 56 responded to the questionnaire), and the authors never tell us who responded and who refused to do so—a serious omission in the light of the role played by staff in several forms of exploitation. Though an attempt was made to interview inmates the authors admit that very little information was gained by these interviews, probably because of fear and/or unwillingness to "rat" on fellow inmates. We are given two different accounts of the number of inmates interviewed. In one place we are told that a "number of inmates" were interviewed (p. 42), in another we are told that "nearly every . . . inmate in the institution was interviewed" (p. 41). The claim that participant observation was also used is open to serious question. It is based on the fact that one of the authors was a staff member in the institution at the start of the study and probably through its early stages. However, it seems that this person never engaged in any systematic form of participant observation. The authors do not even tell us what his role in the institution was. The institution has eight cottages and the data included cover all of them, but we are not even told in which cottage the observer worked. Most of the information is impressionistic in nature and does not derive from any systematic gathering of data. The authors' claim that they "tried to . . . quantify the inner workings of institutional life . . ." (p. 265) is entirely without base: the only quantifications in the book are presented in the form averages and rank orderings of five groups of inmates on three psychological scales, IQ tests, height, prior record, and similar data, which are certainly not part of the "inner workings of institutional life." Other methodological and statistical problems also detract from the value of *Juvenile Victimization.*

With all these qualifications, what does the book tell us? We learn that "exploitation was not found to be a 'cut and dried' phenomenon" (p. 38). The authors talk about exploitation in the following general areas: clothing, food, cigarettes, and sex. While some inmates were found to be serving in the role of exploiters only and others were only victims, a large proportion of the inmates (50 out of 149) sometimes exploited others and sometimes were exploited (this finding is based on a summary of staff ratings and should not be viewed as representing the actual proportion in the inmate population). The authors were "surprised" to find this large group, which they call "give and takers." I am surprised that this group is not even larger. We also learn that some boys are victimized more than others, that some boys are not involved in the exploitation system (or their involvement is not known to staff), that the exploitation system is probably related to the informal hiearchy in the institution, and that some boys are willing to be exploited in some areas but would fight if exploited in others.

The attempt of the authors to distinguish between exploiters and exploited does not provide us with much significant information, except for two factors. (*A*) Exploiters tend to be black and the exploited tend to be white, in what the authors view as a reversal of social roles. The authors suggest that the reason is that blacks are more organized and come from tougher neighborhoods. (*B*) Exploiters tend to have more previous commitments. The possible interaction between these two factors is not discussed in the book. Three psychological tests reveal very few significant differences between exploiters and victims and in fact tend to show that those who mostly exploit others and those who are most frequently victimized had very similar scores on several personality scales. The attempt to tie victimization to the administrative goals of the institution is only partly successful. The reader gets the clear impression that victimization will continue to occur as long as the subculture is generally based on power relations between individual inmates and between races and as long as there are scarcities of goods and services.

The book describes in detail the mechanisms of victimization but offers very few solutions. Though the reader gets the impression that the institution is criticized as a total institution, the authors only offer more invasion of privacy as a solution to victimization (clear view of bathrooms and cells from the outside and similar solutions). Despite its limitations, the book is not without its contributions. It describes a dynamic subculture and a complex system of relations which will help future researchers and provides us with some insights but with very little hard evidence.

The Sociology of the Bay Colony. By Morris Talpalar. New York: Philosophical Library, 1966. Pp. 396. $15.00.

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Anyone attracted to this book because of its title is likely to be disappointed. All readers are apt to be irritated by its overgeneralizations; historians may fret at the careless use of chronology in its organization; sociologists will cringe at the usage of the word "sociology" and will wish the association with their discipline had not been made. Students unschooled in either of these fields will become confused if not misled.

The organization of the work is difficult to grasp. Introductory and background materials take up the first half of the book; in the second half, the space given to the Bay Colony is barely enough to justify the title. In the first 40 pages, the author offers two conceptual themes, to which he occasionally refers later, but neither provides a satisfactory coherence. One theme is the dialectic tension between Hellenism and Hebraism as cultural styles in which institutional forms of religion emerge periodically as resolutions; social change is marked by the recurrence of that tension in ever-

new settings. The second theme involves a triad of land, trade, and labor, which are the respective foundations of the social orders of feudalism, capitalism, and socialism. (The third pair, labor/socialism, is not applied to the Bay Colony, however.) There might have been value in viewing these as two crosscutting themes, each adding depth to the other in an understanding of the Bay Colony, but neither is sufficiently developed for that to happen. Morris Talpalar makes it clear that he sees the Puritans as trader capitalists who struggled against the primeval and against the "natural" tendencies for colonies to regress to a feudal order, and also that by being civilized Calvinists they were caught up in the Hellenism/Hebraism tension, but the reader who is stimulated by such a perspective looks in vain for a convincing or sustained analysis along such lines.

Since Talpalar introduced the concepts, why do they fail? I offer two suggestions: inappropriate writing style and weak scholarship. The book is not difficult to read but seems to have sacrificed clarity of thought on the altar of pedantic display. Writing in intellectual clichés not only dulls the reader's attention (I wish I had a dollar for every "per se" in the book) but also holds a danger for the writer: false generalizations and inconsistencies creep in with the borrowed rhetoric. A sequence of ostentatious phrases may lead to a simply stated but greatly exaggerated conclusion. It is by such a process that we are told "Puritanism . . . dominated the world for several centuries" (p. 180). When a word is chosen for its intellectual appearance its meaning may become open to question. Witness the word "sociology" in the following: ". . . medieval sociology was based on land" (p. 48), or in the statement that one of the two objectives of the Massachusetts Bay Company was "sociology—to give this civil community a certain social form" (p. 217), or in the assertion that "the Reformers' underlying motivation was in sociology" (p. 62).

Several things suggest a weakness in scholarship. Talpalar cites 31 sources for his 132 references, but 76% of the references are from only eight sources, 35% are from only two and one of these is the Bible. These figures refer only to the *cited* sources: there are many quotations for which reference is not given. More important, Talpalar seems to be unaware of scholarly work which would be difficult to overlook. In spite of his emphatic linkage of Puritanism and capitalism, there is not one citation to Max Weber, R. H. Tawney, or the great body of literature associated with those names. His notion of trade as the foundation of the social order "capitalism" leads him to ignore the distinction between mercantilism and capitalism which has been standard in economic history, and further, he seems unaware of there being such a distinction: in this book any activity of trade which involves money is "capitalist." Finally, Talpalar is uninformed about nonindustrialized non-European peoples; when discussing pre-Puritan America he assumes that there could be no "mental" association between the civilized and the aboriginals (his labels). At one point, he speaks of the racist slave-labor system as an "unavoidable" arrangement (p. 172). It is difficult to believe that the section on the relationship between colonial Europeans and the native Americans (pp. 334–50) could

have been written in this decade by someone who presents his work as "sociology," but there it is.

A sociology of the Bay Colony would be interesting but will not be found in this book.

The Sociology of Survival: Social Problems of Growth. By Charles H. Anderson. Homewood, Ill.: Dorsey Press, 1976. Pp. xii+299. $5.95 (paper).

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"Time is running out on the possibilities for a survivalist society. . . . the time is already five minutes past 12" (p. 280). Those are the last lines of this book. The first lines are: "No course on the sociology of survival has, so far as I am aware, yet been introduced in any academic curriculum. Yet it is a subject that needs to be studied if we are to succeed in making our way in today's world" (preface). I agree with that need and have just added *The Sociology of Survival* to the list of basic texts for a three-quarter, interdisciplinary course, "Ethics of Development in a Global Environment."

The task Charles Anderson set for himself was formidable. First, he sought to write a book that would give the reader a summary of present world trends that will have direct bearing on whether human society as we know it can survive into the near future. But the author is not satisfied with merely calling our attention to the state of the world—which, to be sure, he does, admirably. He undertakes an analysis of *why* we are in such a precarious state and, most important, he accepts responsibility for offering his own formula for getting us out of this state and into one which could hold more hope for human survival. "In effect, a sociology of survival is to a large extent both a critique of the present growth system and an inquiry into the nature and prospects of a socially and environmentally rational society" (p. 9).

The survivalist ethic espoused by Anderson has two parts: access to "social quality" (quality of life), and living in harmony with nature (our life-support system). "The capacity to successfully respond to the survival challenges hinges to a large extent upon the form of social organization which people are working with. . . . it is the manner in which social organization interacts with the . . . challenges to survival which shall determine the outcome of the human experiment" (p. 6).

He convincingly examines the "growth society," defined in terms of economic and technological growth, and concludes that there is inherent in such growth a self-destruct mechanism. Regardless of which societies get hooked on continuously expanding growth (he is aware of the need for many of the poorer countries to participate in substantial economic and technological growth) there comes a time when that very process of growth

turns into a monster that destroys both the physical and emotional ba
for a decent quality of life. And he is convinced that the United Sta
has already passed the point where such growth is beneficial.

"A society which can remain permanently stable without econon growth is both possible and necessary to survival. Growth of a differe kind, social and cultural growth, will then be the recipient of human c velopment powers" (p. 23). The author takes pains throughout the bo to make this distinction clear. His is not a "no-growth" ethic. It is grow re-defined in terms of reference to existing human needs such as housi medicine, education, transportation, meaningful work, environmental pr ervation and improvement, and the like, rather than economic growth f its own sake—the sake of profit. "It is growth with a specific hum content and purpose. . . . Social growth is also growth of social equali participatory democracy, cultural productivity, and personal developmen (p. 3).

As the author weaves his web throughout the book, analyzing the d velopment of the U.S. growth society, dissecting the intricate interactio and interdependencies involving man's use of technology to exploit o natural resources to meet our life-style, he manages a convincing job snaring the culprit. His premise is that economic and technological grow as we know it has not only failed to bring happiness and fulfillment to t richest and most powerful society ever to exist on the face of the earth bu in its greed, is destroying the very physical environment that gives it su tenance. What seems to be the driving force behind the growth juggernau "The most fundamental thing to understand about our economy is that is ultimately fueled entirely by profit. . . . Capitalism runs on profit; the can be no substitutes, no escapes" (p. 69). So—the culprit is caught. Cap talism depends on profit and profit depends on economic growth. Econom growth has lost touch with the basic needs of people and is, in fact, destro ing the very source from which any society can draw in order to meet th needs of its people, now and in the future.

Stated very bluntly: ". . . advanced capitalism as a form of social organ zation is incompatible with human viability upon the earth. A new soci order must be created which has the capacity to live in technological an economic harmony with nature" (p. 6).

If one is familiar with the vast literature now devoted to analysis of th global "problematique" he is struck with how it divides itself primaril into three groups. First, there are those whose books call attention to th predicament of humankind and predict various kinds and degrees of disa ter if current trends continue but who, in effect, end their analysis at tha point. Here I would include the following: Robert Heilbroner's *Inquir into the Human Prospect* (New York: Norton, 1975), Donella Meadows *Limits to Growth* (New York: Signet, 1972), Kenneth Watt's *The Titani Effect* (Stanford, Conn: Sinauer, 1974), Paul Ehrlich's *Population Bom* (New York: Ballantine, 1968), and Garret Hardin's *Exploring New Ethic for Survival* (New York: Viking, 1972). Second, there are those who say as Herman Kahn and Buckminster Fuller do, that as soon as we get beyon

the next 25 years everything will be rosy. One example is Kahn's *The Next 200 Years* (New York: Morrow, 1976). The third group includes those who face up to the challenge of the next 25 years and are willing as responsible global citizens to try to suggest alternative options for human survival. Such persons and their works may be divided into three subgroups. First, some try within the confines of their social, political, and economic structures to make the system more moral. Here I would place Kenneth Galbraith's *Economics and the Public Purpose* (New York: Signet, 1975), Lester Brown's *World without Borders* (New York: Vintage, 1973), Richard Barnet and Ronald Muller's *Global Reach* (New York: Simon & Schuster, 1974), Hollis Chenery's *Redistribution with Growth* (Oxford: Oxford University Press, 1974), George Ball's *Global Companies* (Englewood Cliffs, N.J.: Prentice-Hall, 1975), and George Lodge's *New American Ideology* (New York: Knopf, 1975). Second, there are those determined and optimistic souls who design sweeping new international orders, world law, a global ethic, and the like: Mesarovic and Pestel's *Mankind at the Turning Point* (New York: Dutton, 1974); Barbara Ward's *Only One Earth* (Harmondsworth, Middlesex: Penguin, 1972) and *Home of Man* (Toronto: McClelland & Steward, 1976); Lester Brown's *World without Borders* (again), *By Bread Alone* (New York: Praeger, 1974), and *In the Human Interest* (New York: Norton, 1974); The 1975 Dag Hammarskjold Report's *What Now: Another Development* (Uppsala: Dag Hammarskjold Foundation, 1975); and Jan Tinbergen's *RIO—Reshaping the International Order* (New York: Dutton, 1976). Finally, some come to the brutal conclusion that there is no way by which social inequities and environmental destruction can be redressed so long as the driving force in our lives is financial profit. Increasing numbers of analysts are now writing books in this "end growth-for-profit" vein: Barry Commoner's *Poverty of Power* (New York: Knopf, 1976), the editors of the *Ecologist's Blue Print for Survival* (New York: Signet, 1974), E. F. Shumacher's *Small Is Beautiful* (New York: Harper Torchbooks, 1973), E. F. Mishan's *Technology and Growth* (New York: Praeger, 1970), Dennis Goulet's *The Cruel Choice* (New York: Atheneum, 1973), John Gurley's *Challenges to Capitalism* (Stanford, Calif.: Stanford Alumni Assoc., 1975), Herman Daley's *Toward a Steady-State Economy* (San Francisco: Freeman, 1973), Cheryl Payer's *The Debt Trap* (New York: Monthly Review Press, 1974), Robert North's *The World That Could Be* (Stanford, Calif.: Stanford Alumni Assoc., 1976), and André Gunder Frank's *Capitalism and Underdevelopment in Latin America* (New York: Monthly Review Press, 1969).

Once the issue of growth-for-profit is faced amazing things can happen. A vast cloud vanishes, and both the faults and the hopes become more clearly discerned. Certainly Charles Anderson's new book falls here. He helps to unlock the logjam. He suggests that we look critically at how we define the good life, ask ourselves whether the present system is adequate and, if not, why not. If our answer leads us to the need to give up a tried and true cornerstone of our way of life, so be it.

Most of the literature—whether it derives from the "limits to growth"

or the "prospects for survival" school, the utopian "replace the whole system" approach, the "get out the vote" democratic activists, or the technological optimists—ends in relative states of confusion, naivete, blind faith, or abject depression. Why? Because the authors try, consciously and unconsciously, to hold on to parts of the world around them and are not willing to examine those parts objectively in the sunlight to see whether they should, in fact, be retained as parts of attempted solutions. Often what we try to hold on to constitutes an integral part of the problem rather than aids toward a solution. Anderson successfully cuts through all that.

If I must find some fault with the book I would say that it unnecessarily overstates the evils of our present society. No system is that bad. Nobody can deny that vast numbers (even proportionately) of Americans are living better than has been possible in past societies. Today it is the minority that is suffering rather than the majority. It simply is not true that only those who "measure life quality by the size of their investments" (p. 3) have benefited from economic and technological growth. Some readers may find it difficult to swallow the first two pages or other needlessly extreme passages and, deciding that the author is biased, fail to receive the well-reasoned and well-documented arguments that fill the remainder of the book.

Another very critical aspect of the global problematique given inadequate attention is that the majority of people today suffers more from inadequate, unequal, and repressive social, political, and economic systems than from environmental deterioration. Anderson touches on this problem in the chapter entitled "Growth and Underdevelopment" but could have given considerably more strength to his work by placing it squarely and urgently in the present global context.

I am not at all sure that the author's use of such well-worn terms as "capitalist class," "working class," etc., is appropriate to his analysis. Today these classifications, when applied to present conditions, need to be very carefully explained and broken down into their several subcategories. "Growth is a matter of survival to the capitalist class" (p. 31). Who is he referring to in the United States? Business and labor move happily and blindly together arm in arm down the freeways and runways of constant growth. There really is little value in using terms that describe old allegiances and become terribly muddied in the present. It actually obfuscates the otherwise clean case presented by the author.

Possibly the most important new contribution of the book is presented thus: ". . . we should argue that the stable-state society is, in effect, a mature socialist society and, if it is to become a useful concept and effective practice, must be integrated into the general corpus of socialist theory" (p. 61). The book might have been strengthened by pointing to alternative systems now in practice and in transition in Scandinavia, Great Britain, Tanzania, Cuba, and Yugoslavia.

Anderson's logic; his marshaling of descriptive data; his understanding of the intricate fabric of modern society, of the evils of growth, the alternatives to growth, and the relevance of a society oriented to social equity,

individual opportunity, and environmental harmony; his willingness to shake the traditional pillars and to take a stab at defining clearly what he, personally, means by quality living—these all add up to a decidedly positive new contribution to the literature on human survival.

In the last chapter the author summarizes what he believes needs to be done: "Economic growth as an end in itself should be slowed and placed on a course which has largely social and environmental ends in mind. There is no point in being dragged down by the demands of an impersonal economic system which has only its own survival requisites as guidelines. The salvage operations for capitalism are becoming more costly in both human and environmental terms. Its survival is endangering societal survival. The broad course of action is evident here: reconstruct the economic system so as to meet human survival needs and set aside the antiquated assumptions of profit and growth" (p. 270).

The Clouded Vision: The Student Movement in the United States in the 1960s. By David L. Westby. Lewisburg, Pa.: Bucknell University Press, 1976. Pp. 291. $12.00.

Mayday at Yale: A Case Study in Student Radicalism. By John Taft. Boulder, Colo.: Westview Press, 1976. Pp. v+224. $12.50.

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The central theoretical question confronting students of the student movement of the 1960s can be stated simply: what structural conditions generated a movement in opposition to the social system among its most advantaged members, and what is the meaning of this development for social and political change in the advanced societies? Hundreds of studies have systematically documented the privileged social origins of the participants, this being the social movement more intensively scrutinized than any other in the postwar period. Yet neither Marxists, functionalists, nor others have succeeded in organizing the very extensive systematic observations into a coherent theory of the politics of affluence and the affluent. David Westby, one of the early observers of the movement, has now written a new work centrally addressed to theoretical issues, and has presented an account which, while raising more interesting questions than it resolves, is the most stimulating and persuasive theory of the student movement yet advanced.

In *The Clouded Vision* Westby develops a grounded, middle-range theory, moving solidly from the data of the major movement studies to an original, provocative conception of the change dynamics of advanced capitalism. The theory emerges, in the first instance, as an interpretation of the distinctive attributes of student radicals and activists. These were largely

advantaged, upper-middle-class students with, however, three central characteristics differentiating them from other affluent, nonactivist students: (1) a non-Protestant "humanist" family subculture distinguished by a value system stressing autonomy, democratic decision making, intellectualism and critical thought, and liberal or socialist political views; (2) parents rooted occupationally in the professions or intellectual life rather than business; and (3) a nonvocational educational milieu and a social sciences or humanities focus of study—a liberal-arts education relatively uncoordinated with the specific technological demands of the corporate and scientific labor market. Westby's major insight is that these distinctive attributes can be linked theoretically as different manifestations of a shared "peripheral" social location, that is, location at a distance, structurally and ideologically, from the core business institutions of the capitalist system.

Westby's distinction between the core and periphery of the social system, derived in part from the Marxist distinction between the economic base and superstructure, suggests that critical and oppositional consciousness will develop in the institutions and sectors of the population least closely integrated, either structurally or ideologically, with the dominant corporate sector. At the center, inside the large business enterprise, the capitalist spirit is intensively concentrated, all organizational roles are directly subordinated to dominant elites, and sanctions for opposition are immediate and powerful. In noncentral institutions, such as the educational or family systems or the independent professions, noncapitalist or anticapitalist values have, relatively speaking, more fertile soil for cultivation and expression. While powerful arguments can be made, as in the traditional Marxist perspective, that the mainspring of change will emerge from *within* the central sphere, the notion of oppositional forces within the "periphery" is persuasive. It represents an elegant theoretical interpretation of the demography of the student movement in particular. The "humanist" family is on the cultural periphery, a carrier of values outside of and antagonistic to capitalist culture; structurally, the parents of activists are rooted in professions with a measure of historical and institutional independence from the corporate center, and the radical students themselves are following the "peripheral" humanities or social sciences educational and career paths least strongly programmed by or structurally coordinated with central business institutions.

Yet when we return to the key theoretical problem of the revolt of the advantaged we find that the center-periphery theory succeeds only in introducing a new set of fundamental questions. Most important, what are the underlying conditions in advanced societies that lead to the peculiar phenomenon of "peripheralization" of significant sectors of its privileged classes? Any attempt to answer this question requires, in the first place, consideration of the intersection of Westby's theory with a social class analysis, as it becomes immediately apparent that members of any given class, including the dominant classes, can vary widely in terms of the periphery or centrality of their location. Westby has no explicit class theory but implicitly points to the importance of a refined theory of the upper-

middle stratum and the character and dynamics of its central and peripheral sectors. In this connection, the recent discussion by Barbara and John Ehrenreich ("The Professional-Managerial Class," *Radical America*, vol. 11, no. 2 [March-April, 1977]) suggesting that the professional-managerial stratum can be regarded as an emergent class with interests both antagonistic to and convergent with both the working class and the capitalist class has considerable interest. The Ehrenreichs discuss the student movement as an expression of those interests of this class in opposition to the dominant class; Westby's analysis would suggest the importance of distinguishing between corporate managers and other professionals, between institutionally attached and unattached professionals and intellectuals, and among those in the labor force, those in school, and those "dropped out" or unemployed to assess the extent to which the more peripheral sectors act as the carriers of the anticapitalist interests of the emergent class.

The nature of the fundamental forces responsible for the emergence and expansion of the peripheral sectors emerges finally as the central issue underlying Westby's thesis. He argues that capitalist "disaccumulation"—a notion developed by Martin Sklar to identify the stage in the capitalist economy at which goods production can continue to increase with a corresponding decline of employment in the manufacturing sector—is the key to understanding peripheralization of students and other advantaged groups, as well as of goods-producing workers. He interprets disaccumulation as a slowly intensifying crisis of employment in all segments of the labor force, manifested most acutely among the young. The reformist response to this problem is to reduce the labor force, first, by an expansion of the higher-education system which keeps young workers out of the labor market for longer periods and also satisfies the status aspirations of broad sectors of the population. Such solutions, however, serve to concentrate greater numbers outside the central corporate sphere, expanding the pe-iphery structure and base of those, such as the student radicals, whose dentification with and attachment to the center is weakest, as well as to ntensify the employment problems of the expanded population of college-ducated workers.

Westby's analysis dovetails here with the ideas of André Gorz, Richard 'lacks, Ivar Berg, James O'Toole, and others who have been concerned ith emergent contradictions between the educational and occupational stems. A developing thesis is that the two systems are in interdependent sequilibrium, with education expanding in a contracting employment arket and with values, skills, and aspirations imbued in the educational ocess dissonant in critical respects with the nature of work and the job ructure of the economy. Most telling are statistics cited by O'Toole ("The serve Army of the Underemployed," *Change* [May 1975]) indicating, example, that while 54% of all high school seniors and 64% of com-nity college students seek professional or managerial-level jobs, only % of the labor force will be in this category by 1985; moreover, one-half all current jobs do not require even a high school education. O'Toole

projects conservatively that between 1972 and 1985 almost 5 million educated young workers will be unable to find jobs commensurate with their skills. Massive underemployment and unemployment can be regarded as the contemporary dimensions of peripheralization among college-educated youth, an inexorable consequence not only of the disaccumulation process and the accompanying expansion of education, but also of the systematic degradation of work and the organized expropriation of job skills masterfully described by Harry Braverman. In the light of Westby's analysis, these processes may be the most important potential source of new movements among advantaged youth in the next decade.

Westby himself offers no scenarios of the future, indicating only that it is an open question whether the private discontents of a growing number of affluent young in peripheral educational or work locations will be again mobilized in a progressive social movement. A prognosis must depend partly on an analysis of the reasons for the decline of the student movement, a matter unfortunately given relatively scant attention in Westby's study. It is here that John Taft's book, *Mayday at Yale*, a vivid chronology of the events at Yale in the spring of 1970 during the trial of Black Panther leader Bobby Seale, appears most suggestive. Though eschewing any theory, Taft has researched an almost day-by-day narrative of the volatile period leading to Mayday Weekend, the massive nationally publicized demonstration hosted by the university itself. The picture that emerges is of a movement so disorganized as barely to merit the term; a motley collection of radical students and sympathetic faculty, idealistic but confused and guilt ridden, who never gain control over the explosive rush of events; of privileged white leftists who are an easy prey for the manipulative tactics of extremist blacks; and of a mass of apathetic liberal students episodically mobilized by the appeals of charismatic leaders and theatrical events but remaining fundamentally uninvolved. Taft's characterization is one of a largely irrational movement, with a susceptibility to violence, lacking any coherent program or organizational structure, and having no sustained base in the student community. In other words, a movement, like an ephemeral flame in the wind, that would be extinguished by natural processes even before establishing itself.

Taft's account, while touching on weaknesses that undoubtedly characterized the student movement on many campuses, manifests all the pitfalls of any case study which attempts to portray a movement through the prism of one event. Indeed, his portrayal, which conforms unwaveringly to the characterization of irrational mass movements in pluralist theory and writing, is inaccurate and misrepresentative of the student movement as a whole. This is demonstrated incisively in Westby's work, which marshals impressive data to show that violence occurred in less than 1% of campus confrontations during the sixties and that protest normally evolved through a sequence of petition and legitimate institutional recourse and exploded into civil disobedience or other illegal activity only after sustained periods of administrative intransigence and the exhaustion of alternative channels of action. Westby rightfully points as well to the dubiousness and ideology-

laden character of assessments of "rationality" and "irrationality" of spontaneous, mass political action in periods of crisis: what appears irrational in the context of bureaucratic practice and daily ordered conduct may be rational when the structure of the established order is systematically committed to mayhem, as in Vietnam, in the name of rationality itself.

This is not to deny that anti-intellectualism, organizational fragmentation, and, most important, the inability to expand beyond a relatively limited privileged student base into larger political communities were features of the student movement and played a major role in its decline. Indeed, the very essence of any democratic, radical movement of the advantaged is that it is a minority movement which must progressively transform and subordinate itself into a movement of the relatively disadvantaged majority, not only to survive, but to maintain its ideological integrity and ultimately legitimate its own existence. The failure of the student movement of the sixties in this respect is, by all odds, its most important one and has been adequately understood neither by movement participants nor by sociologists or other observers. In the wake of Westby's contribution, this remains the major unresolved issue concerning the student movement and deserves more systematic analysis than it has received in any quarter.